

Ethnic Representation and Political Participation

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Abstract

Does winning a legislative seat for one's ethnic group change how citizens engage with politics and judge democracy? Research on ethnic favoritism documents the material returns to co-ethnic power, but not its consequences for the attitudes and participation of the represented citizens. We study first-past-the-post races in Kenya, Malawi, Nigeria, and Uganda in which the winner and the runner-up belong to different ethnic groups. A regression discontinuity design compares members of the narrowly winning and narrowly losing groups. On average, winning the seat has no detectable effect on perceived responsiveness, institutional trust, civic participation, or evaluations of democracy. Groups that newly enter representation report more trust in political institutions but participate less. Descriptive representation alone does little to change how ethnic groups relate to democratic institutions.

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1 Introduction

Across much of sub-Saharan Africa, parties draw their support along ethnic lines, and political office benefits the officeholder’s ethnic group. Groups with a co-ethnic representative in power see more economic activity, receive more roads, schools, and jobs, and raise children who are more likely to survive infancy (Burgess et al. 2015; Franck and Rainer 2012; Kramon and Posner 2016; De Luca et al. 2018; Beiser-McGrath et al. 2021; Amodio et al. 2024). Much less is known about what representation does to the represented themselves: whether they come to trust political institutions more, whether they take part in political life more or less, and how they judge the democracy that produced their representative. These attitudes matter for the legitimacy of democratic institutions, particularly in a region where support for democracy is widespread but satisfaction with how it works is not (Bratton and Mattes 2001; Mattes and Bratton 2007). In this paper, we ask how winning a legislative seat for one’s own ethnic group changes citizens’ political engagement and their views of democracy.

Ex ante, the direction of the effect is unclear. Representation may empower. A win signals that the political system responds to the group, which can raise trust in institutions and the perceived returns to participation, in line with the role-model and empowerment effects documented for women’s representation (Beaman et al. 2012; Bhalotra et al. 2018). Representation may also breed complacency. When a co-ethnic legislator holds the seat, monitoring the representative and pressing collective demands may seem less necessary, and citizens may delegate rather than participate (Besley and Coate 1997; Osborne and Slivinski 1996; Padró i Miquel 2007). Finally, a seat may raise expectations that a single legislator cannot meet, so that evaluations of democracy fall rather than rise. These channels do not necessarily move attitudes and behavior in the same direction.

Estimating the effect of representation is difficult because the groups that win seats may differ from those that lose them in ways that also shape their engagement with politics. A credible comparison requires groups that differ in representation but not in the reasons they won. Where two ethnic groups compete for first place, however, the election decides which of them is represented. These are the races in which descriptive representation is genuinely at stake, and when they are close, the winning group is as good as randomly assigned.

We study parliamentary elections in Kenya, Malawi, Nigeria, and Uganda, all of which use first-past-the-post voting in single-member constituencies. We focus on races in which the winner and the runner-up belong to different ethnic groups and compare members of the winner’s group with members of the runner-up’s group. We exploit close races and a regression discontinuity design to identify the effect of having a co-ethnic rather than a rival-group representative.

We combine constituency-level results from 14 lower-chamber elections with the ethnicity of each candidate, which we infer from candidates’ names and validate against hand-coded labels. We link these races to geocoded Afrobarometer surveys and describe the local ethnic composition of each constituency with Demographic and Health Surveys (DHS). The analysis sample contains more than 3,200 respondents in 236 races. We examine perceived responsiveness of elected officials, trust in political institutions, civic participation, evaluations of and preference for democracy, perceived

unfair treatment of one’s group, national identity, and the intention to vote.

We first document that such mixed ethnicity races are the exception. In about seven in ten legislative races in the four countries, every candidate on the ballot belongs to the same ethnic group, so the election does not decide which group is represented. In only about one race out of eight do the winner and runner-up belong to different ethnic groups. Part of this pattern reflects geography: ethnic groups tend to be concentrated in particular places, and many constituencies are dominated by a single group. Geography therefore explains part of the pattern, but not all of it. Candidate slates are less ethnically diverse than the constituencies they come from. If candidates were drawn at random from the local population, about half of all races would be mixed. For most citizens, descriptive representation is therefore settled before any votes are cast.

Our main finding is that winning representation does not measurably change how citizens engage with politics or how they evaluate democracy. The design can detect effects of about two thirds of a standard deviation (about half a step in the response scale) with conventional power.¹ The estimates therefore make large effects of representation unlikely, whether in the direction of empowerment or of complacency, but they cannot rule out modest ones. Following Abadie (2020), we view this finding as informative in its own right: it narrows the range of effects that descriptive representation can plausibly have on the political engagement of the represented.

The effect of winning differ by whether the ethnic group already held the seat. Among groups that did not hold the seat after the previous election, narrow winners report less civic participation and a lower intention to vote than narrow losers, and more trust in political institutions. Instead, among groups that held the seat, narrow winners report higher intention to vote than narrow losers. For a newly represented groups, the participation results are consistent with delegation, in which its members trust political institutions more and leave political action to their new representative.

The effects of representation also change over the electoral term. In the first Afrobarometer round after the election, members of the winning group are less likely than members of the losing group to report that their group is treated unfairly by the government. In later rounds, the estimates on trust in political institutions, preference for democracy, and national identity are lower than in the first round, while the effect on voting intention is higher.

Taken together, the results caution against expecting descriptive representation by itself to substantially change political participation or how ethnic groups relate to democratic institutions. In the races where two ethnic groups compete for a seat, we can rule out large average effects of winning on the represented group’s engagement and democratic attitudes. Behind these averages, the estimates for participation differ between groups that newly gain representation and groups that retain it.

Contributions to the literature. Our paper contributes to two strands of research. First, we provide causal evidence on ethnic representation in multiethnic African democracies, where ethnic-

1. Studies of related forms of representation that use the same survey items, such as a randomly assigned female councillor or a co-ethnic president, find effects of a tenth to a quarter of a standard deviation (Clayton 2015; Ahlerup and Isaksson 2015). A seat in parliament does not produce changes much larger than these.

ity structures electoral competition directly. Prior work on descriptive representation comes mostly from established democracies and concerns candidate supply or participation. Studies of women’s representation document effects on the entry and success of other female candidates, alongside mixed evidence on broader political participation (Broockman 2014; Gilardi 2015; Baskaran and Hessami 2018; Bhalotra et al. 2018; Bagues and Campa 2021; Dörr et al. 2025). Related work studies the election of religious and racial or ethnic minorities (Meyersson 2014; Beach et al. 2024). Our design adapts the close-election comparisons used for mixed-gender and mixed-religion races to races between candidates of different ethnic groups. We also extend the outcomes to a broad set of political attitudes.

Second, we contribute to the literature on ethnic politics and favoritism by studying the political consequences of having a co-ethnic in power. This literature has focused mainly on the material returns. The ethnic homelands of national leaders see more economic activity while their co-ethnic is in office (De Luca et al. 2018), and co-ethnics of those in power benefit from more roads (Burgess et al. 2015), more education (Franck and Rainer 2012; Kramon and Posner 2016), and lower infant mortality (Franck and Rainer 2012; Beiser-McGrath et al. 2021). Recent evidence extends these returns to legislative representation, showing employment gains where a group’s ethnic party holds the parliamentary seat (Amodio et al. 2024). Less is known about how access to power shapes citizens’ political attitudes. In earlier work, we show that whether a group has access to state power conditions how its members’ support for democracy responds to violent conflict (Stoelinga and Tähtinen 2026). Whatever the material returns to a seat, we show that, on average, winning does not substantially change how the represented group engages with politics.

The rest of the paper is organized as follows. Section 2 describes the setting and the data. Section 3 presents the empirical strategy. Section 4 reports the results, first for all groups and then by prior representation and by time since the election. Section 5 concludes.

2 Setting and data

2.1 Setting

We study legislative elections in Kenya, Malawi, Nigeria, and Uganda. All four countries elect the lower chamber of parliament by first-past-the-post voting in single-member constituencies. Each race returns exactly one representative, and the candidate with the most votes takes the seat. Whether an ethnic group is represented in a constituency is therefore decided by a single, sharp threshold in the vote. Under proportional representation or in multi-member districts, by contrast, a group’s representation depends on seat allocation across many candidates and parties, and no such threshold exists.² In all four countries, ethnicity is a central cleavage in electoral politics, and parties and candidates often draw their support from particular ethnic groups. We cover 14 elections, listed in Table 1: four in Kenya (2002 to 2017), four in Malawi (2004 to 2019), three in

2. In Uganda, parliament also includes district woman representatives and seats reserved for special interest groups. We use the constituency races only.

Nigeria (2011 to 2019), and three in Uganda (2006 to 2016). These are the elections for which we observe candidate names and can link survey respondents to the constituency in which they live.

Table 1: Elections, surveys, and data sources

Country	Election	Afrobarometer rounds	Ethnic composition source
Kenya	2002	2, 3	2003, 2008–09, 2014, 2015 MIS, 2022
	2007	4, 5	
	2013	6, 7	
	2017	8, 9	
Malawi	2004	3, 4	2000, 2004–05, 2010, 2012 MIS, 2014 MIS, 2015–16, 2017 MIS, 2024
	2009	5, 6	
	2014	7	
	2019	8, 9	
Nigeria	2011	5, 6	2008, 2010 MIS, 2013, 2015 MIS, 2018, 2021 MIS, 2023–24
	2015	7	
	2019	8, 9	
Uganda	2006	4	Population censuses 2002, 2014 (IPUMS)
	2011	5, 6	
	2016	7, 8	

Notes: Lower-chamber elections in single-member constituencies with results from the Constituency-Level Elections Archive (CLEA). Each Afrobarometer round is matched to the most recent legislative election before its fieldwork; the first round listed for an election is the first round conducted after it. Afrobarometer Round 2 has no ethnicity question; in Kenya, we proxy ethnicity in that round with the respondent’s home language. For Kenya, Malawi, and Nigeria, the ethnic composition of constituencies comes from the Demographic and Health Surveys (DHS), including Malaria Indicator Surveys (MIS) where these record ethnicity and cluster locations; shares are pooled across the surveys listed. For Uganda, where only one DHS survey records both ethnicity and cluster locations, it comes from the 10 percent samples of the 2002 and 2014 population censuses distributed by IPUMS International.

2.2 Election results and candidate ethnicity

Constituency-level results come from the Constituency-Level Elections Archive (CLEA), which records every candidate’s name, party, and vote share. CLEA does not record candidates’ ethnicity. We infer it from each candidate’s name, using a large language model that assigns every candidate to one group from a closed, country-specific list of ethnic groups. The list is built from the ethnic categories that Afrobarometer records for each country, so that candidate and respondent ethnicity are measured in the same categories.³ The model also receives the constituency and region, since the same name can be common in more than one group and location often decides between them. Where we have hand-coded labels from documentary sources, these replace the model’s prediction; this is the case for 282 candidates. Against a randomly drawn set of hand-coded Nigerian candidates, the model assigns the correct group to 89 percent of candidates, with similar agreement in close races. Appendix A documents the procedure and its validation.

3. Candidates, survey respondents, and the DHS data described below share a single set of group labels. Where names cannot distinguish between groups, we merge them: we treat Hausa and Fulani in Nigeria as one group, as well as Borana, Gabra, and Orma in Kenya.

2.3 Survey data

Our outcomes come from geocoded Afrobarometer surveys, which interview nationally representative samples of adults face to face. We take respondents’ ethnicity from the survey’s ethnicity question.⁴ We place each respondent in a constituency using the coordinates of the enumeration area and constituency boundaries for the relevant election.⁵ We match each survey round to the most recent legislative election held before its fieldwork (Table 1). Most elections are followed by two rounds, which we use in Section 4.2 to compare responses over the election term.⁶

2.4 Local ethnic composition

To describe the ethnic composition of each constituency, we use the Demographic and Health Surveys (DHS) in Kenya, Malawi, and Nigeria, and the population census in Uganda. We place DHS survey clusters in constituencies by their coordinates and compute each group’s share of respondents in the constituency, pooling all surveys that record ethnicity (Table 1). In Uganda, only one DHS survey records both ethnicity and cluster locations, so we use the 2002 and 2014 censuses instead, which record the ethnicity of every enumerated person and locate households in sub-counties. Because these sources are large and independent of Afrobarometer, the shares do not inherit sampling noise from the survey that supplies our outcomes. We use them to describe the groups that compete and to test the validity of the design, not to define treatment. Appendix B describes the construction of the geographic links, the survey matching, and the local shares in detail.

2.5 Ethnic competition on the ballot

We first document how often ethnic groups compete for a seat at all. Table 2 reports, for the 3,794 single-seat races with at least two candidates in the 14 elections, the share of races in which candidates of at least two ethnic groups stand. In about seven in ten races, all candidates we assign to a group belong to the same group. The share is similar across the four countries and across elections. Because some candidates cannot be assigned to an ethnic group, the share may understate how often groups compete; if each unassigned candidate belonged to a further ethnic

4. Afrobarometer’s ethnicity codes differ across rounds, so we assign groups through a round-specific crosswalk. Round 2 does not ask about ethnicity; for Kenya, we use the respondent’s home language instead, which agrees with self-reported ethnicity for 95 percent of Kenyan respondents in the rounds that ask both questions.

5. For Kenya and Uganda, we use the harmonized constituency maps of Amodio et al. (2024), which we map back to the pre-2010 constituencies for the Kenyan elections of 2002 and 2007. For Malawi, we use a constituency shapefile. For Nigeria, we aggregate local government areas into federal constituencies separately for each election, since constituency identifiers change between elections. In Uganda, the available boundaries are coarser than constituencies, so a respondent in an area covering several constituencies is assigned to one of them: the boundary file of Amodio et al. (2024) has fewer areas than there are constituencies, and their correspondence links each area to one constituency by name. Where an area contains several constituencies, for example a county that was later split into a northern and a southern constituency, or a municipality that forms its own constituency, the link covers only one of them, and we assign all respondents in the area to that constituency. This concerns 86 of the 728 Ugandan respondents in the analysis sample.

6. On average, the first round after an election was fielded 1.5 years after it, ranging from about two months to three years across elections; later rounds were fielded on average 3.8 years after the election, between 2.3 and 4.8 years.

group, about six in ten races would still be uncontested. Moreover, where several groups appear on the ballot, the top two candidates are rarely from different groups. The winner and the runner-up belong to different groups in only about one race in eight, and only half of these races are close.

One explanation is that ethnic groups live apart, and that most constituencies are dominated by one group. To assess how much of the pattern this explains, we compare each candidate slate with a benchmark in which the same number of candidates is drawn at random from the constituency’s population, using the local ethnic shares described above. We restrict this comparison to the races in which every candidate is assigned to a group. Under random candidacy, about half of these races would be mixed, twice the observed share, and the average ballot would carry close to two ethnic groups rather than fewer than one and a half. Figure 1 shows that the gap depends on local diversity. In the most homogeneous fifth of constituencies, the observed share of mixed races matches the random draw benchmark. The gap widens as constituencies become more diverse, and in the most diverse fifth, where almost every race would be mixed under random candidacy, only about half are. About half of all uncontested races take place in constituencies where one group makes up at least 90 percent of the population, so that local composition alone predicts an uncontested race. The other half take place in more diverse constituencies, where the ballot is less diverse than the electorate. Who represents a group is therefore shaped not only by where groups live but also by who stands for office.

Table 2: Ethnic competition on the ballot

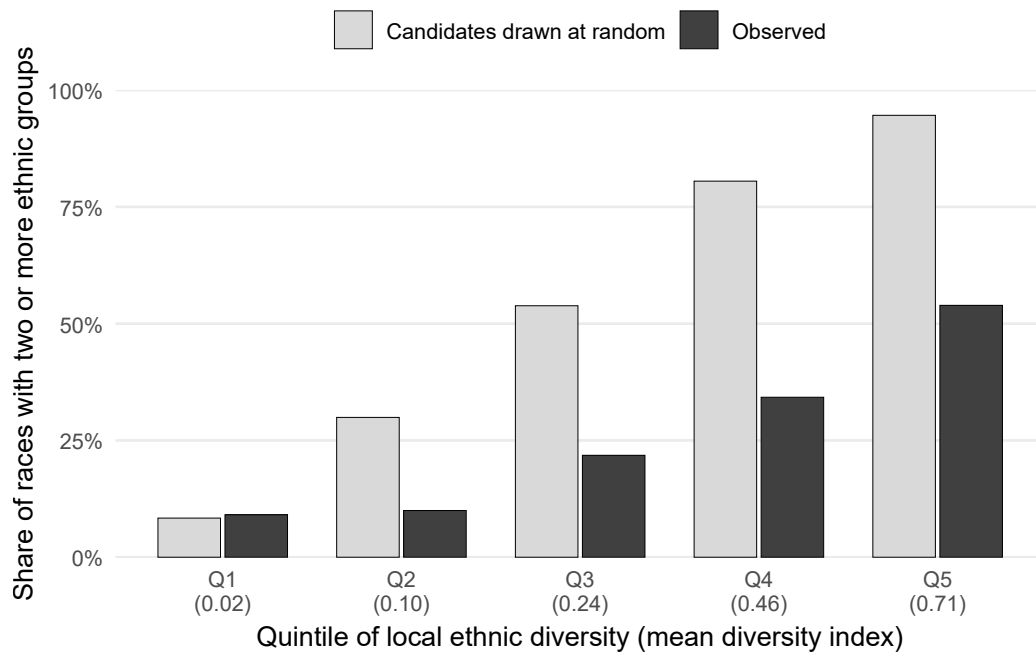
Country	Races	All races				Fully assigned races				
		Mixed (%)		Top two differ (%)		Mixed (%)			Groups on ballot	
		Assigned	Bound	All	Close	Races	Obs.	Random	Obs.	Random
Kenya	1,041	30	34	13	7	891	26	49	1.42	1.87
Malawi	759	33	58	13	7	418	28	70	1.39	2.23
Nigeria	1,051	28	35	12	5	869	24	48	1.31	1.89
Uganda	943	26	34	15	7	570	26	56	1.30	1.89
Total	3,794	29	39	13	7	2,748	26	53	1.35	1.94

Notes: All races: single-seat races with at least two candidates in the 14 elections (Table 3). Mixed, assigned: at least two ethnic groups among the candidates the model assigns to a group. Mixed, bound: upper bound that counts each unassigned candidate as a further group. Top two differ: the winner and the runner-up are assigned to different groups and the candidate with the most votes holds the seat; close: the vote-share gap between them is below 20 percentage points. Fully assigned races: races in which every candidate is assigned to a group and the constituency’s ethnic composition is observed (DHS in Kenya, Malawi, and Nigeria; population census in Uganda). Random: the expected share of mixed races and the expected number of groups on the ballot if the same number of candidates were drawn at random from the constituency’s population.

2.6 Sample

Table 3 shows how the sample is built. Of the 3,794 single-seat races with at least two candidates in the 14 elections, 29 percent are ethnically contested, in the sense that at least two ethnic groups appear among the candidates (Section 2.5). We keep races in which the winner and the runner-up are both assigned an ethnicity, belong to different groups, and the candidate with the most votes

Figure 1: Ethnically mixed races: observed and under random candidacy



Notes: Races in which every candidate is assigned to an ethnic group and the constituency's ethnic composition is observed, grouped into quintiles of local ethnic diversity (one minus the sum of squared group shares; quintile means in parentheses). Observed: share of races with candidates of at least two groups. Candidates drawn at random: expected share if the same number of candidates were drawn at random from the constituency's population. Local shares come from the DHS in Kenya, Malawi, and Nigeria and from the population census in Uganda.

holds the seat. This leaves 496 races, 13 percent of the total, of which about half were decided by fewer than 20 percentage points. Within these races, we keep Afrobarometer respondents who belong to the group of the winner or of the runner-up. Members of other groups do not enter the analysis, because their representation does not change with the outcome of the race.

Table 3: Sample: from legislative races to the top-two ethnic-pair design

Country	Elec.	Races				Analysis sample		
		All	Contested	Top two differ	of which close	Races	Cells	Respondents
Kenya	4	1,041	311 (30%)	136 (13%)	77	90	140	1,205
Malawi	4	759	251 (33%)	97 (13%)	55	41	62	762
Nigeria	3	1,051	291 (28%)	126 (12%)	50	56	74	548
Uganda	3	943	243 (26%)	137 (15%)	70	49	64	728
Total	14	3,794	1,096 (29%)	496 (13%)	252	236	340	3,243

Elections with matched Afrobarometer rounds. All: single-seat races with at least two candidates. Contested: at least two ethnic groups among the labelled candidates. Top two differ: winner and runner-up both labelled, from different ethnic groups, vote leader holds the seat (the eligible races). Of which close: eligible races in which the vote-share gap between winner and runner-up is below 20 points. Analysis sample: eligible races, close or not, where Afrobarometer sampled members of the winner’s or runner-up’s group, so its races are a subset of the eligible races rather than of the close ones; the estimator downweights races by their distance from the cutoff. Cells are group $\times$ race (at most two per race).

2.7 Outcomes

We study nine outcomes that capture how citizens relate to their representatives, to political institutions, and to democracy. Three are indices. *Responsiveness* combines two questions on how often elected officials “try their best to listen to what people like you have to say,” asked about members of parliament and about local government councillors. *Trust in political institutions* combines trust in parliament, in the local government council, and in the ruling party. *Civic attendance* combines whether the respondent attended a community meeting and whether they got together with others to raise an issue. The remaining six outcomes are single items. Three concern democracy: how much of a democracy the country is (*extent of democracy*), *satisfaction with democracy*, and *preference for democracy* over other kinds of government. The other three are how often the respondent’s ethnic group is *treated unfairly* by the government, whether the respondent *identifies as national* rather than ethnic, and whether the respondent *would vote* if an election were held tomorrow.

The grouping into indices follows a factor analysis of the Afrobarometer items in the four countries (Appendix C). To construct each index, we standardize its items within country, average them, and standardize the average by the mean and standard deviation of members of the runner-up’s group. We express single items in standard deviations of the same group, so that every estimate is in comparable units.

Table 4 reports descriptive statistics for the analysis sample. Members of the winner’s and the runner-up’s groups are similar on all outcomes and covariates when pooled across races.

Table 4: Descriptive statistics

	N	Mean	SD	Mean by group	
				Winner's	Runner-up's
<i>Responsiveness</i>					
MP tries to listen (0–3)	2,984	0.66	0.83	0.63	0.70
Councillor tries to listen (0–3)	2,607	0.85	0.89	0.82	0.88
<i>Trust: government side</i>					
Trust parliament (0–3)	3,155	1.46	1.04	1.42	1.49
Trust local government (0–3)	2,940	1.37	1.03	1.33	1.41
Trust ruling party (0–3)	3,147	1.46	1.10	1.47	1.45
<i>Civic attendance</i>					
Join others to raise an issue (0–4)	3,230	2.17	1.30	2.18	2.16
Attend community meeting (0–4)	3,229	2.29	1.26	2.25	2.33
<i>Single items</i>					
Extent of democracy (1–4)	2,979	2.60	0.85	2.59	2.60
Satisfaction with democracy (0–4)	3,020	2.42	0.97	2.41	2.43
Preference for democracy (1–3)	3,032	2.64	0.67	2.65	2.63
Own group treated unfairly (0–3)	3,061	0.94	1.02	0.95	0.93
Identify as national (1–5)	3,030	3.46	1.15	3.47	3.44
Would vote (0/1)	2,538	0.92	0.27	0.92	0.93
<i>Covariates</i>					
Age (years)	3,223	34.67	13.08	34.61	34.73
Female (0/1)	3,243	0.44	0.50	0.45	0.44
Education (0–9)	3,236	3.48	1.98	3.46	3.52
Urban (0/1)	3,243	0.29	0.45	0.28	0.30
Years since the election	3,243	2.62	1.36	2.64	2.59
<i>Races</i>					
Vote-share gap, winner minus runner-up (pp)	236	25.91	22.92		
Candidates in the race	236	7.53	4.52		
Local share of the winner's group (DHS)	229	0.30	0.31		
Local share of the runner-up's group (DHS)	229	0.30	0.30		

Notes: Analysis sample of the top-two ethnic-pair design: Afrobarometer respondents who belong to the ethnic group of the winner or of the runner-up in 236 races in Kenya, Malawi, Nigeria and Uganda (3,243 respondents). Outcomes are shown on their original scales; in the analysis, items are expressed in standard deviations of the runner-up group's members and indices are standardized on the same group. Race-level variables are computed over the 236 races; local shares are constituency ethnic shares from pooled DHS surveys and are missing where the DHS does not cover the constituency or cannot separate the two groups. The winner's-group and runner-up's-group means pool all races and are not estimates of the effect at the cutoff.

3 Empirical strategy

3.1 Design

We want to know how having a co-ethnic legislator affects the political participation and attitudes of an ethnic group. The ethnic group of a winning candidate is not random. Electing a legislator of a given ethnicity may reflect the constituency’s ethnic composition, the group’s mobilization, and strength of the parties its members support. These factors can also shape the group’s engagement and attitudes directly. Political participation may also affect representation: groups that turn out at higher rates are more likely to elect their own candidates.

We compare members of ethnic groups whose candidates narrowly won or lost a seat. Our empirical strategy uses a regression discontinuity design with close mixed-ethnicity races. Consider a race in constituency j at time t , in which the winner and the runner-up belong to different ethnic groups. Define the running variable m_{gjt} as the vote-share margin of victory of group g ’s candidate against the other leading candidate. The treatment indicator equals one if the candidate representing the ethnic group won: $D_{gjt} = \mathbf{1}\{m_{gjt} > 0\}$.

We estimate the regression discontinuity at the group $\times$ election level, using the the unweighted mean outcome of the group’s respondents in the race.⁷ Our specification is

$$Y_{gjt} = \alpha + \tau D_{gjt} + \beta_1 m_{gjt} + \beta_2 D_{gjt} m_{gjt} + X'_{gjt} \gamma + \delta_{ct} + \varepsilon_{gjt}, \quad |m_{gjt}| \leq h, \quad (1)$$

where Y_{gjt} is the mean outcome among members of group g in constituency j after election t . The covariates X_{gjt} are the cell means of age, gender, education, urban residence, and years between the election and the interview, and δ_{ct} are country-by-election fixed effects.⁸ The slope of the running variable is allowed to differ on each side of the cutoff.

We estimate equation (1) by local linear regression with a triangular kernel and weight each cell by its number of respondents. For each outcome, we select the bandwidth h that minimizes the mean squared error of the estimate (Calonico et al. 2014). We report the conventional point estimate together with robust bias-corrected standard errors and p -values (Calonico et al. 2014; Calonico et al. 2019); the figures show the corresponding 95 percent confidence intervals. Standard errors are clustered by race, which accounts for the two cells of a race sharing the same election.

The coefficient τ identifies $\mathbb{E}[Y_{gjt}(1) - Y_{gjt}(0) \mid x_{gjt} = 0]$, where $Y_{gjt}(1)$ is the outcome for group g in j, t when the group’s candidate wins the seat and $Y_{gjt}(0)$ the outcome when the candidate of the other leading group wins. Two features of this estimand matter for interpretation. First, the treatment is the victory of the group’s candidate, which bundles the candidate’s ethnicity with the party, government alignment, and other attributes that distinguish the two candidates (Marshall

7. All respondents of a group in a race share the same value of the running variable. Estimating on individual respondents would therefore create mass points in the running variable, which complicates bandwidth selection and inference. Aggregating to cells avoids this and loses no identifying variation, since treatment varies only across cells. We do not apply Afrobarometer’s sampling weights within cells, since they are designed to make national samples representative, not group-by-constituency cells.

8. Within each estimation window, we drop fixed effects that are constant or collinear with the other regressors among the observations that receive positive weight.

2024). Second, the estimand is local to races in which candidates of two different groups finish nearly tied, and it averages over respondents interviewed between one and five years after the election. Because cells are weighted by their number of respondents, groups and races with more respondents receive more weight.

3.2 Validity

Identification requires that the outcomes of the two groups would evolve smoothly across the cutoff in the absence of treatment. In close races, whether a group’s candidate finishes just ahead or just behind the rival should then be as good as random. Two implications of this assumption can be tested: groups should not sort across the cutoff, and characteristics determined before the election should be balanced.

Figure D.1 in Appendix D shows the distribution of the running variable. The density of group cells is smooth at the cutoff, and a formal test does not reject continuity ($p = 0.30$; Cattaneo et al. 2020). Table 5 reports the balance of predetermined characteristics. None of the respondent characteristics shows a discontinuity. Groups that held the seat at the previous election are not more likely to win close races, so incumbent groups do not appear to secure narrow victories. The same holds for the candidates themselves: narrowly winning and narrowly losing candidates are equally likely to be women, to be the sitting member of parliament, and to belong to the party of the president in office at the time of the election.

Because each race contributes one cell on each side of the cutoff whenever both groups are surveyed, the cell-level density is partly symmetric by construction. We therefore also test for sorting at the race level, orienting each race by the group that is locally larger according to the DHS or the census. This test does not reject, either with the data-driven bandwidth or at fixed bandwidths of up to a 20-point vote-share gap, the windows in which every bandwidth selected for the main outcomes lies. The balance estimate for the group’s own local population share is negative and significant at the 10 percent level: among close races, the winning group tends to be the locally somewhat smaller one (Table 5). Whether the group is the locally largest one is balanced. The local share never enters the definition of treatment. Adding it and the largest-group indicator to the covariates changes the estimates little, with one exception discussed in Section 4.3 (Table 9, column 6).

4 Results

4.1 The effect of co-ethnic representation

Winning representation does not measurably change how members of the ethnic group engage with politics or evaluate democracy. Table 6 presents the main estimates, and Figure 2 plots them with their confidence intervals. Each row reports the discontinuity at the cutoff for one outcome: the difference between members of a group whose candidate narrowly won the seat and members of a group whose candidate narrowly lost it. All outcomes except the intention to vote are expressed in

Table 5: Validity of the design

	Estimate	SE
<i>Prior representation</i>		
Group held the seat at the previous election	−0.044	0.107
<i>Balance of predetermined characteristics</i>		
Age	−1.519	2.753
Female	−0.015	0.057
Education	−0.148	0.391
Urban	0.132	0.202
Years since the election	−0.092	0.359
Own group’s local share (DHS or census)	−0.158*	0.092
Own group is the locally largest group (DHS or census)	−0.223	0.182
<i>Characteristics of the group’s candidate</i>		
Woman	0.042	0.052
Sitting member of parliament	−0.074	0.120
From the party of the president in office	0.220	0.286

Notes: Local-linear regression discontinuity estimates of each characteristic at the cutoff, with the estimator used for the outcomes. Respondent characteristics are weighted by the number of respondents in the cell; local-composition and candidate characteristics weight cells equally. Prior representation is observed for 255 of the 340 cells. Candidate characteristics refer to the candidate of the cell’s own group. Gender is coded from the candidate’s given name (observed for 329 cells). Sitting member of parliament: the candidate’s name matches that of the seat holder after the previous election in the geographically linked constituency (observed for 260 cells; missing for the first election of each country’s series). Party of the president: the candidate’s party is the party of the head of state on election day (observed for 339 cells). Standard errors are robust bias-corrected and clustered by race. Density tests (Cattaneo et al. 2020), reported in the text, do not reject continuity of the running variable: $p = 0.301$ for the observed group cells and $p = 0.158$ for one observation per race, with the running variable oriented by the group with the larger local population share (DHS, or census in Uganda); at fixed bandwidths of up to 0.20, the race-level test gives p -values between 0.47 and 0.95. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

standard deviations of the runner-up’s group, so a coefficient of 0.1 means that winning the seat moves the outcome by a tenth of a standard deviation. The intention to vote is binary, and we report its estimates in percentage points.

None of the nine estimates is statistically significant. Members of a group that narrowly wins the seat do not perceive representatives as significantly more responsive, and they do not report more trust in political institutions. They take part in community meetings and political action as often as members of the group that narrowly lost, and they are as likely to say that they would vote. They do not differ significantly in their evaluations of democracy, their preference for democracy over other forms of government, their sense that their group is treated unfairly, or their national identity.

Most point estimates are negative, but they are small relative to their standard errors. In particular, the estimates offer no support for the empowerment mechanism, under which a narrow win should raise trust and perceived responsiveness. Nor do they support a strong complacency mechanism, under which winning should lower participation.

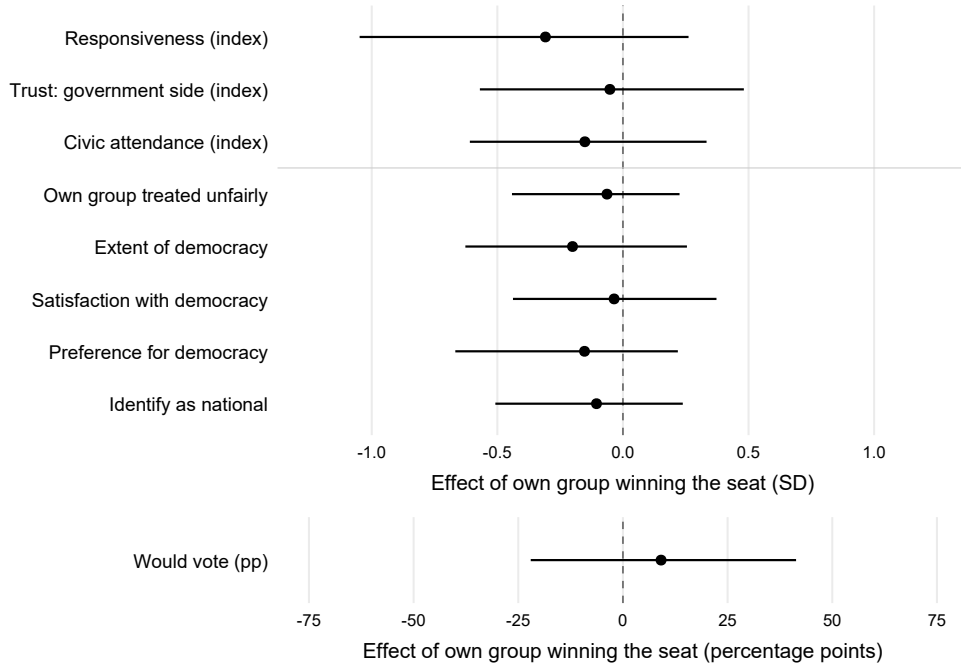
Table 6: The effect of co-ethnic representation

	Coef.	SE	95% CI	BW	N (L/R)
Responsiveness (index)	−0.309	0.334	[−1.048, 0.261]	0.137	65/70
Trust: government side (index)	−0.052	0.268	[−0.569, 0.481]	0.192	80/86
Civic attendance (index)	−0.152	0.240	[−0.609, 0.333]	0.152	72/77
Own group treated unfairly	−0.064	0.170	[−0.442, 0.225]	0.191	80/87
Would vote (pp)	9.1	16.2	[−22.0, 41.4]	0.030	23/16
Extent of democracy	−0.201	0.225	[−0.627, 0.255]	0.108	58/56
Satisfaction with democracy	−0.035	0.207	[−0.438, 0.372]	0.104	54/52
Preference for democracy	−0.153	0.226	[−0.667, 0.219]	0.113	58/59
Identify as national	−0.105	0.190	[−0.508, 0.238]	0.088	50/45

Notes: Regression discontinuity estimates of the effect of the respondent’s own ethnic group narrowly winning rather than narrowly losing the seat. Sample: members of the winner’s and the runner-up’s ethnic groups in races where the two belong to different groups; Kenya, Malawi, Nigeria, and Uganda. Local linear regression on group-by-race cells with a triangular kernel, weighted by the number of respondents; controls for the cell means of age, gender, education, urban residence, and years since the election, and country-by-election fixed effects. Coefficients are conventional estimates; standard errors and 95 percent confidence intervals are robust bias-corrected and clustered by race; stars refer to the robust bias-corrected test. The interval is centered on the bias-corrected estimate and therefore need not be symmetric around the coefficient. Indices are standardized on members of the runner-up’s group; single items are expressed in standard deviations of the same group, except would vote, which is in percentage points. BW: mean squared error optimal bandwidth on the margin of victory (a bandwidth of 0.10 corresponds to a 10-point vote-share margin). N (L/R): cells to the left and right of the cutoff within the bandwidth. Would vote: 91.6 percent of runner-up-group members within the bandwidth say they would vote; one percentage point equals 0.039 standard deviations of that group. The bandwidth selected for would vote is narrow and contains 26 races. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The absence of significant effects is informative only if the design could have detected effects of plausible size (Abadie 2020). The minimum detectable effect, the smallest true effect that the

Figure 2: The effect of co-ethnic representation



Notes: Regression discontinuity estimates from Table 6, with robust bias-corrected 95 percent confidence intervals clustered by race. The horizontal line separates the three indices from the single items. The lower panel shows the intention to vote in percentage points.

design would detect with 80 percent power in a two-sided test at the 5 percent level, equals 2.8 times the standard error in Table 6. Table C.4 in Appendix C converts them to the original response scales. The minimum detectable effects range from about 0.4 to 0.6 steps for the single items and from about 0.6 to 1.1 steps for the components of the three indices.

To judge whether half a step is large, we compare it with two benchmarks from the same surveys (Table C.4). The first is how much ethnic groups in the same country differ from one another. Within a country and election, the interquartile range of the ethnic-group means lies between 0.1 and 0.45 steps for these items. The minimum detectable effects are between one and five times this spread, and for most items between two and four times. The second benchmark is the national counterpart of the representation we study: the gap between members of groups that share in national executive power, as coded by the Ethnic Power Relations data, and members of excluded groups. This gap is about 0.1 steps or less for every item except perceived unfair treatment of the group, for which it is about a third of a step.

Measured against these benchmarks, the design detects only large effects: winning the seat would have to move a group's attitudes by more than the attitudes of different ethnic groups in the same country typically differ. The estimates rule out effects of this size in either direction, but not effects of the size of ordinary differences between ethnic groups. The least precise estimates are those for trust and perceived responsiveness, for which even effects of close to a full response category could go undetected. For the intention to vote, the minimum detectable effect is 45

percentage points, or 1.75 standard deviations, against a mean of 92 percent among members of the runner-up’s group, making this estimate uninformative.

Studies that use the same Afrobarometer items to examine related forms of representation report effects well below these thresholds. In the closest comparison, the random reservation of local council seats for women in Lesotho lowered the belief that councillors try their best to listen by 0.14 standard deviations, and by 0.22 standard deviations among women (Clayton 2015). Regarding national executive power, the president’s co-ethnics differ from other groups by only about a quarter to half of a standard deviation (Ahlerup and Isaksson 2015; Marx et al. 2021). These gaps lie below our minimum detectable effects for trust and satisfaction with democracy. Our design would detect only effects about two to seven times larger than those reported for the most comparable treatments. The estimates therefore rule out that a legislative seat transforms the political attitudes of the represented, but not effects of the size found for related forms of representation.

The consistency of the results across outcomes strengthens this reading. The outcomes capture different dimensions of the relationship between citizens and the state, from perceived responsiveness and trust to participation, democratic attitudes, and national identity. A large effect of representation would be expected to show up in at least some of them, and none of them moves significantly.

4.2 Heterogeneity

The average effect of winning a seat in a close race may conceal differences in how ethnic groups respond. We examine two dimensions. First, we split cells by whether the group held the seat at the previous election in the same area, which we measure by linking constituencies across elections geographically.⁹ Second, we split respondents by survey timing, comparing the first Afrobarometer round after each election with later rounds. Because this split is itself defined by the time elapsed since the election, we do not control for years since the election in the timing comparison.

We estimate each subsample as its own regression discontinuity, with its own bandwidth. To test whether the effects differ between two subsamples, we stack them in a single weighted regression in which every coefficient, including the slopes, covariates, and fixed effects, is specific to the subsample. Each subsample keeps its own bandwidth and kernel weights. The difference between the two treatment coefficients equals the difference between the separate estimates, and we cluster its standard error by race, since a race can contribute cells to both subsamples.

Transition into and continuity of representation. The *transition* subsample contains groups that did not hold the seat at the previous election. For them, a narrow win means entering representation and a narrow loss means remaining excluded. The *continuity* subsample contains

9. Constituency boundaries and identifiers change between elections. We link a constituency to its predecessor through the overlap of their boundaries and record whether a member of the group won the predecessor seat. The first election in each country’s series has no predecessor, so prior representation is observed for 255 of the 340 cells.

groups that held the seat. For them, a narrow win means keeping the seat and a narrow loss means losing it. Within each subsample, the estimate compares members of narrowly winning groups (*narrow winners*) with members of narrowly losing groups (*narrow losers*); an effect cannot be attributed to either side of the contrast alone. Table 7 reports the estimates for the two subsamples and the difference between them, and Figure 3 plots them.

Prior representation shapes how winning a seat relates to participation. The differences between the two subsamples are significant for civic attendance and the intention to vote, although only the former remains significant under the bootstrap in Appendix Table E.1. In the transition subsample, narrow winners attend community meetings and join others to raise an issue less often than narrow losers, and they are less likely to say they would vote. In the continuity subsample, narrow winners are more likely than narrow losers to say they would vote, and the two do not differ significantly in civic attendance. In the transition subsample, narrow winners also report more trust in political institutions than narrow losers, but the difference in trust between the two subsamples is not significant. Perceived responsiveness, evaluations of democracy, preference for democracy, perceived unfair treatment, and national identity do not differ between narrow winners and narrow losers in either subsample.

In the transition subsample, the participation results are consistent with delegation among groups that enter representation. However, they are equally consistent with mobilization among groups that remain excluded. While the design cannot definitively separate the two, literature on collective action and electoral losers suggests that near-misses and continued exclusion often act as powerful catalysts for civic engagement (e.g., Anderson et al. 2005; Moehler 2009). In the continuity subsample, the estimate for the intention to vote may likewise reflect higher turnout intentions among groups that keep the seat. Yet, consistent with theories of loss aversion (Kahneman and Tversky 1979) and political alienation, it is highly plausible that this divergence is driven primarily by the demobilization of groups experiencing the sudden loss of prior representation.

Time since the election. The effects of representation may differ across the electoral term. Early on, a newly won seat may raise expectations; later, citizens can observe what their representative delivers. We compare respondents interviewed in the first Afrobarometer round after each election with respondents interviewed in later rounds. Table 8 and Figure 4 report the estimates.

The effects of representation differ between the first and later survey rounds. The differences between the two periods are significant for four outcomes. For trust in government-side institutions, preference for democracy, and national identity, the estimates are lower in later rounds than in the first round; for the intention to vote, they are higher. Within each period, narrow winners in the first round are less likely than narrow losers to report that their group is treated unfairly, and narrow winners in later rounds report lower preference for democracy and national identity. Responsiveness, civic attendance, the extent of democracy, and satisfaction with democracy do not differ between periods.

Table 7: Transition into and continuity of representation

	Transition					Continuity					Continuity – transition		
	Coef.	SE	95% CI	BW	N (L/R)	Coef.	SE	95% CI	BW	N (L/R)	Diff.	SE	95% CI
Responsiveness (index)	-0.070	0.572	[-1.217, 1.027]	0.301	50/46	-0.313	0.280	[-0.934, 0.161]	0.131	29/27	-0.243	0.328	[-0.893, 0.407]
Trust: government side (index)	0.595***	0.240	[0.281, 1.220]	0.138	27/30	-0.083	0.392	[-0.932, 0.605]	0.181	31/32	-0.678	0.411	[-1.495, 0.139]
Civic attendance (index)	-0.565**	0.256	[-1.125, -0.124]	0.178	34/34	0.152	0.292	[-0.337, 0.806]	0.156	30/31	0.717**	0.349	[0.022, 1.411]
Own group treated unfairly	0.002	0.251	[-0.541, 0.444]	0.169	34/34	-0.134	0.365	[-0.916, 0.516]	0.163	30/32	-0.136	0.374	[-0.879, 0.607]
Would vote (pp)	-13.9*	7.8	[-29.7, 0.9]	0.099	22/23	19.6***	9.5	[6.0, 43.2]	0.104	24/20	33.5*	17.2	[-1.0, 67.9]
Extent of democracy	-0.176	0.286	[-0.704, 0.416]	0.196	36/33	0.178	0.296	[-0.449, 0.712]	0.138	29/27	0.354	0.267	[-0.176, 0.885]
Satisfaction with democracy	0.183	0.338	[-0.433, 0.893]	0.180	34/34	0.017	0.222	[-0.449, 0.422]	0.148	30/30	-0.166	0.236	[-0.636, 0.303]
Preference for democracy	0.048	0.159	[-0.234, 0.388]	0.182	34/34	-0.199	0.229	[-0.725, 0.172]	0.149	30/30	-0.248	0.311	[-0.865, 0.370]
Identify as national	-0.033	0.253	[-0.534, 0.458]	0.176	34/34	-0.057	0.363	[-0.746, 0.677]	0.193	32/34	-0.024	0.326	[-0.672, 0.624]

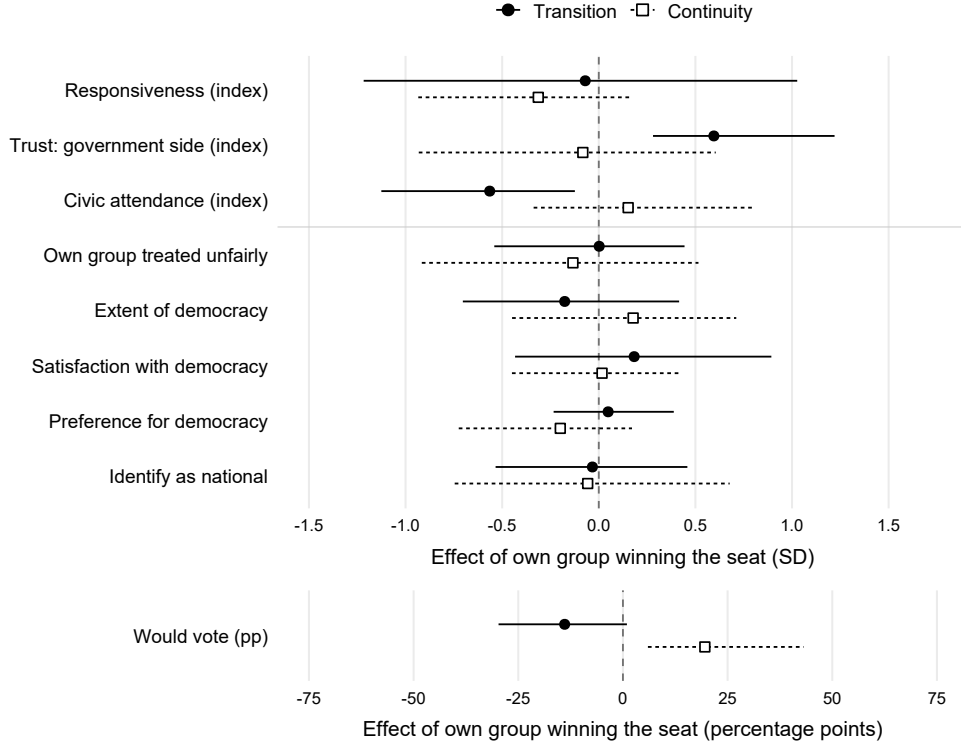
Notes: Regression discontinuity estimates of the effect of the respondent's own ethnic group narrowly winning rather than narrowly losing the seat. Sample: members of the winner's and the runner-up's ethnic groups in races where the two belong to different groups; Kenya, Malawi, Nigeria, and Uganda. Local linear regression on group-by-race cells with a triangular kernel, weighted by the number of respondents; controls for the cell means of age, gender, education, urban residence, and years since the election, and country-by-election fixed effects. Coefficients are conventional estimates; standard errors and 95 percent confidence intervals are robust bias-corrected and clustered by race; stars refer to the robust bias-corrected test. The interval is centered on the bias-corrected estimate and therefore need not be symmetric around the coefficient. Indices are standardized on members of the runner-up's group; single items are expressed in standard deviations of the same group, except would vote, which is in percentage points. BW: mean squared error optimal bandwidth on the margin of victory (a bandwidth of 0.10 corresponds to a 10-point vote-share margin). N (L/R): cells to the left and right of the cutoff within the bandwidth. Transition: cells of groups that did not hold the seat at the previous election in the geographically linked constituency; continuity: groups that did. Cells without an observed previous election are excluded. Each subsample is a separate regression discontinuity with its own bandwidth. The difference comes from both subsamples stacked in one weighted regression with all coefficients specific to the subsample, each at its own bandwidth, so that it equals the difference between the two estimates; its standard error is conventional and clustered by race, and its confidence interval uses the t distribution with the number of races minus one degrees of freedom. Table E.1 reports a race-cluster bootstrap of the bias-corrected difference. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Representation and time since the election

	First round after the election					Later rounds					Later – first		
	Coef.	SE	95% CI	BW	N (L/R)	Coef.	SE	95% CI	BW	N (L/R)	Diff.	SE	95% CI
Responsiveness (index)	–0.245	0.351	[–1.031, 0.345]	0.175	55/58	–0.161	0.262	[–0.702, 0.326]	0.172	48/45	0.084	0.277	[–0.466, 0.633]
Trust: government side (index)	0.329	0.401	[–0.318, 1.253]	0.180	56/58	–0.276	0.273	[–0.807, 0.261]	0.126	41/38	–0.605*	0.308	[–1.216, 0.006]
Civic attendance (index)	–0.175	0.195	[–0.559, 0.206]	0.194	59/63	–0.067	0.289	[–0.621, 0.513]	0.139	41/40	0.108	0.317	[–0.521, 0.737]
Own group treated unfairly	–0.689**	0.337	[–1.467, –0.148]	0.087	38/32	0.050	0.362	[–0.591, 0.826]	0.155	46/42	0.738	0.475	[–0.206, 1.683]
Would vote (pp)	–3.1	10.6	[–24.7, 16.9]	0.232	67/68	7.1	7.1	[–6.0, 22.0]	0.179	46/43	10.3*	5.5	[–0.6, 21.1]
Extent of democracy	–0.142	0.269	[–0.627, 0.428]	0.122	48/44	–0.179	0.210	[–0.620, 0.204]	0.158	46/42	–0.038	0.314	[–0.661, 0.585]
Satisfaction with democracy	0.179	0.316	[–0.383, 0.855]	0.108	43/39	–0.251	0.250	[–0.755, 0.223]	0.062	24/18	–0.429	0.356	[–1.140, 0.281]
Preference for democracy	0.233	0.198	[–0.134, 0.642]	0.164	54/55	–0.539**	0.250	[–1.081, –0.103]	0.093	33/25	–0.772***	0.272	[–1.313, –0.231]
Identify as national	0.266	0.213	[–0.130, 0.705]	0.206	62/65	–0.464*	0.274	[–0.995, 0.080]	0.093	33/25	–0.731*	0.407	[–1.537, 0.076]

Notes: Regression discontinuity estimates of the effect of the respondent's own ethnic group narrowly winning rather than narrowly losing the seat. Sample: members of the winner's and the runner-up's ethnic groups in races where the two belong to different groups; Kenya, Malawi, Nigeria, and Uganda. Local linear regression on group-by-race cells with a triangular kernel, weighted by the number of respondents; controls for the cell means of age, gender, education, and urban residence, and country-by-election fixed effects. Coefficients are conventional estimates; standard errors and 95 percent confidence intervals are robust bias-corrected and clustered by race; stars refer to the robust bias-corrected test. The interval is centered on the bias-corrected estimate and therefore need not be symmetric around the coefficient. Indices are standardized on members of the runner-up's group; single items are expressed in standard deviations of the same group, except would vote, which is in percentage points. BW: mean squared error optimal bandwidth on the margin of victory (a bandwidth of 0.10 corresponds to a 10-point vote-share margin). N (L/R): cells to the left and right of the cutoff within the bandwidth. Years since the election is not controlled for, since the split is defined by survey timing. First round: the earliest Afrobarometer round after the election; later rounds: all other rounds matched to it (none for Malawi 2014, Nigeria 2015, and Uganda 2006). Each subsample is a separate regression discontinuity with its own bandwidth. The difference comes from both subsamples stacked in one weighted regression with all coefficients specific to the subsample, each at its own bandwidth, so that it equals the difference between the two estimates; its standard error is conventional and clustered by race, and its confidence interval uses the t distribution with the number of races minus one degrees of freedom. Table E.1 reports a race-cluster bootstrap of the bias-corrected difference. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3: Transition into and continuity of representation



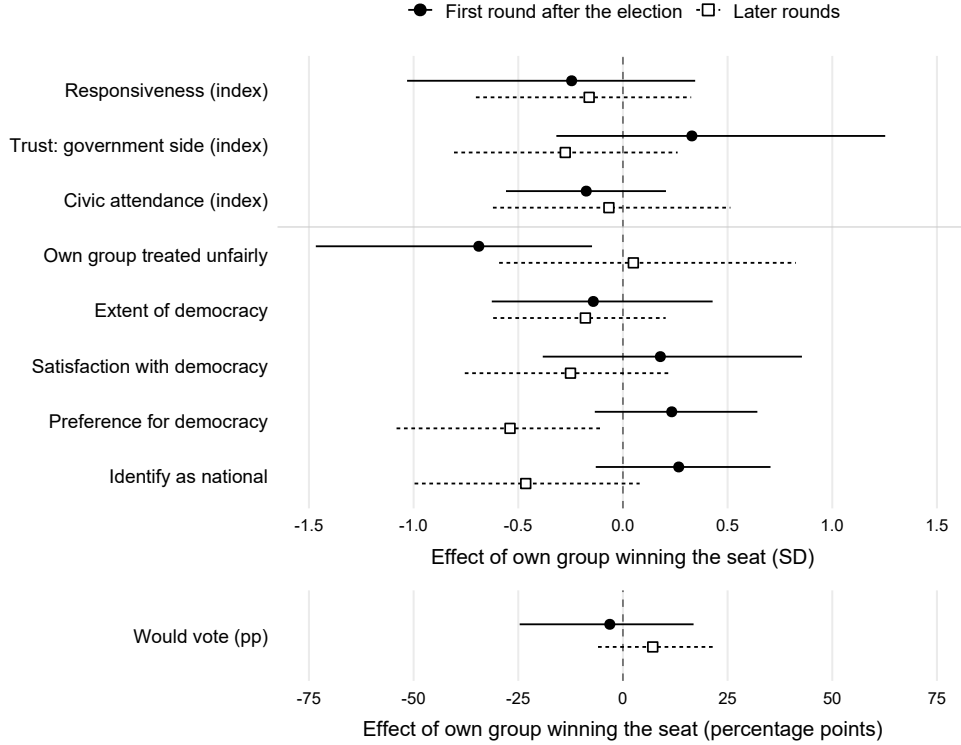
Notes: Regression discontinuity estimates from Table 7, estimated separately for groups that did not hold the seat at the previous election (transition) and groups that did (continuity), with robust bias-corrected 95 percent confidence intervals clustered by race. The horizontal line separates the three indices from the single items.

Inference on the differences. The tests for differences between subsamples use conventional standard errors, whereas the subsample estimates use robust bias-corrected inference. Appendix Table E.1 therefore re-estimates each difference as the gap between the bias-corrected estimates of the two subsamples, with standard errors from a bootstrap that resamples races. Under this more conservative procedure, three differences remain significant: civic attendance between the transition and continuity subsamples, and trust in government-side institutions and preference for democracy between survey rounds. The differences in the intention to vote between the transition and continuity subsamples and in national identity between survey rounds are not significant, and we treat them as suggestive. The difference in the intention to vote between survey rounds remains significant, but its bootstrap distribution is skewed, so we interpret it with caution as well.

4.3 Robustness

We check that the main estimates do not depend on the choice of bandwidth, on any single country, or on the local ethnic composition of the constituency, and we run a placebo test on outcomes measured before the election. Table 9 and Figure 5 report the results.

Figure 4: Representation and time since the election

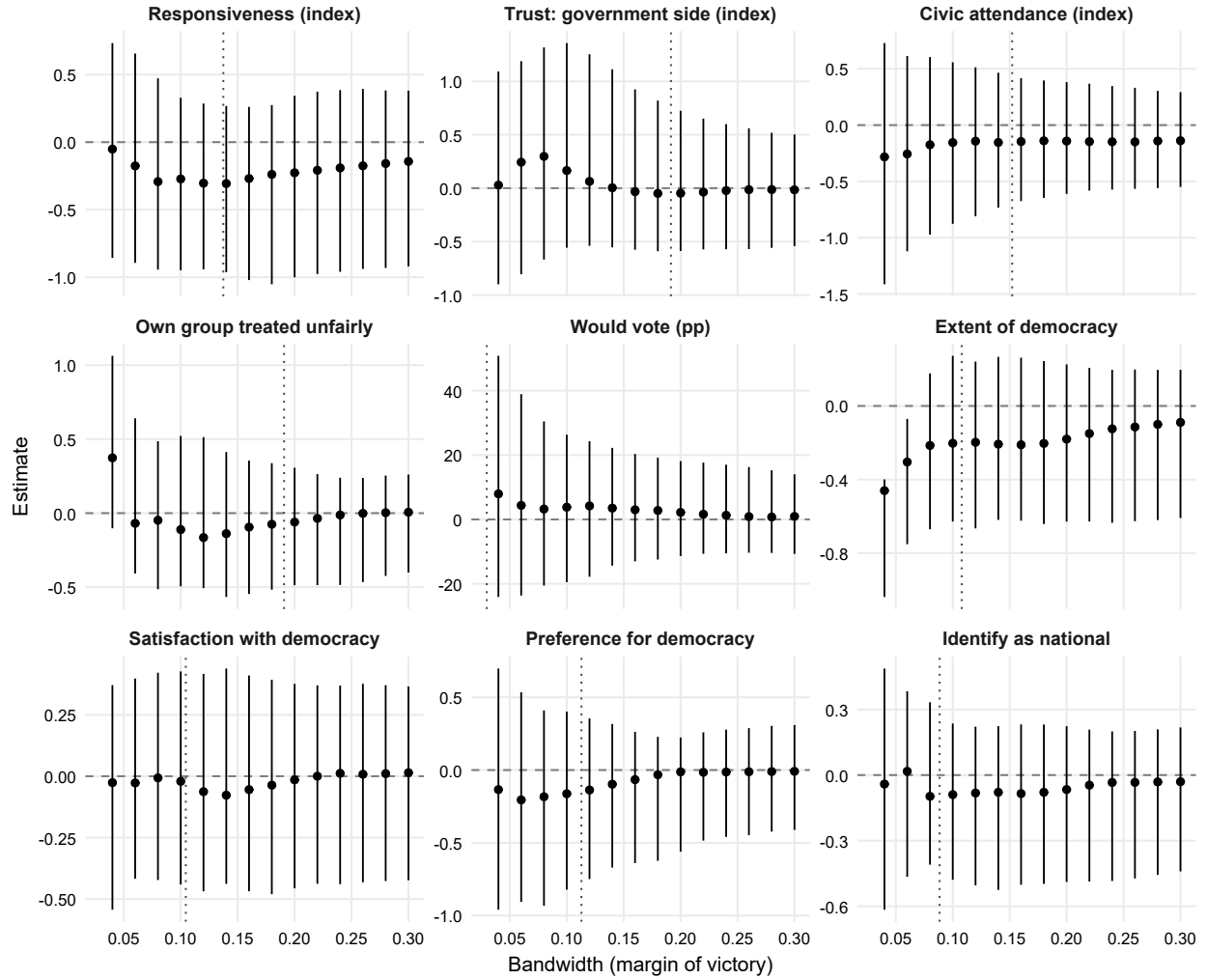


Notes: Regression discontinuity estimates from Table 8, estimated separately for respondents interviewed in the first Afrobarometer round after the election and in later rounds, with robust bias-corrected 95 percent confidence intervals clustered by race. The horizontal line separates the three indices from the single items.

Bandwidth sensitivity. We examine the sensitivity of the estimates to the bandwidth by re-estimating equation (1) at fixed bandwidths from 0.04 to 0.30, in steps of 0.02, that is, in races decided by margins of up to 4 to up to 30 percentage points. Figure 5 plots the estimates. Beyond the narrowest windows, the estimates change little with the bandwidth, and none is significant at any bandwidth with one exception. The estimate for the extent of democracy is negative at every bandwidth and significant at bandwidths of 0.04 and 0.06, which contain only the closest races. As the window widens, it shrinks toward zero. For the intention to vote, the bandwidth selected in Table 6 is narrower than any bandwidth in the grid: it corresponds to races decided by about 3 percentage points and contains only 26 races. We do not extend the grid below 0.04, which already contains only 33 to 34 races for every outcome. At 0.04, the estimate for the intention to vote is close to the main estimate and equally imprecise.

Leaving out one country at a time. The sample pools four countries that differ in the number of close mixed races and of respondents per race. To check that no single country drives the pooled estimates, we re-estimate equation (1) four times, each time dropping one country. Columns 2 to 5 of Table 9 report the estimates. Most estimates keep the sign of the pooled estimate, and almost

Figure 5: Bandwidth sensitivity



Notes: Regression discontinuity estimates with the specification of Table 6 at fixed bandwidths of the margin of victory from 0.04 to 0.30, with robust bias-corrected 95 percent confidence intervals clustered by race. The dotted vertical line marks the mean squared error optimal bandwidth used in Table 6; for would vote, this bandwidth (0.03) lies below the smallest bandwidth shown. Indices and single items are in standard deviations of members of the runner-up's group; would vote is in percentage points.

all remain insignificant. Two estimates become significant: the intention to vote when Kenya is dropped, and preference for democracy when Malawi is dropped. The estimates for preference for democracy vary most across samples and change sign when Kenya or Uganda is dropped.

Local ethnic composition. Among close races, the winning group tends to be the locally smaller one (Section 3). Column 6 of Table 9 therefore adds the group’s own local population share and an indicator for the group being the locally largest one to the covariates. Most estimates change little. The exception is again the extent of democracy, whose estimate becomes larger in absolute value and significant. With the additional covariates, the bandwidth selected for this outcome narrows to races decided by about 4 percentage points, and the adjusted estimate is close to the unadjusted estimate at the same bandwidth in Figure 5. The change therefore reflects the narrower window rather than the adjustment.

Placebo outcomes. For each cell, we measure the outcomes among members of the same group in the same area, using the Afrobarometer rounds matched to the previous election. We place these respondents in the constituency as drawn at the current election, using the location of their enumeration area. They were interviewed before the election that defines treatment, so the discontinuity in their outcomes should be zero. We assign each pre-election cell the running variable and treatment of the current race. We then estimate equation (1) on the pre-election outcomes, with covariates measured among the pre-election respondents and years since the previous election in place of years since the current one. The first election of each country has no earlier survey round, and not every group is surveyed in the same constituency in both periods, so the placebo covers 141 of the 340 cells.¹⁰ Column 7 of Table 9 reports the estimates. We do not find significant pre-election differences between winning and losing groups.

5 Conclusion

Descriptive representation is often expected to bind groups more closely to democratic institutions. In ethnically divided societies, where a seat in parliament is widely read as a seat for a group, this expectation is especially strong. In this paper, we test it using close legislative elections in Kenya, Malawi, Nigeria, and Uganda. We compare members of two ethnic groups whose candidates finished first and second in the same race. We ask how narrowly winning rather than narrowly losing the seat changes their political engagement and their views of democracy.

On average, we find no significant effect of winning representation on how members of the represented group relate to politics. Their perceptions of responsiveness, trust in political institutions, civic participation, evaluations of and commitment to democracy, sense of unfair treatment, and

10. Comparing the covered cells with the other cells within country and election, the two do not differ in the vote-share gap, the side of the cutoff, or the group’s local population share. Covered cells contain more respondents and more often belong to groups that held the seat at the previous election, since larger and long-represented groups are more likely to be surveyed in the same area in both periods.

Table 9: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Main	Without				Local	Pre-election
	estimate	Kenya	Malawi	Nigeria	Uganda	composition	placebo
Responsiveness (index)	−0.309 (0.334)	−0.502 (0.350)	−0.073 (0.284)	−0.264 (0.349)	−0.318 (0.334)	−0.244 (0.335)	−0.315 (0.252)
Trust: government side (index)	−0.052 (0.268)	−0.176 (0.290)	0.008 (0.289)	−0.069 (0.284)	0.003 (0.349)	−0.007 (0.291)	−0.173 (0.215)
Civic attendance (index)	−0.152 (0.240)	−0.150 (0.252)	−0.088 (0.199)	−0.145 (0.269)	−0.216 (0.187)	−0.151 (0.197)	−0.085 (0.139)
Own group treated unfairly	−0.064 (0.170)	−0.032 (0.228)	0.090 (0.213)	−0.154 (0.195)	−0.147 (0.216)	−0.035 (0.199)	0.179 (0.259)
Would vote (pp)	9.1 (16.2)	9.8** (6.2)	−0.7 (10.1)	3.4 (5.0)	2.1 (8.4)	3.1 (10.5)	−5.1 (6.0)
Extent of democracy	−0.201 (0.225)	−0.263 (0.261)	−0.056 (0.265)	−0.212 (0.272)	−0.328 (0.253)	−0.460*** (0.149)	0.151 (0.191)
Satisfaction with democracy	−0.035 (0.207)	−0.249 (0.207)	0.051 (0.201)	−0.048 (0.247)	−0.099 (0.217)	−0.016 (0.199)	0.222 (0.429)
Preference for democracy	−0.153 (0.226)	0.426 (0.312)	−0.321** (0.187)	−0.092 (0.206)	0.062 (0.283)	−0.176 (0.289)	0.037 (0.093)
Identify as national	−0.105 (0.190)	−0.121 (0.324)	−0.108 (0.153)	−0.079 (0.187)	0.090 (0.200)	−0.109 (0.173)	0.162 (0.182)

Notes: Regression discontinuity estimates with the specification of Table 6; standard errors in parentheses. Column (1) reproduces Table 6. Columns (2) to (5) drop one country at a time. Column (6) adds the group's own local population share and an indicator for the group being the locally largest one (DHS, or census in Uganda) to the covariates. Column (7) uses respondents interviewed in the Afrobarometer rounds matched to the previous election, who belong to the same group and live in the same constituency as drawn at the current election; the running variable and treatment are those of the current race, and years since the previous election replaces years since the current one. It covers 1,691 respondents in 141 of the 340 group cells (103 races); the first election of each country has no earlier round. Each column selects its own mean squared error optimal bandwidth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

national identity do not significantly change at the election cutoff. The design is precise enough to rule out large effects in either direction.

The average conceals meaningful differences. Groups that transition into representation report more trust in the institutions in which they are now represented, but less civic participation and a lower intention to vote, a pattern consistent with delegation at entry but also with mobilization among groups that remain excluded. Groups whose representation continues report a higher intention to vote. The estimates also differ between survey rounds. In the first survey round after the election, members of the winning group are less likely to feel that their group is treated unfairly by the government. In later rounds, the effects of representation on their trust in government, their preference for democracy, and their attachment to the nation are lower, while the effect on their willingness to vote is higher.

These findings carry three implications for the study of descriptive representation. First, we find no evidence that a seat for one's group changes attitudes toward democracy. Where the estimates do point to differences in citizens' behavior, they are consistent with members of newly represented groups delegating political action rather than engaging more. Second, the consequences of winning differ by a group's political history: entering office and holding on to are followed by different patterns of participation. Third, ethnic representation is at stake in only a minority of races. Where groups live and who stands for office determine, to a large extent, whether elections can change which group is represented at all.

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A Measuring candidate ethnicity

A.1 Procedure

No administrative source records the ethnicity of parliamentary candidates in any of the four countries. We therefore infer it from the candidate’s name. We classify every candidate that CLEA records in every election to which an Afrobarometer round can be matched; Table A.1 reports the candidates in the races we study. We classify candidates with a large language model (`claude-haiku-4-5`) in batches of 25, using a structured output format in which the label is restricted to a closed list of groups for each country. Restricting the output at the level of the schema, rather than by instruction alone, means that the model cannot return a category outside the list. The prompt supplies the candidate’s name together with the constituency and the region, since geographic context often decides between groups whose naming conventions overlap. The model also returns the name token it treated as diagnostic and a confidence score. We do not filter on confidence. We accessed the model through the Anthropic API (model identifier `claude-haiku-4-5`) with the default temperature and forced structured output. Because outputs at nonzero temperature are not deterministic, the replication files contain the predicted label for every candidate, together with the prompts and scripts.

The list of groups for each country is built from the ethnic categories that Afrobarometer records for that country, aggregated to the level at which names are plausibly informative. This ensures that candidate and respondent ethnicity are measured in the same categories. For Nigeria, whose ethnic landscape is the most fragmented, the list was expanded after an initial pass to separate minority groups that the first list absorbed into larger ones. Each list includes a residual category, *Other / Ambiguous*, for names the model cannot place.

A.2 Coverage

Table A.1 summarizes the classified rosters. Residual assignments are rare, and nearly all Afrobarometer respondents report a group that maps onto a candidate label. Confidence is lowest in Nigeria, which has by far the largest list of groups, consistent with confidence declining as the model must distinguish among more and finer categories.

A.3 Hand-coded labels and validation

For 282 Nigerian candidates, we have labels coded by hand from documentary sources, such as biographies, news reports, and official profiles. These labels always replace the model’s prediction. The hand-coded candidates come from three batches: a random draw of Nigerian candidates, candidates in close races that the model could not classify with confidence, and an additional batch of names drawn to cover smaller groups.

We validate the model against the random batch only, since the other two batches were selected on being difficult to classify. Among the randomly drawn Nigerian candidates with an unambiguous hand-coded label, the model assigns the correct group to 88.9 percent (95 percent Wilson interval: 77.8 to 94.8 percent). Agreement among candidates in close races, the races that identify the design, is similar. Disagreements are not all equally consequential. Assigning a minority candidate to a large group can create apparent co-ethnicity with many respondents and can make an ethnically contested race look uncontested. We therefore tracked disagreements by type, and the expansion of the Nigerian list described above roughly halved misassignments into the largest groups while leaving overall agreement nearly unchanged. The validation covers Nigeria only. Because Nigeria has by far the largest list of groups, it is a demanding test of the model, but it does not rule out

Table A.1: Candidate classification coverage and respondent match rates

<i>Panel A. Candidates</i>							
Country	Elections	Races	Candidates	Labels		Residual (%)	Median confidence
				Available	Used		
Kenya	4	1,041	7,816	21	19	2.0	0.89
Malawi	4	759	4,930	18	13	9.6	0.80
Nigeria	3	1,051	8,104	59	52	3.0	0.75
Uganda	3	943	4,123	34	30	2.8	0.82
Total	14	3,794	24,973				
<i>Panel B. Afrobarometer respondents</i>							
Ethnic categories		Respondents	Matched to a label		Candidates with confidence ≥ 0.85		
Kenya	57	13,189	99.5%		75.2%		
Malawi	18	10,413	99.9%		23.9%		
Nigeria	84	14,840	96.0%		31.4%		
Uganda	41	14,073	99.9%		44.2%		

Notes: Panel A covers the candidates in the 3,794 races of Table 3: single-seat races with at least two candidates in the 14 elections we study. The model also classified the candidates in two further elections that can be matched to an Afrobarometer round but are not part of the analysis (Malawi 1999 and Uganda 2021), and in races that are excluded because they have a single candidate or are not ordinary single-seat constituencies, such as the district woman representative seats in Uganda; including these gives 4,636 races in 16 elections. Races are constituency-by-election contests.

difficulties specific to other countries, such as the overlap between Chewa, Nyanja, and Lomwe names in Malawi. Hand-coding of random samples of candidates in Kenya, Malawi, and Uganda is under way and will extend the validation to all four countries.

A.4 A shared set of group labels

Candidates, Afrobarometer respondents, and DHS respondents share one set of group labels. Where candidate names cannot distinguish between groups, we merge them in all three sources: Hausa and Fulani in Nigeria, and Borana, Gabra, and Orma in Kenya. Some Afrobarometer and DHS categories combine several groups, such as Meru/Embu in Kenya or Lugbara/Madi in Uganda. A respondent in such a composite category counts as a member of the winner’s or the runner-up’s group if the composite contains that group and not the other. Respondents whose composite category contains both groups are excluded, since their treatment status cannot be determined.

B Data construction

B.1 Placing respondents in constituencies

Constituency identifiers in CLEA are serial numbers within an election rather than persistent codes, so the same number can refer to different constituencies in different elections. We therefore place respondents in constituencies geographically, separately for each election. Afrobarometer records the coordinates of each enumeration area, which we locate in the constituency boundaries valid at the time of the matched election.

For Kenya, we use the harmonized constituency maps of Amodio et al. (2024). Kenya increased the number of constituencies from 210 to 290 in 2010. For the elections of 2002 and 2007, we map

the 290 post-2010 constituencies back to the 210 earlier ones, using exact name matches where the name did not change, the correspondence of Amodio et al. (2024) where it did, and hand coding for the remaining cases. For Malawi, we use a constituency shapefile. For Nigeria, federal constituencies are defined as groups of local government areas. We place respondents in local government areas and aggregate these to constituencies separately for each election, decomposing each constituency name in CLEA into the local government areas it covers. For Uganda, the boundary file of Amodio et al. (2024) contains fewer areas than there are constituencies, and their correspondence links each area to one constituency by matching names. Some areas contain more than one constituency, for example a county that was later split into a northern and a southern constituency, or a municipality that forms its own constituency within a county. The correspondence then links the area to only one of these constituencies, and we assign all respondents in the area to it. Of the ordinary (non-reserved) constituencies, 203 of 226 have their own boundary in 2006, 224 of 237 in 2011, and 283 of 296 in 2016. In the analysis sample, 86 of the 728 Ugandan respondents, in 5 of the 49 Ugandan races, live in districts where at least one constituency has no boundary of its own.

B.2 Matching survey rounds to elections

We match each Afrobarometer round to the most recent legislative election held before its fieldwork (Table 1). The first round after an election is the earliest round matched to it; all other rounds matched to the same election are later rounds. Three elections are followed by only one round (Malawi 2014, Nigeria 2015, and Uganda 2006) and therefore contribute only to the first-round subsample.

B.3 Respondent ethnicity

Afrobarometer asks respondents to name their ethnic community or tribe. The codes assigned to groups differ across survey rounds, so we map each combination of round and code to a group through a round-specific crosswalk. Round 2 does not include an ethnicity question. For Kenya, we instead use the respondent’s home language, which Round 2 records with a detailed list of languages. In Rounds 3 to 6, which ask both questions, the group implied by the home language agrees with the self-reported group for 95 percent of Kenyan respondents, and most disagreements concern the level of detail, such as Meru against the composite Meru/Embu. We do not use this proxy in other countries. In Malawi, for example, members of several minority groups report Chichewa, the national lingua franca, as their home language, so the proxy would systematically assign them to the largest group.

B.4 Local ethnic composition

In Kenya, Malawi, and Nigeria, we measure the ethnic composition of each constituency with the Demographic and Health Surveys (DHS). We place each DHS survey cluster in a constituency by its coordinates and compute, for each group, its share of DHS respondents in the constituency, pooling women and men and all surveys listed in Table 1. Shares use only respondents located inside the constituency. The surveys differ in which groups they code separately, and a survey that lacks a code for a group assigns its members to a neighboring group. To avoid this, we use a survey in a constituency only if the groups it cannot code make up less than 5 percent of the constituency’s population according to the survey with the most detailed list. We also correct survey codes that are clearly mislabeled, identified by comparing where respondents with a given code live across surveys.

In Uganda, only the 2011 AIDS Indicator Survey records both ethnicity and cluster locations, which leaves a median of two survey clusters per constituency. We therefore use the 10 percent microdata samples of the 2002 and 2014 population censuses distributed by IPUMS International, which record the ethnicity of every enumerated Ugandan and locate households in counties (2002) and sub-counties (2014). We apportion each census unit to the constituency polygons by the share of its area in each polygon; 2014 sub-counties nest almost exactly within the polygons, whereas about two in five 2002 counties straddle a boundary. We exclude visitors and apply the same rule as for the DHS: the 2014 census, which has the most detailed list of groups, is always used, and the 2002 census is used in a constituency only where the groups it does not code separately make up less than 5 percent of the population. Census groups that neither the candidate labels nor Afrobarometer distinguish are assigned to the corresponding project label, for example the Dodoth and Jie to the Karimojong and the Banyaruguru to the Banyankole. The census shares agree closely with the AIDS Indicator Survey where the latter has data: the two name the same largest group in 86 percent of constituencies, and in 91 percent of those where the census gives that group more than half of the population. We use these shares to describe the groups that compete and to test the validity of the design; they never define treatment.

B.5 Prior representation

For each group in a race, we record whether a candidate of the same group held the seat after the previous election in the same area. Since constituency identifiers and boundaries change between elections, we link each constituency to its predecessor through the overlap of their boundaries, never through the identifier. The group label of the previous seat holder is taken from the same classification as the current candidates. Prior representation is missing, and the cell excluded from the analysis by prior representation, when the election is the first in the country’s series or when the constituency cannot be linked to a predecessor; it is never coded as zero. It is observed for 255 of the 340 cells.

C Outcomes

C.1 Question wording

Table C.1 lists the Afrobarometer questions behind the outcomes. Wording is from Round 6; it differs slightly in other rounds.

C.2 Index construction and factor analysis

To decide which items to combine, we ran an exploratory factor analysis of 22 Afrobarometer items on democratic attitudes, trust, participation, responsiveness, and identity. Because the items are ordinal, we compute polychoric correlations separately within each country and average them across Kenya, Malawi, Nigeria, and Uganda, so that differences in levels between countries do not drive the factor structure. The analysis uses all Afrobarometer respondents in the four countries and the rounds we study, not only the analysis sample. Parallel analysis retains eight factors, which we extract by minimum residuals with an oblique (oblimin) rotation.

Table C.2 reports the solution. The three indices correspond to three clearly separated factors. Trust in parliament, local government, and the ruling party load on a common factor of trust in government-side institutions, while trust in the opposition forms a factor of its own. The two questions on whether members of parliament and local councillors listen form a responsiveness factor, and attending community meetings and joining others to raise an issue form a civic attendance

Table C.1: Outcome questions

Outcome	Question	Scale
<i>Responsiveness (index)</i>		
MP tries to listen	How much of the time do you think the following try their best to listen to what people like you have to say: Members of Parliament?	0 never – 3 always
Councillor tries to listen	[Same question:] Local government councillors?	0 never – 3 always
<i>Trust, government side (index)</i>		
Trust in parliament, local government, ruling party	How much do you trust each of the following, or haven't you heard enough about them to say: Parliament? Your elected local government council? The ruling party?	0 not at all – 3 a lot
<i>Civic attendance (index)</i>		
Attend community meeting; join others to raise an issue	Here is a list of actions that people sometimes take as citizens. For each of these, please tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Attended a community meeting? Got together with others to raise an issue?	0 no, would never – 4 yes, often
<i>Single items</i>		
Extent of democracy	In your opinion, how much of a democracy is [country] today?	1 not a democracy – 4 full democracy
Satisfaction with democracy	Overall, how satisfied are you with the way democracy works in [country]?	0 not a democracy – 4 very satisfied
Preference for democracy	Which of these three statements is closest to your own opinion? Democracy is preferable to any other kind of government. In some circumstances, a non-democratic government can be preferable. For someone like me, it doesn't matter what kind of government we have.	1 doesn't matter – 3 democracy preferable
Own group treated unfairly	How often, if ever, are [respondent's ethnic group] treated unfairly by the government?	0 never – 3 always
Identify as national	Let us suppose that you had to choose between being a [national] and being a [respondent's ethnic group]. Which of the following statements best expresses your feelings?	1 only ethnic – 5 only national
Would vote	If a presidential election were held tomorrow, which party's candidate would you vote for?	1 names a party or candidate, 0 would not vote

Notes: Wording from Afrobarometer Round 6; wording differs slightly across rounds. Responses of “don't know” and refusals are treated as missing. For the intention to vote, respondents who name a party or candidate are coded one and respondents who say they would not vote are coded zero.

factor. The extent of and satisfaction with democracy load on a common factor; we report them as separate items. Preference for democracy, perceived unfair treatment, and national identification do not load clearly on any factor, so we also report them as single items, together with the intention to vote. The factors underlying the three indices replicate when the analysis is run separately in each country and in the analysis sample.

The ordinal reliability (Cronbach’s alpha on polychoric correlations) of the indices is 0.84 for responsiveness, 0.79 for trust in government-side institutions, and 0.77 for civic attendance. Each index is the average of its items, each standardized within country, over the items a respondent answered; the average is then standardized by the mean and standard deviation of members of the runner-up’s group.

Table C.2: Exploratory factor analysis of the Afrobarometer items

Item	Primary factor	Loading	Communality
Trust: president [†]	Trust, government side	0.88	0.74
Trust: ruling party	Trust, government side	0.79	0.68
Trust: parliament	Trust, government side	0.55	0.60
Trust: local government	Trust, government side	0.42	0.55
Own group treated unfairly	(Trust, government side)	−0.24	0.12
Reject one-party rule [†]	Reject authoritarianism	0.80	0.63
Reject military rule [†]	Reject authoritarianism	0.69	0.46
Reject one-man rule [†]	Reject authoritarianism	0.64	0.44
Preference for democracy	(Reject authoritarianism)	0.25	0.13
Councillor tries to listen	Responsiveness	1.02	1.01
MP tries to listen	Responsiveness	0.68	0.53
Extent of democracy	Democracy evaluation	0.82	0.64
Satisfaction with democracy	Democracy evaluation	0.75	0.62
Identify as national	(Democracy evaluation)	0.09	0.03
Join others to raise an issue	Civic attendance	0.79	0.63
Attend community meeting	Civic attendance	0.79	0.63
Contact MP [†]	Direct contacting	1.01	1.00
Contact local official [†]	Direct contacting	0.50	0.48
Trust: opposition [†]	Trust in the opposition	0.59	0.33
Discuss politics [†]	Discussion and voting	0.59	0.38
Would vote	Discussion and voting	0.40	0.27
Attend demonstration [†]	(Discussion and voting)	0.19	0.13

Notes: Minimum residual factor analysis with oblimin rotation of 22 Afrobarometer items, on polychoric correlations computed within each country and averaged across Kenya, Malawi, Nigeria, and Uganda (all respondents in the survey rounds used, not only the analysis sample). Parallel analysis retains eight factors. Each item is listed with the factor on which it loads most strongly; factor names are descriptive. Factors in parentheses: the item’s largest loading is below 0.4, so it does not load clearly on any factor. Loadings slightly above one occur for the two-item factors.

[†]Item not used in the paper.

C.3 Components of the indices

Table C.3 reports the estimates for each item that enters one of the three indices, with the specification of the main results.

Table C.3: The effect of co-ethnic representation on the components of the indices

	Coef.	SE	p	BW	Races	N (L/R)
<i>Responsiveness</i>						
MP tries to listen	-0.174	0.337	0.561	0.185	112	77/85
Councillor tries to listen	-0.299	0.254	0.165	0.131	84	61/62
<i>Trust, government side</i>						
Trust: parliament	-0.071	0.303	0.842	0.175	107	75/81
Trust: local government	0.352	0.327	0.172	0.077	56	44/38
Trust: ruling party	-0.047	0.253	0.899	0.151	101	72/75
<i>Civic attendance</i>						
Join others to raise an issue	-0.175	0.169	0.287	0.140	95	68/71
Attend community meeting	-0.088	0.312	0.849	0.142	96	68/72

Notes: Regression discontinuity estimates for each item that enters one of the three indices, with the specification of Table 6. Items are expressed in standard deviations of members of the runner-up's group. Coefficients are conventional estimates; standard errors and p -values are robust bias-corrected and clustered by race. BW: mean squared error optimal bandwidth on the margin of victory. Races: races within the bandwidth. N (L/R): cells to the left and right of the cutoff. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.4 Minimum detectable effects and benchmarks

Table C.4 expresses the minimum detectable effects of the item-level estimates in steps of the original response scales and compares them with two benchmarks computed from all Afrobarometer respondents matched to the 14 elections. The first is the spread between ethnic groups within a country and election, measured by the interquartile range of the group means. The second is the gap between members of groups that Ethnic Power Relations codes as sharing in national executive power and members of other groups.

D Distribution of the running variable

Figure D.1 shows the distribution of the running variable across the group cells in the analysis sample. Panel (a) plots the number of cells by the margin of the group's own candidate, and panel (b) the local polynomial density estimate on each side of the cutoff. The density is smooth at the cutoff, and the formal test reported in Table 5 does not reject continuity.

E Differences between subsamples

The differences between subsamples in Tables 7 and 8 come from a stacked regression with conventional standard errors, whereas the estimates within each subsample use robust bias-corrected inference. Table E.1 places the differences on the same footing as the subsample estimates. For each outcome, we compute the difference between the bias-corrected estimates of the two subsamples, each at the bandwidths selected in the main tables. We obtain its standard error and confidence interval from a bootstrap that resamples races with replacement. Because races are resampled as a whole, a race that contributes cells to both subsamples enters both or neither, so the bootstrap accounts for the dependence between the two estimates. The confidence interval spans the 2.5th to the 97.5th percentile of the bootstrap distribution.

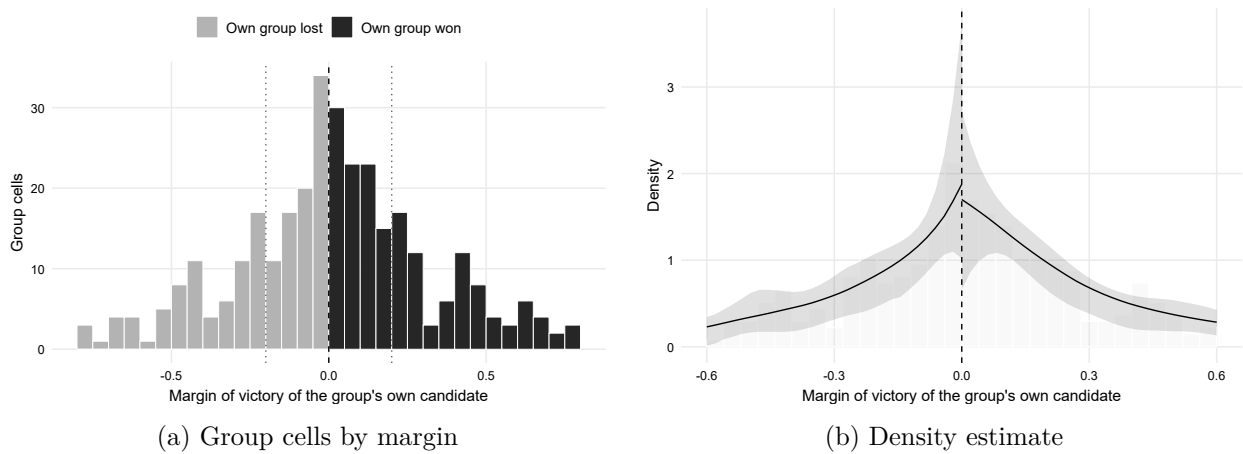
The bootstrap standard errors are larger than the conventional ones for most outcomes, so fewer

Table C.4: Minimum detectable effects and benchmarks, in response steps

	Scale	SD, runner-up group	Minimum detectable effect	Spread of group means	National power gap
<i>Responsiveness (index components)</i>					
MP tries to listen	0–3	0.83	0.79	0.15	−0.02
Councillor tries to listen	0–3	0.90	0.64	0.21	0.01
<i>Trust, government side (index components)</i>					
Trust: parliament	0–3	1.06	0.90	0.28	−0.06
Trust: local government	0–3	1.03	0.94	0.27	−0.06
Trust: ruling party	0–3	1.10	0.78	0.43	0.12
<i>Civic attendance (index components)</i>					
Join others to raise an issue	0–4	1.30	0.62	0.21	−0.04
Attend community meeting	0–4	1.26	1.10	0.26	−0.09
<i>Single items</i>					
Own group treated unfairly	0–3	1.02	0.48	0.44	−0.34
Extent of democracy	1–4	0.85	0.53	0.21	−0.04
Satisfaction with democracy	0–4	0.99	0.57	0.28	0.03
Preference for democracy	1–3	0.68	0.43	0.11	−0.02
Identify as national	1–5	1.14	0.61	0.26	0.01
Would vote (pp)	0/1	25.9	45.3	6.1	1.0

Notes: All columns except the scale are in steps of the original response scale (percentage points for would vote). SD, runner-up group: standard deviation among members of the runner-up’s group in the analysis sample. Minimum detectable effect: 2.8 times the standard error of the item’s estimate with the specification of Table 6 (80 percent power, 5 percent two-sided test). Spread of group means: interquartile range of the means of ethnic groups with at least 30 respondents, within each country-election, averaged over country-elections with weights equal to the number of respondents. National power gap: difference between members of groups that Ethnic Power Relations codes as included in national executive power (dominant, senior or junior partner) and members of other groups, from a regression with country-by-election fixed effects; EPR status is available for 43 percent of these respondents. Both benchmarks use all Afrobarometer respondents matched to the 14 elections.

Figure D.1: Distribution of the running variable



Notes: Panel (a) shows the number of group cells by the margin of the group’s own candidate. Cells to the right of the cutoff belong to the winner’s group, cells to the left to the runner-up’s group; the dotted lines mark a 20-point vote-share gap. Panel (b) shows the local polynomial density estimate on each side of the cutoff with 95 percent confidence intervals (Cattaneo et al. 2020).

differences are significant. Between the transition and continuity subsamples, the difference in civic attendance remains significant, while the difference in the intention to vote does not. Between the first and later survey rounds, the differences in trust in government-side institutions, preference for democracy, and the intention to vote remain significant, while the difference in national identification does not. For the intention to vote, the bootstrap distribution is skewed, reflecting the narrow bandwidths selected for this outcome, and a test based on a normal approximation would not reject. We therefore treat this difference with caution.

Table E.1: Differences between subsamples: bootstrap of the bias-corrected estimates

	Continuity – transition			Later – first round		
	Diff.	SE	95% CI	Diff.	SE	95% CI
Responsiveness (index)	−0.291	0.518	[−1.294, 0.722]	0.155	0.366	[−0.573, 0.840]
Trust: government side (index)	−0.914	0.639	[−2.181, 0.362]	−0.740*	0.428	[−1.590, 0.149]
Civic attendance (index)	0.859*	0.498	[−0.147, 1.796]	0.123	0.364	[−0.580, 0.867]
Own group treated unfairly	−0.152	0.578	[−1.342, 0.919]	0.925	0.609	[−0.269, 2.158]
Would vote (pp)	39.0	33.2	[−14.4, 111.3]	11.9**	7.7	[0.6, 30.0]
Extent of democracy	0.275	0.443	[−0.668, 1.106]	−0.109	0.425	[−0.811, 0.807]
Satisfaction with democracy	−0.243	0.349	[−0.981, 0.462]	−0.502	0.691	[−2.212, 0.578]
Preference for democracy	−0.354	0.414	[−1.115, 0.491]	−0.846*	0.423	[−1.687, 0.029]
Identify as national	0.004	0.480	[−1.091, 0.778]	−0.745	0.560	[−1.977, 0.167]

Notes: Difference between the bias-corrected regression discontinuity estimates of the two subsamples in Tables 7 and 8. Each subsample is estimated as in those tables and keeps the bandwidths selected there. Standard errors and 95 percent confidence intervals come from a bootstrap with 999 draws that resamples races with replacement, so that a race contributing cells to both subsamples enters both or neither; the interval is the 2.5th to 97.5th percentile of the bootstrap distribution, and stars refer to the corresponding percentile test. Indices are standardized on members of the runner-up’s group; single items are expressed in standard deviations of the same group, except would vote, which is in percentage points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.