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Whose Democracy? Collective Winners and Losers in Africa

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Abstract

Research on losers' consent asks whether citizens' own party won. In ethnically divided party systems, winning and losing are also shared within groups. Does a group's position relative to the party in power shape how its members judge democracy, even when they have no party ties of their own? I measure ethnic groups' attachment to governing parties in 174 Afrobarometer surveys from 35 African countries and follow the same groups over time. Members of more aligned groups are more satisfied with democracy and trust electoral commissions more. The relationship is strongest among the half of respondents with no party attachment, whom standard winner–loser measures miss. It holds net of coethnicity with the president and formal inclusion in power, and it survives comparisons within regions and group-specific time trends. After turnovers, groups that gain alignment trust the electoral commission more, while groups tied to a losing challenger do not gain. Alignment is linked to perceptions and to trust in incumbent-linked institutions, not to measured local services or support for democracy as a form of government. The causal evidence is suggestive rather than conclusive. Winning and losing have a collective side: who counts as a winner depends on where one's group stands, not only on one's own vote.

Keywords: losers' consent; group position; democratic evaluations; ethnic politics; Africa; Afrobarometer

Introduction

Democracies depend on the consent of those who lose elections. Citizens who back losing parties are less satisfied with democracy than those who back winners (Anderson and Guillory 1997; Blais and Gélineau 2007). They also trust democratic institutions less (Anderson and Tverdova 2001; Anderson et al. 2005). The gap is clear in Africa, where losers trust their institutions less and are more likely to doubt that elections were fair (Moehler 2009). In almost all of this research, winning and losing belong to individuals. A citizen wins if the party she supports wins.

Kenya's 2007 election shows what this view misses. About six months after the vote, 67 percent of Kikuyu respondents told Gallup that the election had been honest. Among Luo respondents, the figure was 1 percent, and among Luhya respondents, 3 percent (Rheault and Tortora 2008). The election, the count and the electoral commission were the same for everyone. What differed was whose group had won. Kibaki, the declared winner, drew his core support from the Kikuyu, while most Luo and Luhya voters backed Odinga. Kenyans have a phrase for a new government: it is a community's "turn to eat" (Wrong 2009).

The individual view overlooks an important feature of ethnically divided party systems. Parties often draw their leaders, candidates, and core supporters disproportionately from particular ethnic groups. These group-party ties are visible through candidates, local brokers, campaign appeals, and communities' political histories. As a result, citizens may belong to groups that are central to the governing party's support base or largely excluded from it, regardless of their own partisan attachment. About half of the Afrobarometer respondents in this study report no party attachment at all. If citizens interpret politics partly through these group-party ties, then winning and losing are not only individual experiences. They are also shared within groups. So an

election is about more than the fate of a citizen's own party. It also decides where her group stands in relation to power.

The idea that ethnic inclusion in power shapes political attitudes is not new (Flesken and Hartl 2020; García-Rivero and Clari 2023; Ruiz-Rufino 2013). Research also shows that citizens respond to ethnic representation in different ways (O'Brochta 2023). Most of this work compares groups that differ in their access to power, often using expert codings of inclusion. Such comparisons leave two questions open. Do a group's evaluations change when its position changes? And does the effect reach members who have no party attachment of their own? This paper answers both. In doing so, it shows how a group-level political position enters individual judgments of democracy, including among citizens with no partisan ties. Its first contribution is conceptual. Winning and losing have a collective side. A group's political position can shape how its members judge those in power, even when they support no party and gain nothing material. One visible sign of that position is the group's alignment with the governing party. When a group is closely tied to the party in power, its members may view the government and its institutions more favorably, even when that alignment brings no measurable material benefit.

The second contribution is empirical. I measure each group's alignment with the governing party using the party's base of supporters in 174 Afrobarometer surveys from 35 countries, following the same groups over time. The measure captures a group's relative attachment to the governing party rather than its size or formal access to power. I then show that this relationship goes beyond an individual's own partisanship, whether the respondent shares the president's ethnicity, and the group's formal inclusion in executive power. To address reverse causation and selection, I use three increasingly demanding designs. The first fixes the party's ethnic profile before attitudes are measured. The second compares attitudes before and after thirteen changes of

government. The third compares elections in which a group's preferred challenger did and did not come to power. In robustness checks, I also compare respondents from different groups who live in the same region and were interviewed in the same survey, allowing each group its own time trend.

The main results point the same way across these designs. Groups that are more aligned with the governing party are more satisfied with democracy and place greater trust in the electoral commission. The relationship is strongest among citizens with no party attachment. Among them, belonging to the governing party's core group is associated with about a third of the gap in democratic confidence between governing-party and opposition partisans. The relationship remains when coethnicity with the president and formal inclusion in power are held constant. It also survives comparisons among respondents from different groups in the same region and survey, though it is smaller. When government changes, trust in the electoral commission shifts with changes in a group's alignment, while groups whose preferred challenger loses do not gain. Alignment affects political perceptions, judgments of electoral fairness, and trust in institutions linked to the incumbent. By contrast, it is not associated with the local services or household deprivation measured by Afrobarometer, nor with support for democracy as a form of government. This pattern is consistent with a "group lens" account of democratic evaluations rather than a simple story of material benefits. The causal evidence is suggestive rather than conclusive, and the designs do not observe the psychological mechanism directly.

From partisan winners to collective winners

Election losers are less satisfied for two reasons. They bear the policy costs of defeat, and they may doubt that the process was fair (Anderson et al. 2005; Mauk 2022; Nadeau and Blais 1993).

Repeated losses can also weaken political trust (Kern and Kölln 2022). Much of this gap appears to be psychological. It emerges soon after election results are known (Blais and Gélineau 2007). It remains when policy distance from the government is taken into account, and it depends on voters' past experience of winning and losing (Curini, Jou and Memoli 2012). What counts as winning also matters: being on the side that forms the government matters most (Singh, Karakoç and Blais 2012). Recent work shows that even citizens who strongly support democracy can judge election outcomes through a partisan lens (Blais, Bol and Plescia 2026).

In ethnically divided party systems, however, citizens may experience winning and losing through their group as well as through their own party attachment. Ethnic groups are visible and relatively stable, and parties often build support within particular groups (Chandra 2004; Müller-Crepon and Bormann 2026; Posner 2005). I call a group a collective winner when the party most identified with it controls the executive. I call it a collective loser when the executive is held by a party identified with other groups. Collective political alignment is the extent to which a group is tied to the party in power. It is continuous. "Collective winners" and "collective losers" refer to the two ends of that continuum rather than to fixed categories. A citizen can be a collective winner without a party attachment, without voting and without any personal gain.

Alignment is related to, but distinct from, other ways of thinking about ethnic inclusion in power. These include sharing the president's ethnicity, holding cabinet positions, and having formal access to power as coded by experts (Cederman, Wimmer and Min 2010). A group may be strongly tied to the governing party without receiving cabinet posts or patronage. Another group may hold formal positions even when few of its members support the governing party. I treat

alignment as one visible sign of a group's political position, not as a direct measure of political status or access.

Group alignment shapes evaluations

A material explanation is that aligned groups are more satisfied because they receive greater benefits from those in power. African leaders sometimes favor their home regions and coethnics (De Luca et al. 2018; Franck and Rainer 2012; Hodler and Raschky 2014). Expectations about future benefits can also shape how coethnic voters judge performance (Carlson 2015). But such favoritism is uneven. It varies across types of goods (Kramon and Posner 2013), and it can disappear under democracy (Burgess et al. 2015). Leaders may even tax their home regions more heavily (Kasara 2007). The benefits of political inclusion may also flow mainly to elites (Arriola 2009; Francois, Rainer and Trebbi 2015). For most members of an aligned group, any material benefit is likely to be limited, indirect, or only anticipated.

The group-lens account offers a different explanation. It has three steps. Citizens must see where their group stands. They must feel their group's gains and losses as partly their own. And this feeling must color how they judge the government and its institutions.

The first step is seeing the tie. Parties in ethnically divided societies acquire ethnic images. Voters read these images from the identity of party leaders and candidates, from where parties campaign and from who turns out for them (Chandra 2004; Ferree 2010; Horowitz 1985). Where other information is scarce, such cues carry much of the weight in voters' judgments (Conroy-Krutz 2013). Citizens do not need to know how much their group supports the governing party. They need only a rough sense of whether the party in power is theirs. I show below that

perceptions of whether one's group is treated fairly by the government move with alignment, even among citizens with no party attachment.

The second step is feeling the result. Social identity theory holds that people draw part of their self-worth from the groups they belong to (Tajfel and Turner 1979). When a group identity is salient, people feel emotions on behalf of the group, even when nothing is at stake for them personally (Mackie, Devos and Smith 2000). Partisans react to their party's wins and losses in this way (Huddy, Mason and Aarøe 2015). Members of ethnic groups can react to their group's place in government in the same way. Research on African American politics calls this sense of a shared stake "linked fate" (Dawson 1994). Horowitz (1985) places it at the center of ethnic politics: being in or out of government is a public statement about a group's worth.

The third step is judging through the group. Theories of group position hold that attitudes rest on a sense of where one's group stands relative to others (Blumer 1958). Political attachments shape how people process political information and judge the economy and the government (Bartels 2002; Taber and Lodge 2006). Similar patterns appear in African democracies, both for partisan ties (Carlson 2016) and for ethnic ties (Long and Gibson 2015). A group tie can work in the same way. Members of collective winners give the government and its institutions the benefit of the doubt. Members of collective losers withhold it.

The idea is not limited to Africa or to ethnic parties. In the United States, Pew asked people whether they trusted the government in Washington to do what is right always or most of the time. After Hurricane Katrina in 2005, 12 percent of Black Americans said they did, against 32 percent of white Americans. In 2010, after Barack Obama took office, the figures were 37 and 20 percent (Pew Research Center 2010). Earlier work found that Black citizens in cities with Black

mayors were more politically engaged than Black citizens elsewhere (Bobo and Gilliam 1990). In both cases, a group's place in power changed how its members related to the political system. The argument here is that such effects should be strongest where parties are tied to ethnic groups and control of the executive decides which groups are in power.

Observable implications

The argument produces three sets of observable implications.

1. Specific rather than diffuse support. Alignment should increase satisfaction with how democracy works and trust in the electoral commission. It should not increase support for democracy as a form of government (Easton 1975). Partisan advantage and commitment to democratic principles need not move together (Cohen et al. 2023; Graham and Svolik 2020).
2. Proximity to power rather than individual partisanship or coethnicity. If alignment matters because it captures a group's position relative to those who govern, its effect should extend beyond individuals' own party attachments, including among nonpartisans. It should also remain when coethnicity with the president and formal inclusion in executive power are held constant. When a change of government alters a group's alignment, attitudes should change with it. By contrast, groups whose preferred party loses should not become more satisfied, and alignment with parties outside government should not improve evaluations. Presidents build coalitions beyond their own ethnic group (Arriola 2009), and party networks often outlast individual leaders. For this reason, a change of leader within the same governing party should matter less than a change in the party that controls government.

3. An evaluative lens rather than material delivery. If alignment mainly shapes how citizens interpret politics, it should have little effect on concrete local conditions. Its effects should instead appear most clearly in judgments that allow greater room for interpretation. Alignment should affect assessments of the national economy more than reports of personal living conditions, increase trust most strongly in institutions associated with the incumbent, and reduce trust in the opposition. And if members feel their group's position as partly their own, alignment should matter more for people who identify with their ethnic group at least as strongly as with the nation.

The argument should apply most strongly where party competition follows ethnic lines and the executive dominates political life. Both conditions are common in African democracies. They are less common in many democracies elsewhere.

Data and measurement

I use respondent-level data from Afrobarometer Rounds 3 to 10. Each respondent reports an ethnic group and whether she feels close to a political party, and if so which one. I standardized ethnic labels within each country so that the same group is identified consistently across rounds. The analysis includes every survey with party-by-group data, an identifiable governing party, and a usable ethnic group question. The final sample contains 244,374 respondents in 174 surveys from 35 countries, fielded between 2005 and 2025. These respondents belong to 961 country-groups. Online Resource A documents how the sample was constructed from 226 country-survey rounds, along with the ethnic group coding and the size of group-by-survey cells. Groups are the ethnic categories respondents report. As a check, I also aggregate these categories into the politically relevant groups of the Ethnic Power Relations data (Vogt et al. 2015), linked

through the language tree (Müller-Crepon, Pengl and Bormann 2022). The results are very similar (Online Resource B5).

I define the governing party as the party of the head of government at the time of fieldwork. In presidential and semi-presidential systems, this is the president's party. Where the prime minister heads the government, as in Mauritius, Morocco, and Ethiopia, I use the prime minister's party. I coded the governing party using the survey fieldwork dates and checked each case against WhoGov (Nyrup and Bramwell 2020). In ten surveys from Benin, Mali, and The Gambia, the president was elected as an independent or as the candidate of an alliance, so I use the president's political movement or the main party in the presidential alliance. Surveys conducted under military or transitional governments are excluded. Online Resource A documents these coding decisions.

Alignment with the governing party

For each survey, let G mean membership in an ethnic group, and D be attachment to the governing party. Let $p = Pr(G \mid D)$ be the group's share of governing-party supporters, and let $s = Pr(G)$ be its share of all respondents. By Bayes' rule,

$$p / s = Pr(D \mid G) / Pr(D).$$

The ratio compares the group's rate of attachment to the governing party with the party's attachment rate in the country as a whole. A value of one means that members of the group are attached to the governing party at the national rate. Values below one indicate weaker attachment, approaching zero when almost no members of the group are attached to the party. Alignment is thus a measure of relative group-level partisan attachment. It does not directly

measure political status, representation, or access to power. The argument is that this group-level tie to the party in power can shape individuals' political evaluations beyond their own partisan attachment.

Small groups can produce extreme ratios simply because they contain few respondents. To reduce this noise, I shrink p toward s using a prior equivalent to ten respondents and then take the natural logarithm. The median group-by-survey cell contains 29 respondents. Because larger groups account for more respondents, the typical respondent belongs to a much larger cell. The median respondent is in a cell with 205 respondents, and only 1.3 percent of respondents are in cells smaller than ten. Governing parties have a median of 349 supporters per survey. Alignment has a standard deviation of 0.62 across respondents and 0.39 within groups over time. Its split-half reliability is 0.82 across cells and 0.69 within groups over time (Online Resource B).

Measuring alignment in the same survey as the outcomes creates two concerns. First, political attitudes may themselves affect reported party attachment. Members of a group who are dissatisfied with the government, for example, may become less likely to say that they feel close to the governing party. Second, each respondent's own party attachment contributes to the alignment score assigned to that respondent. I address the second concern with two alternative measures. A leave-one-out measure removes each respondent's own answer, while cross-fitting constructs alignment from one half of the sample and assigns it to the other half. To address the first concern, I use a "frozen" measure that constructs alignment from the governing party's ethnic profile in the previous Afrobarometer survey round. When a party was renamed or merged, I use its direct predecessor. Frozen alignment is available for 174,139 respondents in

133 survey rounds from 30 countries. Nine percent of these respondents rely on a hand-coded predecessor party (Online Resource A).

Outcomes and other measures

Satisfaction with democracy is measured on a four-point scale, while trust in the electoral commission ranges from zero to three. The correlation between the two is 0.37. I combine them into a democratic confidence index by taking the mean of the two standardized items. The index captures evaluations of how the system performs. It is closer to what Easton (1975) calls specific support than to diffuse support for democracy as a form of government. I examine two additional outcomes. The first is whether respondents believe their ethnic group is treated unfairly by the government, an item available from Round 5 onward. The second is whether respondents believe democracy is preferable to any other form of government. The models control for age, gender, education, and urban residence. Online Resource B reports descriptive statistics and alternative versions of the democratic confidence index.

To distinguish group alignment from ethnic ties to the political leader, I coded the ethnic group of the head of the executive in each survey. The coding is rated as high confidence in 142 surveys, medium confidence in 29, and low confidence in 3. Respondents who share the leader's ethnic group make up 22 percent of the sample. To examine possible mechanisms, I also use measures of:

• the availability of six local services and a lived poverty index;

• evaluations of the national economy and respondents' own living conditions;

• perceptions of whether the last election was free and fair and how democratic the country is;

• trust in nine political institutions;

• national versus ethnic identification.

Research design

The baseline model regresses each outcome on alignment. It includes survey fixed effects, which absorb factors shared by all respondents in a country at a given time, and country-by-group fixed effects, which absorb stable differences between ethnic groups. The estimate comes from changes within the same groups over time. Standard errors are clustered by country, and I report wild cluster bootstrap p-values using Webb weights with the null imposed. I treat the same-survey estimates as descriptive since alignment and attitudes are measured at the same time. The frozen measure provides a stronger test by measuring the party's ethnic profile before attitudes are observed. Two further checks address shocks that hit particular places or groups. The first replaces survey fixed effects with region-by-survey fixed effects. Members of different groups are then compared with respondents interviewed in the same region and survey, which absorbs regional favoritism, local economic shocks and local services. The second gives each group its own linear time trend, so that slow changes in a group's fortunes cannot drive the estimate.

Turnover change design. The second design focuses on changes in the governing party. It requires that the incoming party be observed in the survey round before the turnover. For each group, I calculate the change in alignment that would result from the change in government:

$\Delta A_{ge} = A_{ge}^{in} - A_{ge}^{out}$.

Here, A_{ge}^{in} is group g's alignment with the incoming party before turnover event e, and A_{ge}^{out} is its alignment with the outgoing party, also measured before the turnover. I then estimate:

$$\Delta Y_{ge} = \beta \Delta A_{ge} + \lambda_e + \varepsilon_{ge}.$$

Here, ΔY_{ge} is the change in group g's average outcome from the survey before event e to the survey after it, and λ_e is an event fixed effect. The coefficient β captures whether groups that become more aligned with the new governing party also become more positive in their evaluations.

As a placebo test, I examine the change from two surveys before the turnover to the survey immediately before it. This allows me to see whether the same relationship was already present before power changed hands. The design is a before-and-after comparison with one earlier period, rather than an event study with multiple leads and lags. Thirteen turnovers in ten countries meet the requirements for this analysis.

Election-pair comparison. Changes of government are not random. Groups tied to an opposition party may become more satisfied for reasons that also make that party more likely to win. To address this problem, I use every pair of consecutive surveys with an executive election between them. For each group, I calculate a potential alignment shift,

$$P_{gk} = A_{gk}^{challenger} - A_{gk}^{in}cumbent,$$

where both alignment measures come from the survey before election pair k. The quantity P_{gk} captures how much a group's alignment would change if the challenger replaced the incumbent party in power. I then estimate:

$$\Delta Y_{gk} = \beta_R(P_{gk} \times T_k) + \beta_P P_{gk} + \mu_k + \varepsilon_{gk}.$$

Here, T_k equals one when the election produces a turnover, and μ_k is an election-pair fixed effect. When power changes hands, the challenger is the party that takes power. When the incumbent remains in office, the challenger is the largest opposition party among supporters in the earlier survey. The coefficient β_P captures whether groups more closely tied to the challenger change even when that challenger loses. The coefficient β_R captures the additional change when the challenger actually wins and becomes the governing party.

The key assumption is that, in the absence of turnover, groups with similar potential alignment shifts would have changed in similar ways. This is a continuous difference-in-differences assumption, and the design does not guarantee that it holds. I examine it in five ways. First, I test for similar changes before the election. Second, I restrict the sample to elections decided by fewer than ten percentage points. Third, I keep only groups whose potential shifts overlap across turnover and non-turnover elections. Fourth, I compare the challenger coding with official election results. Fifth, I adjust for changes in group composition. The close-election restriction reduces comparisons across very different electoral settings, but it includes only 18 election pairs and does not create random assignment. The full analysis contains 67 election pairs, 13 of which produced a turnover. Online Resource A reports the dates, parties, winner, runner-up, and margin for each election pair.

Both event designs compare group averages across different survey samples. As a result, the respondents representing a group can differ from one round to the next. I address this by estimating both designs with group averages adjusted for demographic composition and by controlling directly for changes in group composition. Because the number of country clusters is

relatively small, I report wild cluster bootstrap p-values for all event estimates. Two additional checks use permutation tests that reshuffle treatment across groups or elections. These tests rely on the assumption that the reshuffled units are exchangeable; they do not create or assume random assignment.

I used an AI tool (Claude, Anthropic) to help review the analysis code, check that the results reproduce from the replication files, and edit the text. I checked all analyses and am responsible for the content.

Results

Table 1 reports the within-group estimates. A one-unit increase in log alignment is roughly equivalent to moving from a group whose attachment to the governing party matches the national rate to one whose attachment rate is 2.7 times as high. This increase is associated with a 0.18 standard-deviation rise in the democratic confidence index, a 0.17-point increase in satisfaction with democracy, and a 0.21-point increase in trust in the electoral commission. It is also associated with a 0.10-point decline in the belief that one's group is treated unfairly. This item checks the first step of the argument. If people did not perceive where their group stands, alignment should not change how they see the government's treatment of their group. It does, and the decline holds among nonpartisans (−0.07 with same-survey alignment and −0.05 with frozen alignment; both $p < .01$). By contrast, alignment has essentially no relationship with support for democracy as a form of government.

Table 1. Group alignment with the governing party and democratic evaluations

	Democratic confidence index	Satisfaction with democracy	Trust in electoral commission	Group treated unfairly	Democracy preferable
Panel A. Same-survey alignment					
Same-survey alignment	0.179***	0.169***	0.209***	−0.099***	0.003
	(0.023)	(0.023)	(0.027)	(0.018)	(0.006)
Leave-one-out alignment	0.148***	0.140***	0.172***	−0.091***	0.002
	(0.026)	(0.025)	(0.030)	(0.018)	(0.006)
Respondents	200,372	211,799	210,041	177,403	211,658
Panel B. Frozen alignment					
Frozen alignment (previous survey)	0.103***	0.107***	0.112***	−0.068***	0.005
	(0.024)	(0.024)	(0.030)	(0.020)	(0.006)
Wild cluster bootstrap p	.007	.004	.010	.017	.441
Same-survey alignment, frozen sample	0.171***	0.160***	0.202***	−0.107***	−0.003
	(0.021)	(0.024)	(0.023)	(0.020)	(0.009)
Frozen, holding same-survey alignment constant	0.052***	0.062***	0.048***	−0.034***	0.007
	(0.012)	(0.013)	(0.015)	(0.012)	(0.004)
Respondents (frozen sample)	142,910	149,765	148,639	139,079	149,360

Note. OLS with survey and country-by-group fixed effects and controls for age, gender, education, and urban residence. Standard errors are clustered by country (35 countries; 30 in the frozen sample). The leave-one-out measure removes each respondent's own answer before alignment is computed; its samples are at most 0.1 percent smaller. Wild cluster bootstrap p-values use Webb weights, 9,999 replications, with the null imposed; $p < .001$ for every same-survey and leave-one-out estimate except democracy preferable ($p = .653$ and .726). * $p < .1$, ** $p < .05$, *** $p < .01$. In this table and in the respondent models of Tables 2, 3 and 6, stars use clustered standard errors. In the event designs, which have fewer clusters, stars use wild cluster bootstrap p-values.

Removing each respondent's own answer from the alignment measure reduces the estimate for the democratic confidence index from 0.18 to 0.15, or by about one sixth. Cross-fitting provides a more demanding check by constructing alignment from one random half of each survey and assigning it to the other half. Across twenty splits, the average estimate is 0.11, with estimates ranging from 0.107 to 0.117. Because the half-sample measure is less reliable, some attenuation is expected. All these checks suggest that the same-survey relationship is not driven simply by respondents contributing mechanically to the alignment score assigned to them.

The positive relationship remains under alternative choices about shrinkage and cell size. The estimate is 0.16 with a prior of five respondents, 0.20 with a prior of twenty, and 0.25 with an empirical Bayes prior. When cells with fewer than 10, 20, 30, or 50 respondents are excluded,

the estimates range from 0.20 to 0.25. Across priors between 1 and 50, weaker priors produce smaller estimates, as expected when the measure becomes noisier. The unshrunk ratio, which is defined only for cells with governing-party supporters, gives an estimate of 0.21 (Online Resource B).

Figure 1 and Panel B of Table 1 use frozen alignment, in which the governing party's ethnic profile is measured before the outcome survey. A one-unit increase in frozen alignment is associated with a 0.10 standard-deviation increase in the democratic confidence index (wild bootstrap $p = .007$). This is between one half and two thirds of the same-survey estimate in the same sample. The smaller coefficient is consistent with some feedback from attitudes to party attachment, while the remaining association shows that the relationship persists when the party profile is fixed in advance. Results are similar with logit models for the ordered outcomes, survey weights, coalition-based alignment, and the exclusions reported in Online Resource C.

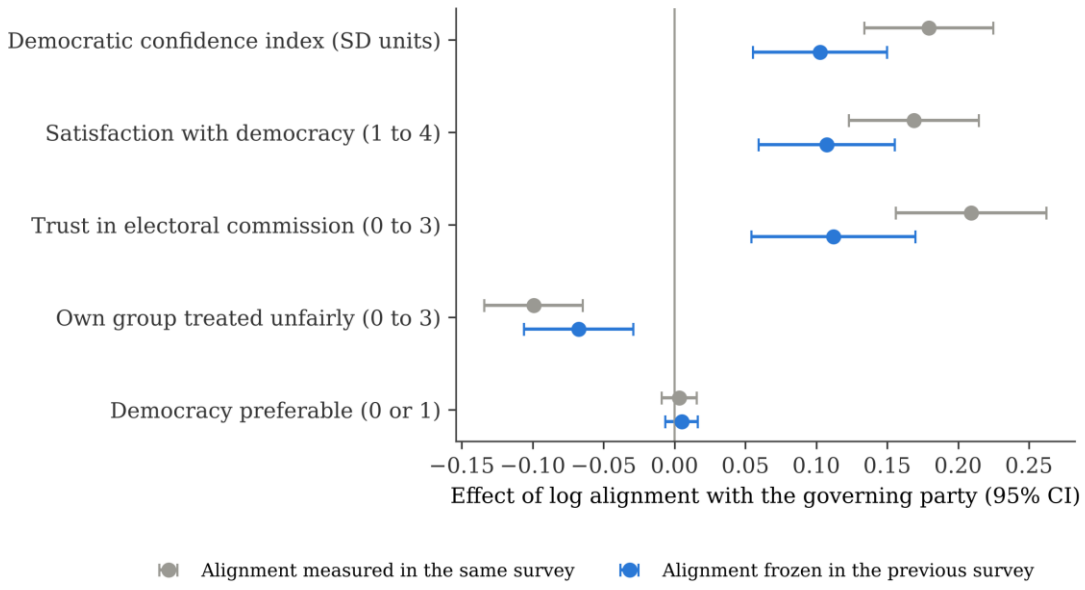


Figure 1. Effects of alignment with the governing party on democratic evaluations. Grey markers use alignment measured in the same survey; blue markers use the governing party's ethnic profile from the previous survey. Survey and country-by-group fixed effects, 95 percent confidence intervals, standard errors clustered by country.

Robustness: comparisons within regions and group trends

Table 2 shows that the relationship survives two demanding checks (Online Resource C5 to C7). The first compares respondents from different groups who live in the same region and were interviewed in the same survey. Region-by-survey fixed effects absorb regional favoritism, local economic shocks and differences in local services. The estimate falls from 0.18 to 0.10 but remains statistically significant (wild bootstrap $p < .001$). It also remains among nonpartisans (0.05, $p = .009$) and in ethnically mixed regions, where group and region do not coincide (0.09, $p < .001$).

The second check gives each group its own linear time trend. The same-survey estimate changes little (0.15, $p < .001$), including with region-by-survey fixed effects (0.09, $p < .001$). The

estimate with party profiles frozen in advance is more fragile. It falls to 0.06 under group trends and is no longer statistically significant, although its confidence interval includes the baseline estimate. For this reason, I treat the frozen estimate as supporting evidence on timing rather than as decisive evidence.

Table 2. Robustness: comparisons within regions and group-specific trends

Democratic confidence index	All respondents, same-survey alignment	Nonpartisans, same-survey alignment	All respondents, frozen alignment
Survey and group fixed effects	0.182*** (0.023) [<.001]	0.117*** (0.018) [<.001]	0.103*** (0.024) [.007]
Region-by-survey and group fixed effects	0.097*** (0.018) [<.001]	0.046*** (0.015) [.009]	0.042** (0.016) [.035]
Region-by-survey and group fixed effects, mixed regions only	0.094*** (0.022) [<.001]	0.045** (0.019) [.036]	0.037** (0.018) [.072]
Survey fixed effects and group-specific trends	0.154*** (0.023) [<.001]	0.093*** (0.020) [<.001]	0.056 (0.037) [.235]
Region-by-survey fixed effects and group-specific trends	0.089*** (0.017) [<.001]	—	0.021 (0.021) [.463]
Respondents, region-by-survey row	198,951	96,374	142,522

Note. Each cell is the alignment coefficient from a separate model with controls for age, gender, education and urban residence and country-by-group fixed effects. Rows 1 to 3 use respondents whose survey records a region; rows 4 and 5 use groups observed in three or more surveys. Mixed regions have ethnic fractionalization above the median (0.61). — not estimated. Standard errors clustered by country in parentheses; stars use them (* p < .1, ** p < .05, *** p < .01). Wild cluster bootstrap p-values in brackets (Webb weights, 9,999 replications, null imposed).

Collective position beyond individual partisanship, coethnicity and formal inclusion

Alignment is a group-level partisan tie, but the key question is whether it matters beyond a person's own partisanship. Figure 2 estimates the relationship separately for three groups of respondents. Thirty percent feel close to the governing party, 20 percent to another party, and 50 percent report no party attachment.

The relationship is strongest among nonpartisans. For them, a one-unit increase in alignment is associated with a 0.12 standard-deviation increase in the democratic confidence index using

same-survey alignment and a 0.08 increase using frozen alignment (both $p < .001$). Among supporters of other parties, the estimates are 0.09 with same-survey alignment ($p = .003$) and 0.04 with frozen alignment ($p = .14$). The relationship is smallest among governing-party supporters, at 0.05 and 0.03. These respondents are already personal winners, and the effect is significantly smaller for them than for nonpartisans in both specifications. The collective dimension appears strongest among people whom the usual partisan framework would not classify as winners or losers. A similar pattern appears among opposition supporters, although the frozen estimate is less precise.

A simpler comparison shows how large the collective effect is. The governing party's core group is the ethnic group that supplies the largest share of its supporters. Among nonpartisans, members of a group are 0.16 standard deviations more confident in democracy when their group is the governing party's core group than when it is not (wild bootstrap $p < .001$). With the core group fixed in the previous survey, the difference is 0.09 ($p = .012$). Both comparisons are within groups over time, so they come from groups that move into or out of the core. Among all respondents, the gap between governing-party partisans and partisans of other parties is 0.47. Being a collective winner without any party attachment is thus worth about a third of the individual partisan gap, and about a fifth with the frozen measure (Online Resource E5).

One concern is that some respondents who report no party attachment may still lean toward the governing party. Afrobarometer also asks which party's candidate respondents would support in a presidential election held the next day. Among nonpartisans, 23 percent name the governing party, 21 percent name another party, 16 percent say they would not vote, and 40 percent do not know or refuse to answer. If quiet support for the governing party were driving the result, the

relationship should be strongest among the first group. It is not. Among nonpartisans who would not vote for the governing party, the estimates are 0.09 with same-survey alignment and 0.05 with frozen alignment (wild bootstrap $p < .001$ and $p = .012$). The relationship also remains among nonpartisans who would vote for another party, at 0.07 and 0.04. Among those who would vote for the governing party, the estimates are no larger, at 0.08 and 0.05. Because vote intention is measured in the same survey, this comparison conditions on a response that alignment itself may influence. I treat this as a supporting check rather than a clean causal test (Online Resource E2).

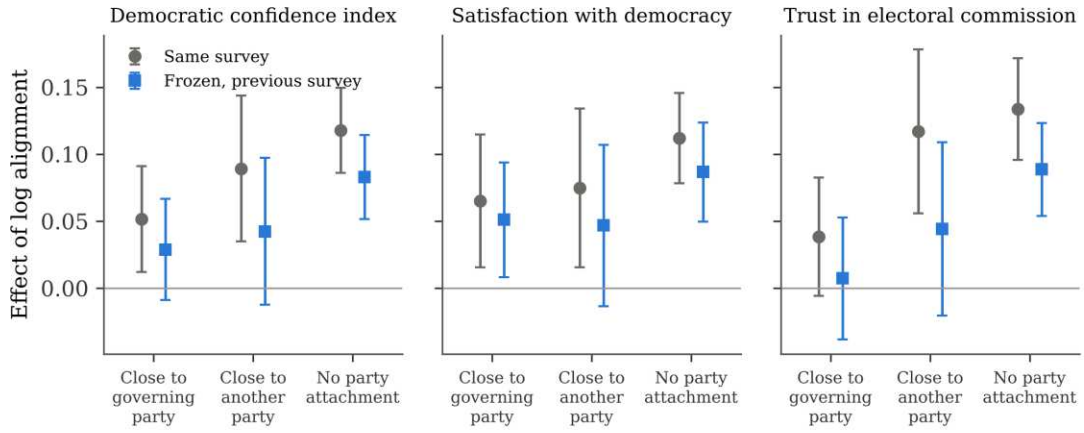


Figure 2. Effect of group alignment by the respondent's own partisan status. Slopes are estimated in one model per outcome with survey and country-by-group fixed effects, partisan status indicators and controls. 95 percent confidence intervals, standard errors clustered by country.

Table 3 compares group alignment with coethnicity with the head of the executive. On its own, sharing the leader's ethnicity is associated with a 0.17 standard-deviation increase in the democratic confidence index within groups over time. When alignment and coethnicity are included together, the alignment estimate changes very little, from 0.18 to 0.17. The coethnicity

estimate falls to 0.09. Frozen alignment remains largely unchanged, declining only from 0.10 to 0.09. It continues to predict evaluations even among groups that do not share the leader's ethnicity.

Changes in political leadership provide a stronger comparison. At party turnovers, the effect of the alignment shift remains substantial when changes in coethnic status are held constant. I also examine changes of leader that occur without a change in the governing party. There are eighteen such cases, seventeen in the democratic confidence sample, including Mbeki to Zuma in South Africa and Kikwete to Magufuli in Tanzania. In these cases, gaining or losing a coethnic leader is associated with only a 0.04 standard-deviation change in the index, an estimate that is not statistically distinguishable from zero. A combined model shows that changes in coethnic status matter considerably more when they coincide with a change in the governing party. The difference is 0.29 standard deviations (wild bootstrap $p = .002$). These results suggest that which party holds power matters more than a change in the ethnic identity of the leader alone.

Table 3. Alignment with the governing party and coethnicity with the head of the executive

	Democratic confidence index	Satisfaction with democracy	Trust in electoral commission
Respondent models			
Coethnic with head of executive, alone	0.172*** (0.048)	0.171*** (0.041)	0.203*** (0.060)
Log alignment, holding coethnicity constant	0.169*** (0.022)	0.157*** (0.023)	0.196*** (0.026)
Coethnic, holding alignment constant	0.090*** (0.029)	0.098*** (0.025)	0.107*** (0.037)
Frozen alignment, holding coethnicity constant	0.092*** (0.023)	0.096*** (0.024)	0.099*** (0.027)
Frozen alignment, groups not coethnic with the leader	0.068*** (0.024)	0.075** (0.029)	0.069*** (0.025)
Party turnovers (13 events)			
Alignment shift, holding change in coethnicity constant	0.108** (0.023)	0.084* (0.026)	0.144*** (0.025)
Change in coethnicity, holding alignment shift constant	0.170** (0.051)	0.164** (0.045)	0.200** (0.092)
Leader changes within a continuing party (17 events; 18 for satisfaction)			
Change in coethnicity with the leader	0.041 (0.032)	0.054 (0.036)	0.045 (0.037)
Difference in coethnicity effect, party turnover minus leader change	0.291*** (0.065)	0.236*** (0.052)	0.369** (0.108)
Wild cluster bootstrap p for the difference	.002	.003	.013

Note. Respondent models as in Table 1. Event models regress the change in group means on the listed variables with event fixed effects, weighted by group size, standard errors clustered by country. The difference is the interaction of the change in coethnic status with a party-turnover indicator in a stacked model of both kinds of events. Stars in the event rows use wild cluster bootstrap p-values (Webb weights, 9,999 replications, null imposed). Strict coding of the leader's group; results are the same with the broad coding and high-confidence codings only.

Alignment is also distinct from formal inclusion in power. I link the Afrobarometer categories to the politically relevant groups of the Ethnic Power Relations (EPR) data through the language tree (Müller-Crepon, Pengl and Bormann 2022; Vogt et al. 2015). The linked sample covers 76 percent of respondents. Alignment and EPR inclusion in executive power correlate at only 0.10. With EPR groups, alignment raises the democratic confidence index by 0.26 standard deviations with same-survey alignment and 0.17 with frozen alignment (wild bootstrap $p < .001$ and $p =$

.002). Holding EPR inclusion constant leaves these estimates almost unchanged, at 0.26 and 0.16. Inclusion has its own positive association, at 0.14 and 0.23. Expert-coded access to power and a group's tie to the governing party are related, but each matters on its own (Online Resource B5).

Alignment also appears to matter because of political power, not partisan ties in general. Holding alignment with the governing party constant, alignment with the main opposition party is associated with lower evaluations (−0.05), while alignment with a third party has only a small negative association (−0.01). Strong group-level ties to a party do not by themselves raise democratic evaluations. What matters is whether that party controls the government (Online Resource C).

Changes of government

Table 4 and Figure 3 report the turnover change design. The shift is measured with party profiles from before the change. Groups whose alignment rose at a change of government came to trust the electoral commission more. A one-unit shift raises the change in commission trust by 0.17 points (wild bootstrap p = .001). It raises the change in the democratic confidence index by 0.13 standard deviations (p = .026) and in satisfaction by 0.10 (p = .037). It lowers the change in perceived unfair treatment by 0.09 (p = .036). The average shift at a turnover is 1.27 units in absolute value. For a typical group, that is a change in the index of about 0.16 standard deviations. That is more than 40 percent of the gap between governing-party supporters and everyone else.

Table 4. Turnover change design with party profiles fixed before the turnover

	Democratic confidence index	Satisfaction with democracy	Trust in electoral commission	Group treated unfairly	Democracy preferable
Change after the turnover	0.129** (0.022)	0.103** (0.023)	0.167*** (0.024)	−0.092** (0.025)	−0.002 (0.008)
Wild cluster bootstrap p	.026	.037	.001	.036	.832
Change after, equal group weights	0.098** (0.023)	0.076* (0.019)	0.119* (0.038)	−0.052 (0.030)	0.006 (0.008)
Change after, composition-adjusted group means	0.126** (0.025)	0.109** (0.027)	0.153*** (0.028)		
Placebo, change before the turnover	0.002 (0.015)	0.042 (0.018)	−0.044 (0.026)	0.010 (0.024)	0.029*** (0.008)
Wild cluster bootstrap p (placebo)	.914	.135	.169	.672	.008
Group events	199	200	200	158	200

Note. Unit is an ethnic group in a turnover event (13 events in 10 countries; 11 events for unfair treatment). The shift is the group's log alignment with the incoming party minus its log alignment with the outgoing party, both measured in the survey before the turnover. The outcome is the change in the group mean between the surveys before and after the turnover; the placebo uses the change in the preceding period. Event fixed effects, weights for group size, standard errors clustered by country. Composition-adjusted means remove the contribution of age, gender, education, and urban residence; that row uses 172 to 173 group events in 9 countries (one country lacks covariates). Wild cluster bootstrap with Webb weights, 9,999 replications, null imposed. Stars use the wild bootstrap p-values.

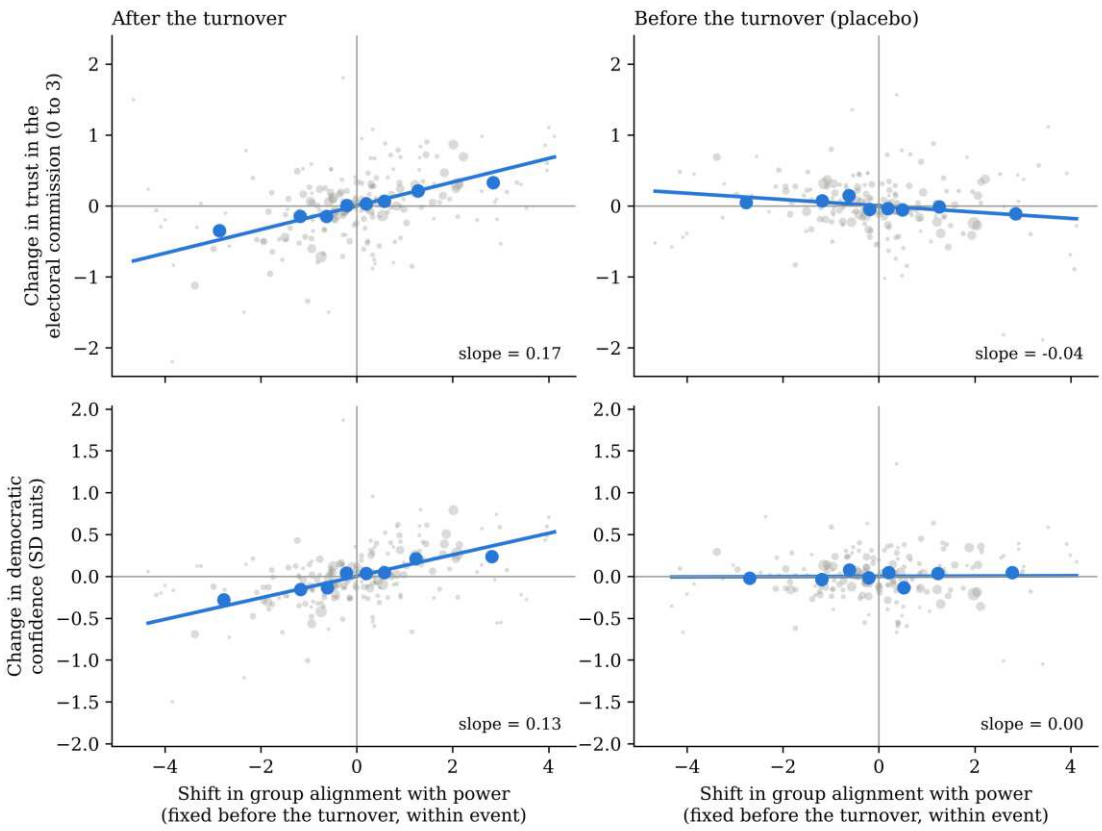


Figure 3. Change in trust in the electoral commission (top) and in the democratic confidence index (bottom) by shift in group alignment with power at changes of government. Left: changes from the survey before to the survey after the turnover. Right: changes in the preceding period. Points are groups in 13 turnover events; blue markers are binned averages and lines come from event fixed-effects regressions.

In the period before the turnover, the same shifts do not predict changes in the index or in electoral commission trust. Satisfaction alone shows a positive estimate of 0.04 in that period. This is less than half the later effect and not significant with bootstrap inference ($p = .135$). It fits some anticipation before elections but does not prove it. Twelve events have enough groups to estimate a separate effect, and ten of these are positive. The two exceptions, Mauritius 2014 and Madagascar 2018, are imprecise (Online Resource D). Adjusting for group composition leaves

the estimates almost unchanged. One placebo does stand out. Support for democracy as a form of government rose before the turnover in groups about to gain alignment (0.03, bootstrap p = .008). It did not move after the turnover (p = .83). This outcome is not part of the index. Still, it suggests that groups about to gain power were already on a slightly different path in diffuse support.

A version of the design compares groups that live in the same region at the same turnover. It keeps the sign but is less precise (0.09 against 0.12 in the same cells, p = .111; Online Resource D8).

Elections that did and did not change the party in power

Table 5 compares elections in which the challenger took power with elections in which it did not. Across all 67 election pairs, the realized shift raises trust in the electoral commission by 0.17 points (wild bootstrap p = .013). It raises the democratic confidence index by 0.12 standard deviations (p = .029). The potential shift is essentially zero (0.004). This means that groups more closely tied to the challenger did not become more positive simply because of that relationship. Their evaluations improved when the challenger actually took power. The realized shift has similar effects on trust in the president and on perceptions of whether the election was free and fair. For trust in the president, the potential shift is also slightly positive (0.03).

The results are stronger in close elections. Among elections decided by fewer than ten points, the realized shift raises the index by 0.17 standard deviations (p = .013). Groups tied more closely to a challenger that lost moved in the opposite direction. Their trust in the electoral commission fell by 0.07 points, and their assessment of election fairness fell by 0.08 points. Neither estimate is

significant under the bootstrap (p = .12 and .15), so I treat them cautiously. Their direction is still consistent with collective losers' consent.

Table 5. Election-pair comparison: elections with and without a turnover

	Democratic confidence index	Satisfaction with democracy	Trust in electoral commission	Trust in the president	Election free and fair
All pairs (67 pairs, 66 for the index and electoral-commission trust; 13 turnovers)					
Realized shift	0.125** (0.025)	0.098** (0.028)	0.166** (0.030)	0.257** (0.051)	0.223** (0.041)
Wild cluster bootstrap p	.029	.049	.013	.022	.018
Potential shift	0.004 (0.010)	0.006 (0.012)	0.001 (0.013)	0.031** (0.014)	−0.015 (0.014)
Placebo, realized shift in previous period	0.011 (0.019)	0.058* (0.023)	−0.043 (0.029)	−0.012 (0.025)	0.020 (0.034)
Wild cluster bootstrap p (placebo)	.601	.086	.208	.632	.583
Margin under 10 points (18 pairs, 17 for the index and electoral-commission trust; 8 turnovers)					
Realized shift	0.169** (0.032)	0.113* (0.035)	0.238*** (0.027)	0.331*** (0.059)	0.293*** (0.041)
Wild cluster bootstrap p	.013	.069	.008	.010	.004
Potential shift	−0.026 (0.021)	0.013 (0.026)	−0.075 (0.019)	0.012 (0.032)	−0.078 (0.023)
Placebo, realized shift in previous period	0.025 (0.040)	0.087** (0.044)	−0.034 (0.057)	−0.020 (0.036)	0.013 (0.049)
Wild cluster bootstrap p (placebo)	.564	.039	.731	.625	.805

Note. The unit is an ethnic group observed across a pair of consecutive surveys with an executive election between them. The potential shift is the group's log alignment with the challenger minus its alignment with the governing party, both measured in the earlier survey. The realized shift equals this potential shift when the challenger takes power and zero otherwise. The outcome is the change in the group mean between the two surveys. Models include election-pair fixed effects and weights for group size. Standard errors are clustered by country. The placebo models use the change from the preceding survey to the first survey in each pair. Stars are based on wild cluster bootstrap p-values using Webb weights, 9,999 replications, and the null imposed.

The evidence from the earlier period differs across outcomes. The democratic confidence index, trust in the electoral commission, trust in the president, and perceptions of election fairness show no similar movement before the election. Satisfaction with democracy is different. It rises modestly in the earlier period, with an estimate of 0.06 across all election pairs and 0.09 in close elections (bootstrap p = .039). Most of this movement comes from cases in which the earlier survey was conducted within a year of the election, which is consistent with some anticipation.

I also subtract the earlier change from the change after the election. This reduces the estimate for satisfaction with democracy to 0.06 (bootstrap p = .13), while the estimates for the democratic confidence index (0.13) and trust in the electoral commission (0.22) remain positive (Online Resource D). For this reason, I place more weight on these two outcomes in the turnover and election-pair analyses and report the satisfaction result with this qualification.

The comparison survives several checks on how the election pairs are constructed (Online Resource D). Turnover and non-turnover pairs overlap substantially in their potential alignment shifts. Restricting the analysis to this common range retains 94 percent of the group-pair observations and gives an estimate of 0.15. Reweighting turnover and non-turnover pairs to the same distribution of potential shifts gives 0.13.

I also checked how the challenger was identified. In multiparty systems, identifying a single main opposition party can involve judgment. The coded challenger matches the actual runner-up in 30 of 54 elections without a turnover. Restricting the analysis to those cases gives 0.13 (bootstrap p = .017). Recoding the challenger as the actual runner-up, when that party appears in the survey, gives 0.14 (p = .018). Adjusting group averages for demographic composition gives 0.12 across all election pairs and 0.18 in close elections. The close-election estimate is also 0.18

when the sample is limited to margins below five points, although only three turnovers meet that threshold. At wider margins, the estimate is 0.13 to 0.14.

How collective alignment works

Table 6 and Figure 4 test the implications that separate an evaluative lens from a simple material account. These outcomes help tell the two accounts apart. They are not proven mediators. They are measured at the same time as satisfaction and trust, so they cannot show that alignment works through them.

Within groups, alignment is unrelated to the services in respondents' areas, whether alignment is measured in the same survey or frozen. Groups that gain alignment at a turnover see no change in local services or household deprivation. Aligned groups report slightly less lived poverty with same-survey alignment. But the difference is only 0.02 standard deviations, and it disappears with frozen alignment and at turnovers.

Perceptions move where these concrete conditions do not. Alignment raises ratings of the national economy by 0.17 standard deviations and of one's own living conditions by 0.12. Within the same respondents, the gap between the two is clear (0.06 with same-survey alignment, 0.03 frozen). Alignment also raises the perceived fairness of the last election and the perceived extent of democracy. Trust follows a gradient. Effects are largest for the president and the ruling party. They are smaller for the electoral commission and parliament, and much smaller for the courts, police, army, and traditional leaders. Trust in opposition parties falls. National versus ethnic identification does not change.

A further test speaks to the feeling step. Because alignment does not move national versus ethnic identification, that item can serve as a moderator. If members feel their group's position as their

own, alignment should matter more for the 57 percent who identify with their ethnic group at least as strongly as with the nation. The interaction is positive in every specification, but its strength depends on the measure. With frozen alignment, the slope for the index is 0.09 among respondents who put the nation first and 0.11 among the rest. The interaction is 0.026 (wild bootstrap $p = .006$), and 0.024 among nonpartisans ($p = .022$). With same-survey alignment, the interaction is small and imprecise (0.011, $p = .35$). The evidence for this step is suggestive rather than firm (Online Resource E6, Table E5).

Table 6. Alignment, material conditions, perceptions, procedure, trust and identity

Outcome (standard deviations)	Same-survey alignment	Frozen alignment	Nonpartisans only	Turnover shift
Material conditions				
Services in the enumeration area	0.002 (0.016)	−0.005 (0.010)		−0.014 (0.022)
Lived poverty	−0.024*** (0.009)	−0.007 (0.014)	−0.024** (0.011)	0.015 (0.022)
Perceptions				
Country's economic condition	0.175*** (0.025)	0.118*** (0.032)	0.122*** (0.021)	0.154*** (0.030)
Own living conditions	0.120*** (0.028)	0.085*** (0.029)	0.080*** (0.021)	0.089*** (0.017)
Procedure				
Last election free and fair	0.211*** (0.024)	0.147*** (0.033)	0.149*** (0.021)	0.185** (0.036)
Extent of democracy	0.140*** (0.020)	0.095*** (0.017)	0.095*** (0.018)	0.093* (0.028)
Trust				
President	0.285*** (0.039)	0.208*** (0.053)	0.177*** (0.029)	0.252** (0.043)
Ruling party	0.247*** (0.038)	0.174*** (0.048)	0.141*** (0.022)	0.221** (0.039)
Electoral commission	0.187*** (0.024)	0.100*** (0.026)	0.110*** (0.019)	0.149*** (0.022)
Parliament	0.128*** (0.014)	0.069*** (0.021)	0.077*** (0.013)	0.114** (0.025)
Courts	0.080*** (0.009)	0.036*** (0.013)	0.045*** (0.013)	0.060* (0.020)
Police	0.074*** (0.014)	0.027* (0.015)	0.056*** (0.014)	0.058 (0.021)
Army	0.082*** (0.015)	0.041** (0.017)	0.059*** (0.014)	0.033 (0.034)
Traditional leaders	0.059*** (0.014)	0.026 (0.016)	0.034** (0.016)	0.039*** (0.009)
Opposition parties	−0.111*** (0.032)	−0.098** (0.036)	−0.041** (0.017)	−0.148** (0.031)
Identity				
National over ethnic identity	0.013 (0.022)	0.007 (0.020)	0.013 (0.014)	0.020 (0.022)

Note. Every outcome is standardized. Respondent models include survey and country-by-group fixed effects and controls as in Table 1. The turnover column regresses the change in group means across 13 turnovers on the alignment shift with event fixed effects; its stars use wild cluster bootstrap p-values. Standard errors clustered by country.

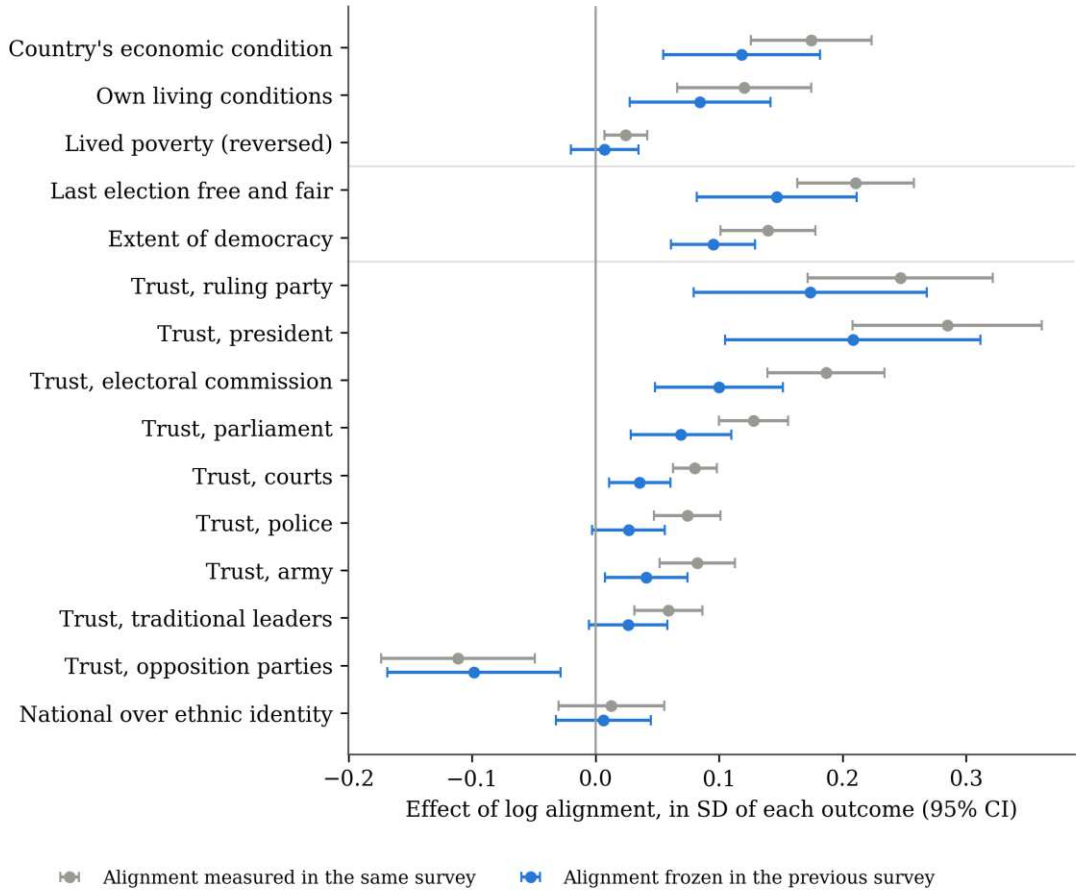


Figure 4. Effects of alignment on material conditions, perceptions, procedural judgments, institutional trust and identity, in standard deviations of each outcome. Lived poverty is reversed so that positive values mean better conditions. Survey and country-by-group fixed effects, 95 percent confidence intervals, standard errors clustered by country.

Timing gives little leverage. The post-turnover surveys were taken between 4 and 41 months after the election, with a median of 20. The estimates are too imprecise to show whether the effect grows or fades with time in office (Online Resource D6, Table D5).

The material measures have clear limits. Local services change slowly, and the sampled areas differ between survey rounds. Neither services nor lived poverty capture public jobs, contracts, appointments, protection or expected future benefits. The safe conclusion has two parts.

Attitudes shift without matching changes in the local services and household deprivation that Afrobarometer measures. And the pattern across perceptions, fairness judgments and trust fits an evaluative group lens. This does not rule out every material channel.

Scope

The relationship is not limited to less democratic settings. When I split the sample at the median level of V-Dem electoral democracy, alignment remains positively related to democratic evaluations in both halves of the sample. The relationship is, if anything, somewhat larger in the more democratic half.

Nor is there clear evidence that alignment matters more when the governing party has a more ethnically concentrated support base (Hamza and Maeda 2025). The interaction is weak and depends on how party ethnicization is measured (Online Resource E3).

In a selected sample of fifteen democracies from the Comparative Study of Electoral Systems, fourteen of them outside Africa, alignment is also associated with satisfaction with democracy (0.14, wild bootstrap $p = .025$). There, however, the association runs mainly through individual vote choice (Online Resource F). I do not claim that the African pattern generalizes.

Discussion and conclusion

In ethnically divided party systems, citizens can win and lose collectively as well as individually. Across 35 African countries, groups more closely aligned with the party in power judge democracy and incumbent-linked institutions more favorably. The relationship appears within groups over time, remains when party profiles are measured before attitudes, and survives the

removal of each respondent's own answer from the alignment measure. It is strongest among citizens without a party attachment and remains after accounting for coethnicity with the president and formal inclusion in power. The pattern also appears when power changes hands, while groups tied to challengers who lose do not show the same improvement. It survives comparisons within regions and group-specific time trends. The relationship does not extend to support for democracy as a form of government.

For research on losers' consent, the results show that the divide between winners and losers has a collective side that individual vote choice can miss. Studies of democratic support in multiethnic societies should consider where a citizen's group stands relative to the government, not only which party the individual supports. For research on ethnic inclusion, a group's tie to the governing party predicts evaluations beyond its expert-coded access to executive power. For research on ethnic politics, the findings also qualify the view that closeness to power matters mainly through access to goods and services (De Luca et al. 2018; Franck and Rainer 2012). Groups closer to power judge democracy more favorably even when local services and household conditions do not measurably improve. A change in the party holding power also matters more than a change in the leader when the governing party remains the same. For research on alternation (Moehler and Lindberg 2009), turnover may reduce one partisan divide in legitimacy while creating another. In ethnically divided systems, a change in government moves some groups closer to power and others farther away. It can shift the gap in democratic confidence from one set of groups to another rather than eliminate it.

The study has important limits. Alignment measures a group's relative partisan attachment to the governing party. It is a sign of political position, not a measure of cabinet representation or

formal access to power. None of the designs randomly assigns alignment. The frozen estimate does not survive group-specific time trends, and the turnover estimate is smaller and imprecise within regions. The close-election comparison includes relatively few elections, and satisfaction with democracy shows some movement before elections. Sensitivity analyses (Cinelli and Hazlett 2020) show that confounders as strong as coethnicity or a group's average education, urban residence or poverty would change the estimates only modestly. A confounder as strong as a respondent's own attachment to the governing party would cut the same-survey estimate by more than two thirds. One twice as strong would remove both estimates (Online Resource C). Group-level shocks of that size cannot be ruled out. The evidence on mechanisms is indirect. It comes from differences across outcomes rather than from measured mediators, and the available measures of material conditions are limited. A test based on news exposure gives the seeing step no direct support (Online Resource E8). Future work could follow the same individuals across changes of government or examine whether information about a group's position relative to those in power changes political evaluations.

Democracy asks losers to accept defeat. In many societies, what people accept or resent is not only their party's defeat. It is also the distance between their community and those who govern. A group's distance from power tracks confidence in how democracy works, but not support for democracy itself. Taking this collective side seriously helps explain why the same democracy can look so different to neighbors who live side by side.

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