Who Benefits? How Local Ethnic Demography Shapes Political Favoritism in Africa.

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Abstract

Empirical studies show that many governments gear the provision of goods and services towards their ethnic peers. This article investigates governments' strategies for the provision of ethnic favors in Africa. Recent studies of ethnic favoritism find advantages of either presidents' ethnic peers or home regions, yet cannot disentangle whether goods are provided to entire regions or co-ethnic individuals. We argue that local ethnic demography determines whether governments provide non-excludable public goods or more narrowly targeted handouts. Where government co-ethnics are in the majority, public goods benefit all locals regardless of their ethnic identity. Outside of these strongholds, incumbents pursue discriminatory strategies and only their co-ethnics gain from favoritism. Using fine-grained geographic data on ethnic demography, we find support for our argument's implications for the local incidence of infant mortality. Our findings have important implications for theories of distributive politics and conflict in multi-ethnic societies.

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Introduction

A large body of research shows that where ethnicity is a salient political cleavage, governments often gear the provision of goods and services towards their ethnic peers (Kimenyi, 2006; Franck and Rainer, 2012; Burgess et al., 2015; Hodler and Raschky, 2014; Kramon and Posner, 2016; Dreher et al., 2019; Jablonski, 2014). Ethnically biased goods provision slows economic development and can lead to distributional, sometimes violent ethnic conflict (Easterly and Levine, 1997; Cederman, Weidmann and Gleditsch, 2011). We build on studies demonstrating the existence of ethnic favoritism but move beyond them by investigating competing strategies of goods provision. We argue that governments can either provide regional public goods or target their ethnic peers with local club goods and private handouts. Understanding where governments provide which type of good yields important insights into the degree of ethnic discrimination that exists at various geographic levels.

Existing studies usually operationalize government co-ethnicity either at the level of individuals or subnational geographic units. In doing so, they remain largely agnostic as to whether governments target co-ethnic individuals (e.g. Franck and Rainer, 2012) or co-ethnic regions (e.g. De Luca et al., 2018; Hodler and Raschky, 2014). This distinction is particularly relevant in Africa where citizens' ethnic identity correlates with place of residence, but not perfectly so. Thus, individual and regional ethnic favoritism become observationally equivalent if tested with conventional research designs. Distinguishing the two strategies requires information on government co-ethnics in largely non-co-ethnic areas and vice versa. If targeting is purely individual, citizens only benefit when their ethnic peers hold power. If ethnic favoritism follows a regional logic, government co-ethnics and non-co-ethnics benefit in co-ethnic strongholds but not elsewhere.

In this paper, we develop and test the more nuanced argument that local ethnic demography affects national governments' choice between broad regional and more precisely targeted provision strategies. In areas mainly populated by government non-co-ethnics, incumbent coalitions specifically target co-ethnic sublocalities and individuals. Since such highly localized targeting is comparatively expensive, governments provide locally non-excludable public goods in co-ethnic majority regions. These goods benefit all residents of a region, including non-co-ethnic minorities.

We use rich geographic data on local ethnic demographies and infant mortality to test the implications of our theory across 22 African countries. Infant mortality is a broad 'catch-all' proxy for the consequences of favoritism as it is affected by goods provided to individuals (e.g. jobs and rents), villages (e.g. health clinics), or entire regions (e.g. large-scale infrastructure). The Demographic and Health Surveys contain information on the survival and death of more than 1.5 million African infants born between 1960 and 2013. Data on mothers' ethnic identities allow us to code individual co-ethnicity with the ruling coalition at the time of each infant's birth. The Spatially Interpolated Data on Ethnicity (SIDE; Müller-Crepon and Hunziker, 2018) provides data on local ethnic demographies used to construct the yearly, district-level population share that is co-ethnic with the national government. We use information on the ethnic composition of governments from the Ethnic Power Relations (EPR) data (Vogt et al., 2015).

With these data, we estimate the effects of changing co-ethnicity with the government at the individual and district levels. We identify these effects by only exploiting temporal variation in government co-ethnicity within ethnic groups and districts. Our findings support the implications of our argument on governments' differential goods provision strategies. We find that being born to a mother ethnically represented in the national government increases an infant's chance of survival by about 1.5 percentage points. However, this effect only holds in districts where the governing ethnic groups are in the minority at the time of birth, suggesting the provision of individually targeted benefits to government co-ethnics. Being born in a district with a high percentage of government co-ethnics similarly increases infant survival rates by 0.9–1.8 percentage points. In these districts, however, individual co-ethnicity with the government does not increase infant survival rates any further. This is consistent with the provision of locally non-excludable public goods.

Several robustness checks show that our results are not due to reverse causality, systematic migration patterns, or segregation of ethnic groups within districts. Measuring government co-ethnicity with data on individual cabinet members' ethnic identities from Francois, Rainer and Trebbi (2015) does not substantively alter our results. Additional analyses show that neither regime type nor electoral rules moderate our findings. Correlational evidence from Afrobarometer data indicates that governments indeed provide public goods to co-ethnic districts and more narrowly target their peers elsewhere.

Literature

Ethnic favoritism constitutes the main material underpinning of political competition along ethnic cleavages. According to Bates (1974, 152), ethnic groups can be understood as coalitions formed to secure scarce benefits of modernization and economic development. Ethnic markers such as individuals' language or phenotype enable an ethnically biased targeting of 'pork' (Fearon, 1999) and thus foster the formation of ethno-political coalitions that benefit their members at the expense of other groups (Padró i Miquel, 2007; Wantchekon, 2003).

Empirical studies provide substantive evidence that governments favor their co-ethnic citizens and home regions (Franck and Rainer, 2012; Burgess et al., 2015; Hodler and Raschky, 2014). A first strand of research investigates whether presidents provide benefits to individuals who share their ethnic identity. Franck and Rainer (2012) find that presidents' co-ethnics are better educated and enjoy lower infant mortality rates than other citizens. Kramon and Posner (2013), on the other hand, show that the effect of co-ethnicity with the president varies across countries and specific goods.

A second body of work operationalizes ethnic favoritism as an (ethno-)regional phenomenon. Burgess et al. (2015) report that Kenyan road investments during autocratic spells favor districts that share the president's ethnicity. Hodler and Raschky (2014) and De Luca et al. (2018) show that presidents' home regions experience faster growth measured by nightlights. Similarly, Dreher et al. (2019) find favoritism in Chinese-funded aid projects.[1] Kramon and Posner (2016) provide evidence that alternative measures of individual- and district-level co-ethnicity with the president both contribute to educa-

[1] See also Jablonski (2014).

tional attainment in Kenya.

While sufficient to demonstrate the existence of favoritism per se, these two empirical approaches do not distinguish ethnic from geographic drivers of favoritism. Subnational regions in multi-ethnic countries are rarely perfectly homogeneous and many citizens live outside their ethnic home region.[2] The empirical inability to differentiate the forms of ethnic favoritism comes with theoretical imprecision. Do governments target individuals or regions? Or do they follow a mixed strategy to minimize costs in the face of regionally varying ethnic demographies?

Recent work on voting and distributive politics has started to disentangle the interplay between individuals' identities and local ethnic demography in Africa. In Ghana, Ichino and Nathan (2013) find that rural voters from local minority groups support non-coethnic majority candidates who promise locally non-excludable goods that benefit all residents. Similarly, Carlson (2015) and Nathan (2016) find that where one ethnic group dominates, residents expect public goods from politicians aligned with the local majority. However, local minority voters expect private goods from co-ethnic governments. Voters thus believe that politicians distribute goods in response to local demographics. However, neither study tests whether these patterns are reflected in data on actual goods provision or economic well-being.

Two recent studies on local public goods provision provide evidence that local demography affects distributive politics. Ejdemyr, Kramon and Robinson (2018) argue that providing local club goods can only serve as a favoritism device if a politician's co-ethnics live geographically segregated. Data from Malawi supports their argument: ethnically segregated districts receive more boreholes and within these districts, MPs target mainly co-ethnic localities. Harris and Posner (2019) find similarly fine-tuned targeting of development projects in Kenya, where MPs tend to favor their co-ethnics and spatially segregated political supporters.

This article joins these studies in highlighting the importance of local ethnic demography but moves beyond them in four ways. First, our argument theorizes the role of ethnic

[2] The DHS data (USAID, 2012) suggests that 84% of the districts and 72% of the enumeration areas in our sample are populated by more than one ethnic group.

demography across a broader range of government-provided goods than very locally targeted club goods. Second, we show that not only geographically segregated co-ethnic strongholds but also co-ethnic minorities in 'opposition' districts profit from having their peers in power. Third, we test whether non-co-ethnic individuals indeed benefit from living in co-ethnic strongholds as argued by Ichino and Nathan (2013) and Ejdemyr, Kramon and Robinson (2018) and distinguish this effect from that of local ethnic segregation. Fourth, covering 22 countries over 54 years, our analysis allows for more general conclusions than studies based on single countries, specific goods, or cross-sectional variation.

Theoretical argument

We argue that areas where the majority population shares the national executive's ethnicity receive locally non-excludable club goods that benefit all residents. In areas with a small share of co-ethnics, governments provide more narrowly targeted goods to the few inhabitants who share their identity. As a result, government co-ethnics benefit from favoritism everywhere whereas non-co-ethnics only benefit if they reside in governments' ethnic strongholds.

These predictions rest on three assumptions. First, the executive ruling coalition holds most power over distributive spending. Second, governments can more easily secure political support from co-ethnic citizens. Third, the per-beneficiary provision costs of goods increases the more narrowly they are targeted to individuals. We start by highlighting the government branches that can best engage in ethnic favoritsm, discuss why they would do so, and how they tailor goods provision strategies to local ethnic demographies.

Why do co-ethnics benefit?

Power and incentives within ruling coalitions. African state leaders want to stay in power but cannot rule alone (Francois, Rainer and Trebbi, 2015; Svolik, 2012). To survive politically, autocratic and democratic leaders need sufficient popular support to ward off revolutions, rebellions, or electoral defeat (Svolik, 2012; Bormann, 2019; Francois, Rainer and Trebbi, 2015).[3] Leaders secure this support by targeting benefits to important constituencies.[4] Support buying is ethnically biased if in-group support is cheaper to buy than out-group backing. However, relying exclusively on co-ethnic support is often insufficient. As a remedy, most African leaders include elites from other ethnic groups in their cabinets (Bormann, 2019; Francois, Rainer and Trebbi, 2015).[5] To make such ethnic power-sharing work, leaders need to give their coalition partners sufficient power and resources. Otherwise, their partners loose support from their constituencies (Beiser-McGrath and Metternich, 2020) or, worse, may threaten the regime from within (Roessler, 2011). Potentially opposing their governments, African legislatures in electoral autocracies and (semi-)democratic regimes have long remained too week to effectively constrain executive appointment and spending decisions (Fish and Kroenig, 2009; Rainer and Trebbi, 2014).[6] For these reasons and in line with past research, we see the executive branch of government, comprising the leader and her most important coalition partners, as the main locus of materially consequential power brokering (Arriola, 2009; Rainer and Trebbi, 2014).

Buying support from co-ethnics. Executive ruling coalitions prefer to buy support from their ethnic peers for several reasons. Citizens who derive expressive or "psychic" utility from supporting co-ethnic candidates require less material incentives to be swayed (Chandra, 2004). As such, they constitute a governing coalition's core support group and are the first to be targeted with goods and services (Cox and McCubbins, 1986). Even where citizens are a priori indifferent between politicians from different groups and expected material benefits are all that matters, governments may serve co-ethnics first. A common culture, language, dense social networks, and higher trust in co-ethnics

[3] The need for national-level support mainly holds in winner-takes-all executive elections rather than majoritarian legislative elections where politicians face incentives to target contested swing districts (Casey, 2015).

[4] See Golden and Min (2013) and Cox (2009) for useful reviews of the distributive politics literature.

[5] According to the EPR data set, 74% of all country-years analyzed below were multi-ethnic. Francois, Rainer and Trebbi (2015) list not a single mono-ethnic cabinet.

[6] Note however that this changes as parties and legislatures become more important in consolidating democracies (Barkan, 2008; Ariotti and Golder, 2018; Opalo, 2019).

lower transaction costs, facilitate information flows, and ease in-group policing (Fearon and Laitin, 1996; Larson and Lewis, 2017; Robinson, 2020). Better information about co-ethnics' preferences and reliable local intermediaries enable governments to more efficiently allocate benefits (Dixit and Londregan, 1996; Baldwin, 2016). Similarly, promises of favoritism and political support are more credible if coming from co-ethnics (Carlson, 2015).

For governments, each unit spent on co-ethnics thus promises higher and more certain political support. While voters can of course also support non-co-ethnic candidates (Nathan, 2016; Ichino and Nathan, 2013), empirical evidence shows that good performance and clientelistic appeals by politicians generate more support among co-ethnic than non-co-ethnic voters (Carlson, 2015; Adida et al., 2017; Wantchekon, 2003; Kramon, 2017). This evidence supports our assumption that governments can more cost-effectively buy political backing from their ethnic peers than from other groups. Case studies suggest that this argument also holds in autocracies such as Nigeria under military rule. The ruling coalition of Hausa-Fulani and other Muslim elites from the North favored their ethnic and regional peers through redistricting reforms, federal revenue sharing formulas, and rampant patronage (Abubakar, 2001; Bah, 2004).

Who gets what?

Governments have to decide where to provide what kind of favors. We argue that differential provision costs for different types of goods as well as local ethnic demographics inform this decision.

Precise ethnic targeting is costly. Government-provided goods can be ordered along a continuum between local public goods and individually targeted private goods. At one end, there are non-excludable (local) public services such as schools and hospitals, or infrastructure such as a highway that benefit all local residents regardless of their ethnic affiliation. At the other end of the spectrum, governments serve individually selected citizens with targeted handouts such as public employment, food, and ultimately money (Albertus, 2013; Besley et al., 2004). In between these extremes are local public goods that are targeted at ever smaller localities, for example a small village road or a neighborhood water well. Goods thus vary in the degree to which their provision can exclude (groups of) citizens that are not intended to benefit.

We assume that the per-beneficiary cost of providing goods increases in their excludability. Locally non-excludable goods and infrastructures require high initial investments but usage costs typically remain low. Once set up, a large number of local beneficiaries profits over extended periods. As a result, the provision cost per user is lower than for more narrowly targeted club goods and private handouts. The latter require more information on the intended recipients – locations, groups, and individuals – and favors need to be customized to recipients' specific needs (Armesto, 2010; Ichino and Nathan, 2013).[7] While the non-discriminatory provision of public goods is cheaper on a per capita basis, governments have to pay the cost of local public services for every individual in the targeted locality, including those citizens that the government does not intend to target (Nathan, 2016). Thus, the cost-efficiency of locally non-excludable goods increases with the number of recipients the government wants to reach (Ejdemyr, Kramon and Robinson, 2018).

Local ethnic demography and goods provision. In short, governments face two, potentially conflicting, logics of goods provision. First, they prefer to target co-ethnic individuals. Second, governments prefer to deliver locally non-excludable goods that come at lower per-capita cost and benefit all citizens in their vicinity. Local ethnic population shares determine the optimal provision strategy that addresses this trade-off.

Homogenous co-ethnic areas constitute the easiest case. Here, governments want to reach almost all residents and therefore provide locally non-excludable goods. The dominant majority population in these government strongholds and the small local minority of non-co-ethnics benefit equally. Specifically targeting co-ethnics in non-co-ethnic 'opposition' districts requires more expensive club and private goods. Strategic incumbents only

[7] Governments may also transform local public goods into private ones by restricting access, which is costly.

incur these costs if they rely on the support from co-ethnics in such non-co-ethnic areas. Where public goods provision to mainly co-ethnic regions ensures political survival, co-ethnics elsewhere may end up getting nothing and favoritism turns into a purely regional phenomenon. However, *if* governments require at least some support from co-ethnics in districts where they are in the minority, they will serve them with targeted goods to save costs.

Ruling coalitions with a relatively small ethnic support base may additionally need the support of at least some non-co-ethnics. To minimize costs, they will provide public goods to areas with the highest proportions of co-ethnics first, before turning to non-co-ethnics in other areas. Similarly, the higher the costs of providing targeted benefits to co-ethnic individuals, the more governments rely on public goods spending in increasingly mixed districts to sway non-co-ethnic minorities. As long as co-ethnics respond more readily to favoritism, however, targeted strategies remain rational in districts with low proportions of government co-ethnics.

Taken together, we argue that governments allocate non-excludable goods to districts with a high share of government co-ethnics. In areas dominated by ethnic groups not represented in the executive, governments target their co-ethnics with local club goods or private handouts. These two expectations lead to the following observable implications:

Hypothesis: In districts with low shares of government co-ethnics, co-ethnic individuals are more likely to receive benefits from the government than non-co-ethnic individuals (*individual ethnic favoritism*). Districts with a higher share of co-ethnics receive greater benefits but an inhabitant's ethnic identity has no effect on her likelihood of receiving goods from the government (*regional ethnic favoritism*).

Data

We test our theory with a large, individual-level dataset on infant mortality covering 22 African countries and 54 years (1960-2013). We expect our argument to be relevant in other multi-ethnic Sub-Saharan African cases as well, but the availability of geocoded data on our main outcome measure, infant mortality, limits our sample.[8]

Outcome data: Our infant mortality data come from the Demographic and Health Surveys (DHS, USAID, 2012). Female DHS respondents' birth histories record the survival and death of 1.5 million infants born between 1960 and 2013.[9] The surveys include mothers' ethnic identity, current location, and their children's birth years. This information and the large sample size allow us to exploit cross-sectional variation in mortality rates within countries and temporal variation within districts and ethnic groups. We are not aware of any other individual-level socio-economic data with similar spatio-temporal resolution.

The usefulness of infant mortality as a proxy for both individual and regional favoritism depends on the extent to which mortality rates respond to government-provided private and public goods. Both relationships are well documented in the literature. Private goods such as public sector jobs (Posner, 2005), access to local markets (Bates, 1974), or handouts such as medication or other supplies[10] can affect infant mortality either directly as in the case of medical supplies or by increasing household income. Increases in household income have been shown to mitigate various risk factors such as malnutrition, respiratory infections, and malaria among vulnerable children in the DR Congo (Grellety et al., 2017), Burkina Faso (Houngbe et al., 2017), and Kenya (Huang et al., 2017). Similarly, conditional cash transfers combined with health or nutritional programs decrease child mortality (Barham, 2011; Rasella et al., 2013).

With respect to public goods, previous research highlights the importance of infrastructure (Fay et al., 2005), especially public health care facilities (Gruber, Hendren and Townsend, 2014) and sanitation systems (Galiani, Gertler and Schargrodsky, 2005). Of course, other types of public goods such as roads may also affect infant mortality via their effects on household income and lower travel times to health facilities.

While infant mortality rates plausibly capture various types of government-provided

[8]Although ethno-regional favoritism is not unique to Africa (De Luca et al., 2018), the validity of our argument beyond our sample depends on whether our main assumptions hold in the respective cases.

[9]We drop infants born less than 12 months before an interview.

[10]E.g. fertilizer as studied by Abman and Carney (2019).

goods, they do not allow us to directly distinguish excludable private from non-excludable public goods. We address this shortcoming by (1) testing whether individual-level ethnic favoritism is apparent within DHS survey clusters (typically villages or urban neighborhoods) and (2) analyzing cross-sectional data on local public services provision from the Afrobarometer surveys.

Co-ethnicity with the government: To test for individual-level favoritism, we measure infants' co-ethnicity with the government by linking their mothers' ethnic identities to the ruling ethnic groups in the respective country and birth year. Information on ethnic representation in executive governments comes from the Ethnic Power Relations dataset (EPR, Vogt et al., 2015).[11] For each country-year, EPR lists all politically relevant ethnic groups[12] and codes whether they have meaningful representation in the national executive as opposed to mere window-dressing. Aiming to identify ethnic groups whose elites hold power and control distributive resources in the executive, the data thus code multi-ethnic coalitions, which are frequent in Africa (Francois, Rainer and Trebbi, 2015).[13] If government elites from other than the president's group indeed engage in favoritism, this coalition-based approach is more precise than a mere focus on heads of state.[14]

To operationalize the regional aspect of favoritism, we measure the proportion of each district's population that is co-ethnic with the government in a given year. The SIDE dataset on local ethnic demographies in Africa provides non-parametric spatial interpolations of DHS survey locations' ethnic population shares (Müller-Crepon and Hunziker, 2018). For each DHS survey, SIDE predicts cell-level ethnic compositions on a raster with about 1 × 1 km resolution. Although the interpolation increases precision

[11]See Figure A5 for the ethnic power constellations in each country-year. Groups are matched based on group names and information from encyclopedias such as `ethnologue.com`, `wikipedia.com`, and `joshuaproject.org`.

[12]EPR codes ethnic groups as politically relevant if they are politically mobilized at the national level or discriminated against by the government (Vogt et al., 2015).

[13]EPR provides an ordinal measure of ethnic representation and does not list the positions and elites informing this proxy. Francois, Rainer and Trebbi (2015) provide alternative data on the ethnic composition of cabinets in 15 countries, used in robustness check A11.

[14]But see robustness check A7 where we analyze EPR's "senior partners", in most cases the presidents' ethnic groups.

over the raw DHS data or more commonly used polygon-data, the estimates are not precise enough to capture local segregation in ethnically mixed areas.[15]

Linking the SIDE data to EPR and aggregating it to the district level, we calculate the percentage of government co-ethnics for each district, year, and DHS survey round as

$$\mathsf{District\ Share\ Co\text{-}Ethnic}_{dts} = \frac{\sum_{c=0}^{C_d} \sum_{e=0}^{E} \mathsf{pop}_{ct} * \mathsf{share}_{ecs} * \mathsf{incl}_{et}}{\sum_{c=0}^{C_d} \mathsf{pop}_{ct}},$$

where d denotes a district in year t with SIDE information on ethnic groups E estimated on the basis of DHS survey s. C_d are all raster cells c in district d,[16] and each cell has a population of pop_{ct}.[17] In each raster cell, each ethnic group e has an estimated population share_{ecs} derived from the SIDE data for DHS survey s. Linking ethnic group e to EPR provides information on its representation in government: $\mathsf{incl}_{et} \in [0; 1]$. In sum, this spatial computation yields, for each district-year per survey, the population share of government co-ethnics. Our measure exhibits substantial spatial variation within countries (Figure 1).[18] Similarly, variation in the ethnic composition of governments produces large changes within districts over time (Figure 2).

Finally, we assign $\mathsf{District\ Share\ Co\text{-}Ethnic}_{dts}$ to infants via mothers' geocoded enumeration area, survey round s, and each infants' year of birth t. Calculating district-level co-ethnicity on a per-survey basis ensures that the coding of ethnic groups, which changes between DHS rounds, remains consistent when we match an infant to the district-level data.[19]

Lacking data on migration patterns, we take each mother's place of residence at the time of the survey as all her childrens' birthplace. In parallel, we assign each district a constant ethnic demography. To identify valid effects, we therefore assume that, conditional on several fixed effects, unobserved migration is orthogonal to spatio-temporal

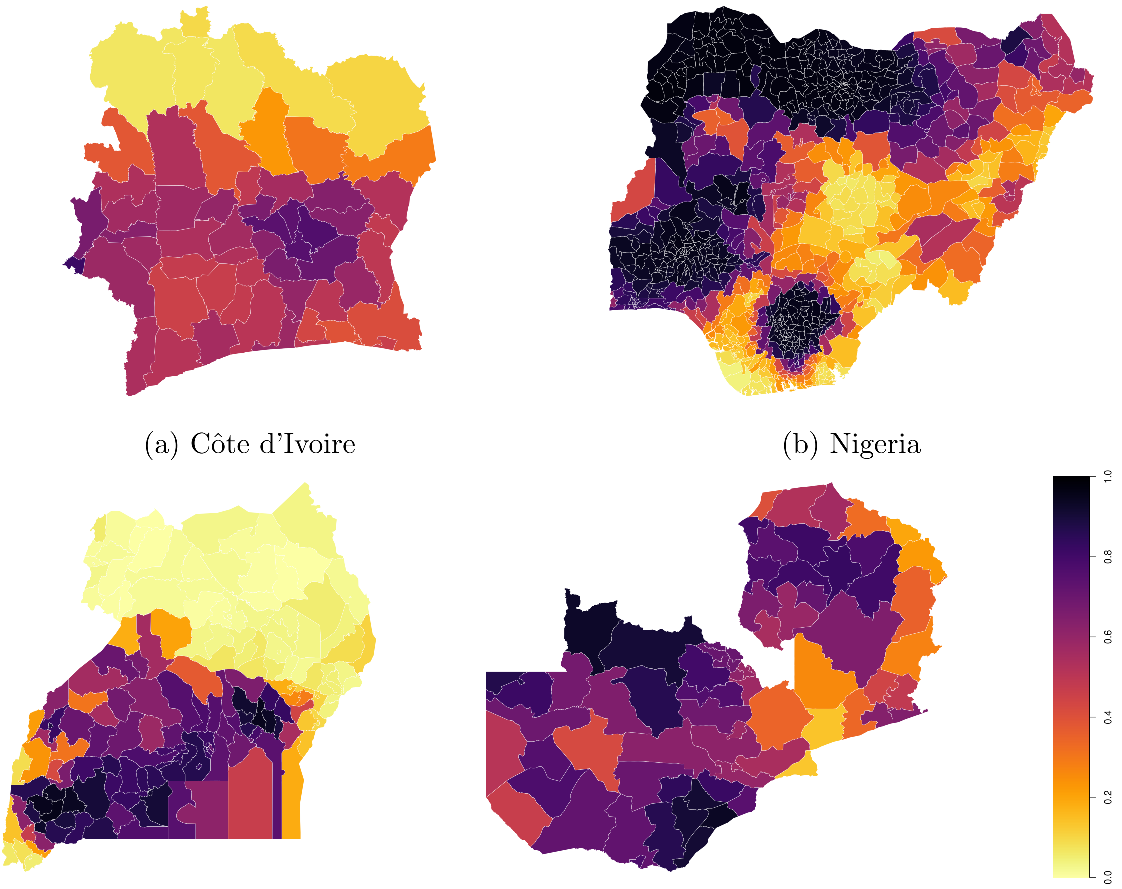
[15]Since no non-ethnic covariates affect the predictions, interpolation errors constitute random noise.

[16]District borders are from 2000 and come from FAO (2014).

[17]Population rasters with the same resolution as SIDE come from CIESIN et al. (2011) and cover the years 1990, 1995, and 2000. We take the latest year available for each t and assume that changes in districts' population distribution only marginally affect the ethnic compositions.

[18]Co-ethnicity shares are bimodally distributed with 60% districts with shares below 10% or above 90% and 40% in between. 33% of DHS enumeration area-years have 10-90% government co-ethnics.

[19]Appendix Table A2 shows that both measures coincide.

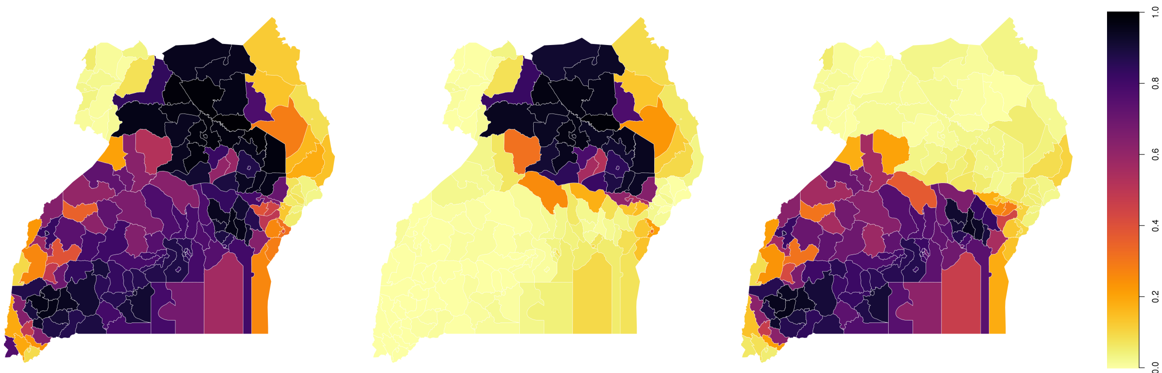


(a) Côte d'Ivoire (b) Nigeria

(c) Uganda (d) Zambia

Figure 1: District-level co-ethnicity with the government in 2000 using the most recent available SIDE data

See Figure A3 in the Appendix for maps of all other countries in our sample.



(a) Uganda, 1965 (b) 1980 (c) and 1995

Figure 2: District-level co-ethnicity with the government over time; SIDE data from 2011

changes in ethnic inclusion and infant mortality. We address this assumption below.

Empirical Strategy

Our models estimate the effect of individual- and district-level co-ethnicity with the government on infant mortality. To test our intuition that governments only discriminate between their co-ethnics and other citizens in districts where the co-ethnic share is low, we include an interaction term assessing whether district-level co-ethnicity with the government moderates any individual-level co-ethnicity advantage. We use linear probability models to enable a fixed effects strategy that controls for confounding characteristics of ethnic groups, districts, and temporal shocks.[20]

Our baseline specification takes the following form:

$$
\begin{aligned}
Y_{iedrst} = \alpha_{es} + \lambda_{ds} + \gamma_{rst} + \beta_1\,\textsf{Co-Ethnic Government}_{est-1} + \beta_2\,\textsf{District Share Co-Ethnic}_{dst-1} \\
+ \beta_3\,\textsf{Co-Ethnic Government}_{est-1} \times \textsf{District Share Co-Ethnic}_{dst-1} + \delta X_{iedrst} + \epsilon_{iedrst}
\end{aligned}
$$

The unit of analysis is infant i born in year t to a mother who hails from ethnic group e, resides in district d in subnational region r, and was interviewed in DHS country-survey round s. Y_{ierst} is a dummy variable coding the death ($Y = 100$) or survival ($Y = 0$) of infant i in the first twelve months after its birth. We code the dummy as 100 to straightforwardly interpret the coefficients in terms of percentage points.

The main variables of interest are a mother's co-ethnicity with the government, the district-level population share of government co-ethnics, as well as the interaction term of these two variables.[21] We temporally lag these predictors by one year to allow the hypothesized effects to unfold.[22] Our hypothesis stipulates that β_1, the coefficient of individual-level co-ethnicity, is negative because governments favor their co-ethnics everywhere. Co-ethnics in government strongholds receive local public goods whereas co-ethnic minorities in opposition districts are targeted individually. As public goods also

[20]Absent well-founded assumptions about the "true" functional form, the LPM does not perform worse than logit or probit models (Angrist and Pischke, 2008).

[21]See e.g. Sands and de Kadt (2019) for a similar but not equivalent modelling strategy.

[22]Our results are robust to using alternative time lags (Table A3).

benefit non-co-ethnic residents, we expect β_2 to be negative. Finally, because co-ethnics in mainly co-ethnic districts receive the same public goods as non-co-ethnics and no additional private handouts, we expect the interaction effect β_3 to be positive.

In order to credibly identify the effects of our three variables of interest, the model only exploits yearly variation in ethnic representation as coded in the EPR data. We treat these changes as shocks that differentially affect infants born in the same year and to mothers from the same ethnic group and district. We thus add three sets of fixed effects.

The first, α_{es}, is a vector of ethnic-group-survey-round fixed effects. They ensure that we only compare infants within ethnic groups whose mothers were interviewed in the same survey. This controls for time-invariant characteristics of ethnic groups such as their demographic size or geographic settlement area that may simultaneously affect infant mortality and their political representation. We use separate fixed effects for each survey round to ensure that mothers' ethnic identities line up with the SIDE district shares derived from the same DHS surveys. Second, λ_{ds} is a vector of district-survey-round fixed effects that controls for unobserved, time-invariant differences between districts such as resource endowments. These ethnic group and district fixed effects imply that we only identify effects from temporal variation within ethnic groups and districts.

Third, region-survey-birthyear fixed effects γ_{rst} flexibly control for temporal shocks to infant mortality that differentially affect first-level administrative units. Subnational shocks such as differential economic growth may correlate with government composition and bias results from models with looser temporal controls. In fact, infant mortality rates in Sub-Saharan Africa exhibit far greater within-country than between-country variation (Burke, Heft-Neal and Bendavid, 2016). These region-year fixed effects also absorb all common shocks that operate at the the continent- or country-level, for example institutional reforms or changes in government budgets.

X_{ierst} is a vector of individual control variables including female and twin dummies, an infant's birth rank and its square, as well as mothers' age and age square at the time of birth. We report two-way clustered standard errors at the level of both ethnic and district-survey rounds to account for cross-sectional and serial correlation across infants and birth cohorts within the units affected by our treatment.[23] Regression weights ensure that the 22 countries in our sample contribute equally to the estimated effects.[24]

Results

Our baseline specification as well as increasingly conservative models yield support for our hypothesis. In districts with high shares of government co-ethnics, infants from *all* ethnic groups exhibit lower mortality rates. In districts with few government co-ethnics, on the other hand, this is only the case for co-ethnic children.

Table 1: Main Specifications

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	-1.487^{***}		-1.651^{***}	
	(0.391)		(0.462)	
Dist. Share Gov. Co-Ethnics (t-1)	-1.816^{***}	-1.687^{*}		
	(0.637)	(0.878)		
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.824^{***}	1.880^{***}	1.835^{***}	1.714^{***}
	(0.503)	(0.554)	(0.537)	(0.582)
Survey-Ethnic FE	yes	–	yes	–
Survey-District FE	yes	yes	–	–
Survey-Region-Birthyear FE	yes	yes	–	–
Survey-Ethnic-Birthyear FE	no	yes	no	yes
Survey-District-Birthyear FE	no	no	yes	yes
Controls	yes	yes	yes	yes
Observations	1,503,930	1,503,930	1,503,930	1,503,930
Adjusted R^2	0.060	0.061	0.068	0.069

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.78 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Model 1 in Table 1 presents the results of our main specifications. The coefficient of the individual-level government co-ethnicity variable is negative and statistically significant. It suggests that co-ethnic infants born in districts with very low shares of government co-ethnics have a 1.49 percentage points lower mortality risk than non-co-ethnic infants

[23] Our results are robust to clustering at different resolutions (see Table A4).

[24] Weights are computed as the inverse of the number of observations from each country. They avoid undue influence of countries that are (a) more frequently surveyed (i.e. more developed) by the DHS or (b) have high fertility rates (i.e. less developed). Unweighted regressions lead to similar results (Table A5).

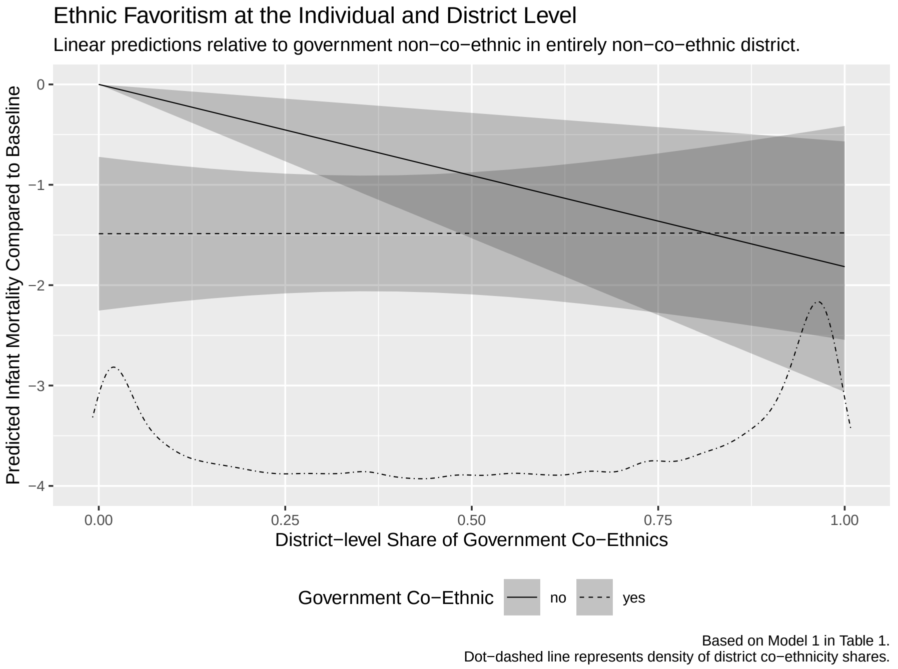


Figure 3: Predictions for government co-ethnics and non-co-ethnics conditional on local ethnic demography

in these districts. The coefficient on district-level government co-ethnicity is also significantly negative. Increasing the share of co-ethnics in an infant's district by 50 percentage points is associated with a decrease in non-co-ethnic mortality by 0.91 percentage points.

The coefficient of the interaction term of individual-level co-ethnicity × district-level share of co-ethnics is significantly positive and of about the same size as the constitutive terms. Figure 3 visualizes how the interplay of individually and regionally targeted ethnic favoritism affects infants' mortality. A non-co-ethnic infant in a district with no government co-ethnics serves as the baseline category to which we compare predicted outcomes for infants in all other categories.[25] The x-axis displays the proportion of non-co-ethnics residing in a district. At low district shares of government co-ethnics, co-ethnic infants (dashed line) have a 1.49 percentage points lower mortality than their non-co-ethnic counterparts (solid line). As the share of government co-ethnics rises, co-ethnic infants' estimated survival advantage over non-co-ethnic infants in non-co-ethnic districts remains

[25]These predictions visualize the substitution effect between individual and district-level co-ethnicity. Predictions for co-ethnics are calculated as $\beta_1 + a_d\,\beta_2 + a_d\,\beta_3$; those for non-co-ethnics as $a_d\,\beta_2$ where a_d is the district share of government co-ethnics in hypothetical district d. Appendix Figure A6 shows conventional marginal effect plots of individual co-ethnicity across the range of district-level government co-ethnicity.

essentially constant. Mortality estimates for non-co-ethnic infants show a markedly different pattern. With increasing shares of government co-ethnics in a district, non-co-ethnic infants' mortality rates decrease. At very high district shares of government co-ethnics, non-co-ethnic infants' predicted mortality rate is about 1.82 percentage points below the one for non-co-ethnic infants in entirely non-co-ethnic districts. In districts with high shares of co-ethnics, co-ethnic infants have no advantage over non-co-ethnic children.[26]

The size of these effects is substantial. The average infant mortality rate in our sample drops by about .2 percentage points each year.[27] Thus, the disadvantage of non-co-ethnic children in non-co-ethnic regions compared to co-ethnic children or those born in co-ethnic districts amounts to between 7 and 9 years of that trend.

As our hypothesis suggests, co-ethnic and non-coethnic mortality rates only differ significantly in districts where government co-ethnics are the minority. Where government co-ethnics constitute more than about half of a district's population, co-ethnic and non-coethnic mortality rates are statistically indistinguishable. These findings are consistent with the provision of locally non-excludable goods in ethnic government strongholds and the provision of excludable goods in government minority districts.

Models 2–4 in Table 1 add fixed effects that account for unobserved temporal shocks at the level of districts and ethnic groups. Both may affect the ethnic composition of governments and bias our estimates. Model 2 includes survey-ethnic-group-year-of-birth fixed effects. They control for all yearly events at the level of groups such as changes in economic development or the outbreak of violence. These fixed effects fully absorb variation in individual-level government co-ethnicity and the respective constitutive term remains unidentified. Model 3 follows a similar logic and adds survey-district-year-of-birth fixed effects that control for shocks to a given district. Comparing only infants born in the same year and district, the model no longer identifies the district-level constitutive term. Finally, Model 4 combines both of these spatio-temporal fixed effects and thus only identifies the interaction term. Throughout these specifications, the identified coefficients

[26]The binning estimator proposed by Hainmueller, Mummolo and Xu (2019) demonstrates that our findings are not an artifact of linearity assumptions or lack of common support. See Appendix Figures A7 and A8.

[27]See Appendix Figure A2.

of interest remain stable and statistically significant. Our results are thus not caused by time-variant omitted variables at the level of ethnic groups or districts.

Alternative explanations and sensitivity analyses

In this section, we provide evidence that our results are not driven by within-district ethnic segregation, non-parallel trends between ethnic groups, and unobserved migration patterns. We then show that the results are robust to an alternative operationalization of co-ethnicity with the government. Lastly, our heterogeneity analysis fails to detect moderating effects of regime type or electoral systems.

Ethnic segregation within districts. The effects of both individual-level and district-level government co-ethnicity in the main analyses may not result from the provision of different types of goods but from uniform club goods provision to mainly co-ethnic localities in ethnically segregated districts (as in Ejdemyr, Kramon and Robinson, 2018). Homogeneous co-ethnic villages rather than co-ethnic individuals might be the primary beneficiaries of ethnic favoritism in mainly non-co-ethnic districts. Similarly, the district-level effects might be driven by government non-co-ethnics who happen to reside in relatively homogeneous co-ethnic villages rather than by entirely non-co-ethnic villages benefiting from broader district-level public goods.

We estimate three additional models to address this inferential threat. First and foremost, we limit comparisons to government co-ethnics and non-co-ethnics born in the same year and survey cluster, typically a village or urban neighborhood. The results in Model 1 of Table 2 suggest that government co-ethnics born in survey clusters in non-co-ethnic districts have, on average, better survival chances than non-co-ethnic infants born in the same year and location.[28] Second, we define regional favoritism at the survey cluster rather than the district level. Even in mainly non-co-ethnic enumeration areas, co-ethnic infants survive longer (Model 2 in Table A10). Third, we aggregate our main

[28] Additional models in Appendix Table A14 limit comparisons to children born (1) within the same household and (2) to the same mother. While standard errors increase due to a drastic reduction in the effective sample size and many coefficients miss significance, the substantive pattern in our point estimates remains consistent with the baseline results.

variables to the level of DHS enumeration-area-years. In districts with high shares of government co-ethnics, even entirely non-coethnic enumeration areas show significantly lower infant mortality (Model 3 in Table A10). The very local individual and broader district-level effects suggest that our baseline results are not entirely due to local club goods provided to co-ethnic villages in segregated districts.

Reverse Causation and non-parallel trends. To interpret our results as causal, we must assume that changes in the ethnic composition of ruling coalitions are exogenous to observed or correctly anticipated trends of ethnic groups' infant mortality rates and their causes. If, for example, ethnic groups' economic development affects their political inclusion, reverse causation can bias our findings. To address this potential caveat, we include dummies for the three years before an ethnic group gets upgraded to or excluded from the national executive.[29] The results from Model 3 in Table 2 do not show any evidence of differential infant mortality in these periods. Additional results from regressions including individual dummies for the three (one) year(s) prior to government changes or linear time trends for three or five years before such changes are reported in Appendix Table A9. All of these terms remain small and insignificant or point in the direction that supports our argument. This reduces our concerns about reverse causation, anticipation effects, and non-parallel trends.

Migration. Unobserved migration patterns and other demographic shifts present a third challenge to our findings. Recall that we use a mother's place of residence at the time of the DHS interview as the birthplace of her children, thereby introducing measurement error. Similarly, the ethnic group shares from SIDE are assumed to be constant. All temporal variation in the district share of government co-ethnics thus comes from changes in governments' ethnic composition. If political inclusion correlates with fertility rates or migration patterns, our measure of district-level co-ethnicity will be slightly off. Given the fixed effects in Table 1, these patterns only bias our results if they co-vary with government changes in a way not captured by ethnic group- and

[29] See Hodler and Raschky (2014) for a similar approach.

Table 2: Robustness: Cluster-Year Fixed Effects, Trends, and Subsample Analysis

	Infant Mortality			
	Cluster-YoB FE	Time Trend	Pre-Trends	Less Educated
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.506**	−1.318***	−1.662***	−1.027**
	(0.698)	(0.486)	(0.420)	(0.436)
Dist. Share Government Co-Ethnics (t-1)		−1.835**	−2.094***	−1.654**
		(0.772)	(0.703)	(0.699)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	0.928	1.891***	1.941***	1.228**
	(0.776)	(0.540)	(0.517)	(0.594)
$\text{Upgrade}_{t \text{ to } t+2}$			−0.619	
			(0.503)	
$\text{Downgrade}_{t \text{ to } t+2}$			−0.248	
			(0.495)	
Survey-Ethnic FE	yes	yes	yes	yes
Survey-District FE	–	yes	yes	yes
Survey-Region-Birthyear FE	–	yes	yes	yes
Survey-Cluster-Birthyear FE	yes	no	no	no
Survey-Ethnic Time Trend	no	yes	no	no
Controls	yes	yes	yes	yes
Observations	1,503,930	1,503,930	1,483,404	1,315,931
Adjusted R^2	0.099	0.060	0.060	0.062

Notes: OLS linear probability models. Column 4 restricts the sample to children born to mothers with less than secondary education. The sample mean of the dependent variable is 10.77 in columns 1–3 and 11.37 in column 4. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared, as well as childrens' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

district-year fixed effects and the pre-trends analyzed above.

However, well-off non-co-ethnics from poor districts may systematically migrate to areas that are strongholds of a *new* government in order to access emerging opportunities. Similarly, if the incoming government coalition hands out jobs to co-ethnics, newly appointed and comparatively wealthy state bureaucrats may be posted to non-co-ethnic districts. To bias our results, such migration patterns have to temporally co-vary with government changes and be distinct from the overall temporal trend at the level of districts and ethnic groups. We regard such scenarios as rather unlikely.

If relevant, such migration patterns most likely apply to relatively educated individuals. Model 4 in Table 2 therefore drops mothers with more than primary education. The relevant coefficient sizes are somewhat smaller than before but remain substantively and statistically significant.[30]

Co-ethnicity with whom? As an alternative to the EPR data on ethnic power constellations, we use Francois, Rainer and Trebbi's (2015) data on cabinet members' ethnic identities. We replicate our baseline model for the subset of 13 countries and 44 years for which both minister data and DHS birth records are available. Results remain similar to the EPR analysis, especially if we define our treatment as coethnicity with Francois, Rainer and Trebbi's (2015) "top government" ministers (see Table A11). These results are consistent with our argument that not only the leader but also powerful ethnic coalition partners have control over distributive spending.

Alternative DHS Outcomes. We capture the provision and use of health services more directly through DHS data on prenatal and birth assistance, the use of birth clinics, and newborns' weight. Unfortunately, while mothers report the survival and death of all their children, these items are only enumerated for children born within five years before each survey. The respective sample is therefore smaller and covers a much shorter time span than our baseline data. Nonetheless, both cross-sectional (Table A12) and within-district and ethnic group estimates (Table A13) reveal similar main and interactive effects

[30]A lower likelihood of less educated respondents to report infant deaths may also cause this divergence.

of individual and district level co-ethnicity with the government as observed in the infant mortality specifications.

Heterogeneous effects. Finally, we investigate whether democratic institutions moderate our findings as they constrain executive governments.[31] The mixed results presented in Appendix A3 suggests that democratic institutions may reduce but not eliminate favoritism. An exploration of the impact of varying electoral systems on ethnic favoritism yields no statistical difference between PR and first-past-the-post systems. These findings suggest that executive governing coalitions have incentives to favor their ethnic constituents even in the absence of democratic elections and regardless of specific electoral rules.

Evidence on ethnic favoritism from the Afrobarometer

We argue that governments provide public goods in co-ethnic districts and more narrowly target co-ethnics in non-co-ethnic districts. While the main analysis tests the distributional consequences of the argument, our data on infant mortality are unable to distinguish between public and private goods provision. To explore this question in more detail, we turn to Afrobarometer (2015) survey data. These data contain information on governments' provision of public services, thus allowing to test whether governments provide non-excludable goods to co-ethnic districts. In addition, we analyze whether patterns in households' economic well-being coincide with our findings on infant mortality.

We link the geocoded Afrobarometer rounds 1 to 5 (Afrobarometer, 2015; Ben Yishay et al., 2017) to the EPR and SIDE data using the same procedures as described above (see also Appendix A4). We then estimate the effect of individual- and district-level government co-ethnicity on the reported ease of (1) accessing public services and (2) households' economic well-being based on questions on how often respondents have "gone without" food, water, health care, and income. Below, we report effects on principal components

[31] Previous studies provide mixed findings on the effect of democracy on favoritism (Burgess et al., 2015; Hodler and Raschky, 2014; Franck and Rainer, 2012; Kramon and Posner, 2016).

that summarize items in both categories, and those items that directly relate to health care and thus infant mortality.[32] Because the Afrobarometer data lacks the temporal depth necessary for difference-in-differences models, we only exploit cross-sectional information within survey rounds in the same country. Omitted variables at the ethnic group and/or district-level might therefore bias the results below.

Table 3: Economic hardship and public services: Cross-sectional OLS

	Ease of public service access		Economic hardship	
	PC	Medical services (1-4)	PC	Medical treatment (0-4)
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−0.100	0.056	−0.382***	−0.239***
	(0.162)	(0.088)	(0.136)	(0.088)
Dist. Share Gov. Co-Ethnics (t-1)	0.284**	0.165***	−0.745***	−0.445***
	(0.125)	(0.063)	(0.117)	(0.072)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	0.117	−0.076	0.504***	0.307***
	(0.211)	(0.107)	(0.168)	(0.106)
Individual-level covariates:	yes	yes	yes	yes
Country-survey fixed effects:	yes	yes	yes	yes
Observations	26,418	38,950	64,026	70,432
Adjusted R^2	0.076	0.055	0.150	0.123

Notes: OLS linear models. Control variables include 4 levels of education, age and age squared, as well as a female dummy. Two-way clustered standard errors in parentheses (language group and district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Model 1 in Table 3 shows that public services such as health care and primary schooling are more accessible to respondents in co-ethnic districts. However, there is no association between individual-level government co-ethnicity and public service access. This pattern also holds with regard to accessing medical services (Model 2) and supports the argument that co-ethnic districts receive more public goods.

Turning to households' well-being, Model 3 shows a negative association between government co-ethnicity at the individual and district-level with the economic hardship factor. In line with our main analysis, there is no distinguishable difference between co-ethnics and non-co-ethnics in districts with a majority of government co-ethnics. The same pattern affects the reported frequency with which respondents or their family members lack access to medicines or medical treatment (Model 4). These findings closely align with our main results.

[32]Appendix A4 reports disaggregated analyses.

In combination, the results suggest that while co-ethnics in non-co-ethnic areas may not have better access to *public services* than their neighbors, they are substantively better off than non-co-ethnics. This divergence is consistent with our argument since *private goods* such as subsidies and jobs provided by their government may explain the divergence between access to public services and households' actual access to food, water, and health care.

Conclusion

This paper studies how local ethnic demography moderates patterns of ethnic favoritism in 22 African countries. We argue that governments try to favor their co-ethnics at minimal cost, tailoring the types of goods they provide to local ethnic population shares. Co-ethnic government strongholds receive locally non-excludable public goods that also benefit non-co-ethnic minorities. Where governments' co-ethnics are in the minority, in contrast, they discriminate and reach their peers with more narrowly targeted goods and services.

We take these predictions to data on infant mortality and district-level ethnic demography in Sub-Saharan Africa that allow us to disentangle the effects of individual and district-level government co-ethnicity. Our results are consistent with the provision of (1) non-excludable public goods to ethnic government strongholds and (2) ethnically targeted local or individual-level handouts to co-ethnics in opposition districts. Infants born into an ethnic group in power have substantially higher chances of survival, regardless of where they live. Conversely, government non-co-ethnics only have comparably low mortality rates if they are born in districts in which governing groups constitute the local majority. These effects are identified from temporal variation within ethnic groups and districts only, making it unlikely that they are caused by omitted variable bias or reverse causality.

The insight that ethnic geography affects strategies of favoritism has broader implications for the study of distributive politics in multi-ethnic societies. First, we highlight the importance of local ethnic demography for understanding the interplay between the ethno-political macro and micro levels. Second, we build on previous literature and stress that governments choose between locally non-excludable public goods and individually targeted handouts when designing distributive policies. Unfortunately and absent better data on public and private goods provision, we are not yet able to optimally operationalize these different strategies with cross-national data. Third and in line with Ichino and Nathan (2013), voters may adjust their voting behavior to governments' goods provision strategies. Non-segregated ethnic geographies and other incentives for the provision of public goods may thus reduce the political salience of ethnic identities and identity-based distributive conflicts.

The dire effects of ethnic favoritism on children's lives suggest that equitable development policy should focus on those who are disenfranchised from government services. Our findings can be used to identify those citizens least likely to be served by the government based on the interplay between their geographic location and ethnic identity. Citizens that are not ethnically represented in government *and* reside in areas where the majority shares that fate are least likely to benefit from government services. This information can help to target aid at the most vulnerable and alleviate ethnic inequality.

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Who Benefits? How Local Ethnic Demography Shapes Political Favoritism in Africa

ONLINE APPENDIX

This Online Appendix provides additional results, plots, and tables that are referenced but not reported in the main body of the paper "Who Benefits? How Local Ethnic Demography Shapes Political Favoritism in Africa." Section A1 contains descriptive statistics and maps, Section A2 reports the DHS-based robustness checks and heterogeneity analyses discussed in the main paper, while Section A3 provides more extensive information on the data, methods, and results behind the Afrobarometer results shown in Table 3 in the main paper.

A1 Summary statistics

Table A1: Summary Statistics (DHS Data)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Infant Death	1,540,884	10.749	30.973	0	0	0	100
Government Co-Ethnic (t-1)	1,540,884	0.575	0.494	0	0	1	1
District Share Government Co-Ethnics (t-1)	1,540,884	0.579	0.396	0.000	0.096	0.948	1.000
Senior Government Co-Ethnic (t-1)	1,540,884	0.296	0.456	0	0	1	1
District Share Senior Government Co-Ethnics (t-1)	1,540,884	0.298	0.357	0.000	0.015	0.634	0.993
Upgrade to Political Inclusion	1,540,884	0.009	0.095	0	0	0	1
Downgrade to Political Exclusion	1,540,884	0.004	0.060	0	0	0	1
Mother's education	1,540,884	1.538	0.749	1	1	2	4
Mother's age	1,503,930	24.463	6.526	10.000	19.000	29.000	49.000
Birthorder	1,540,884	3.406	2.305	1	2	5	18
Female	1,540,884	0.460	0.498	0	0	1	1
Twin or Higher Multiple Birth	1,540,884	0.034	0.181	0	0	0	1
Polity IV > Median	1,540,731	0.439	0.496	0.000	0.000	1.000	1.000
VDEM Polyarchy > Median	1,540,790	0.487	0.500	0.000	0.000	1.000	1.000
FPTP Electoral System	1,030,592	0.681	0.466	0.000	0.000	1.000	1.000
Year	1,540,884	1,995.157	9.940	1,955	1,988	2,003	2,013

Table A1 shows summary statistics for all variables used in the infant mortality models reported in the main body of the paper and in Section A2 below. Table A2 reports results from a validation exercise of the DHS-based SIDE data from which we computed district-level co-ethnicity shares with the government. More specifically, we estimated linear models using all geocoded infants' individual co-ethnicity with the governing coalition as the outcome and the SIDE district-level share of government co-ethnics as the only explanatory variable. These models yield coefficients close to one, regardless of whether we use survey-round-specific fixed effects at the country, subnational region, or district level. These results increase our confidence that the SIDE interpolations coincide with the raw DHS data we use in our main analyses. Remaining deviations from one are arguably

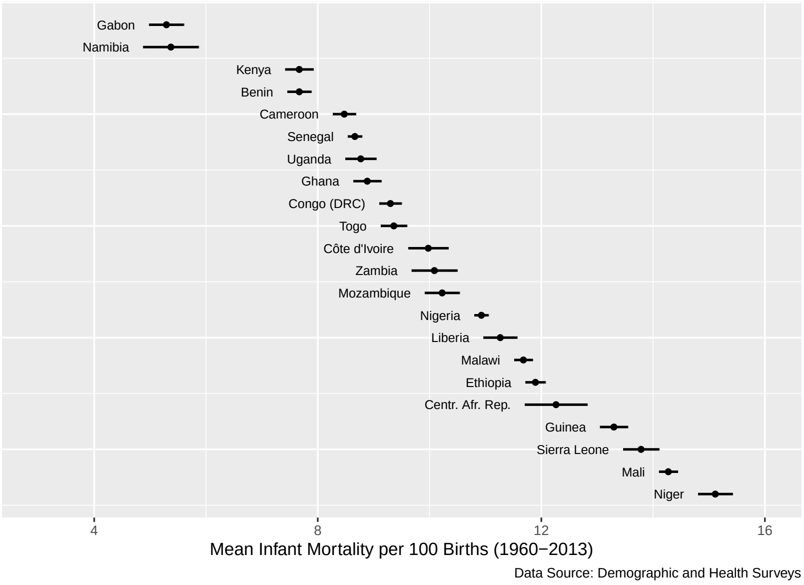


Figure A1: Mean infant mortality in the 22 countries in our sample, 1960–2013 (means and 95% confidence intervals)

due to limited amounts of measurement error or slightly different fertility rates[33] between different ethnic groups.

Table A2: Regressing Mothers' Individual Co-Ethnicity on District Share

	Individual Government Co-Ethnicity			
	(1)	(2)	(3)	(4)
Share of Dist. Pop. Included	1.022***	1.063***	1.045***	1.024***
	(0.006)	(0.009)	(0.010)	(0.006)
Country-Survey-Round FE	no	yes	–	–
Survey-Round-Region FE	no	no	yes	–
Survey-Round-District FE	no	no	no	yes
Controls	no	no	no	no
Observations	1,616,534	1,616,534	1,616,534	1,616,534
Adjusted R^2	0.663	0.665	0.671	0.687

Notes: OLS linear probability models. The sample mean of the dependent variable is 0.583. Observations are weighted to ensure equal weights for each country. Clustered standard errors in parentheses (country-survey-round). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Figure A3 maps the district-level co-ethnicity variable in 2000 for the 18 countries in our sample not mapped in Figure 1 in the main article. Figure A4 depicts the spatial distribution of all 19'622 geocoded DHS survey clusters that we use in our analyses to match infants to districts.

Figure A5 provides information on which ethnic groups are coded by the Ethnic

[33]This is because SIDE is estimated on the basis of the ethnic identities of adults and not their children.

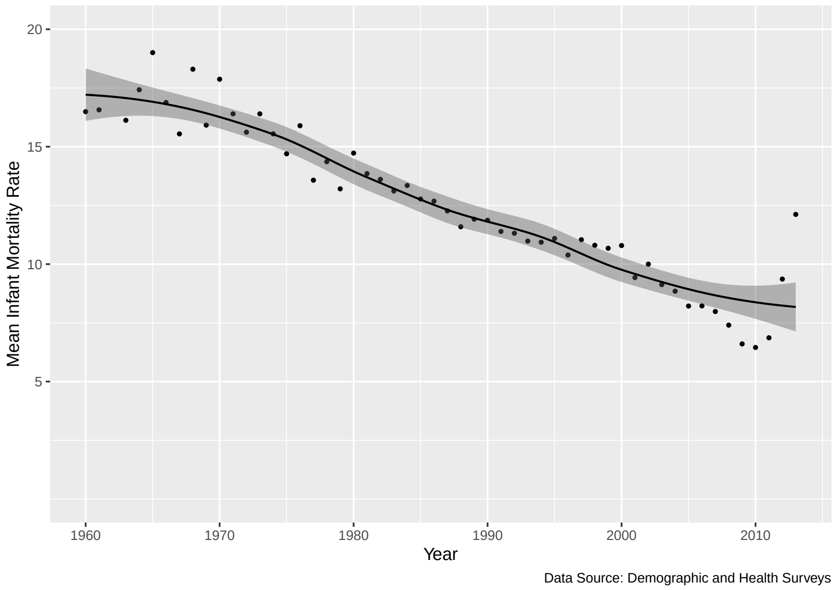
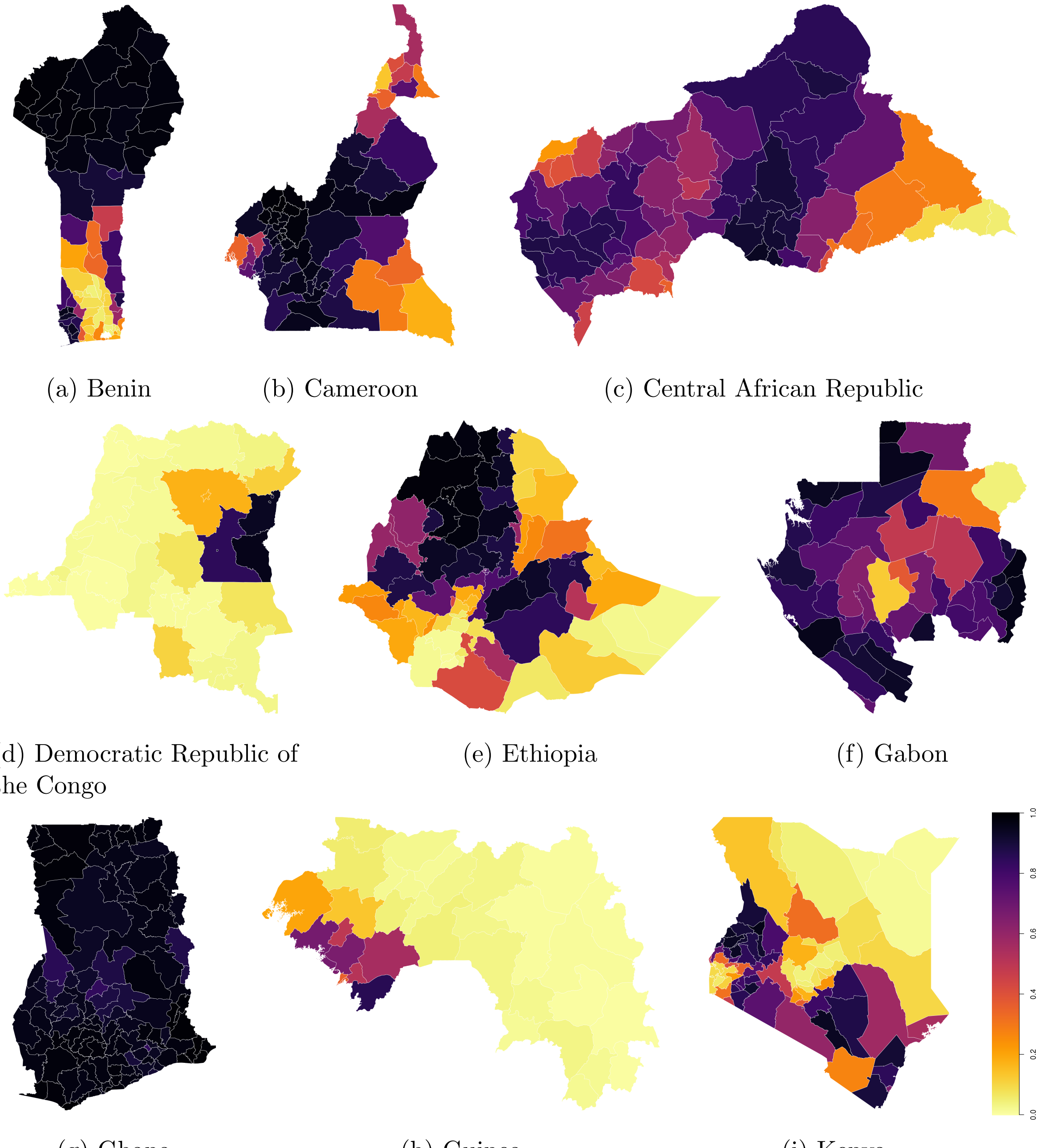


Figure A2: Annual infant mortality per 100 births across the 22 African countries in our sample (mean, LOESS curve and 95% confidence interval)

Power Relations database (Vogt et al., 2015) as being included in the government for every year in the 22 countries for which we have geocoded DHS data. Since our main models only exploit variation within ethnic groups and districts, all effects are identified from temporal changes in individual and district-level co-ethnicity with the government due to the political upgrades and downgrades visualized by the Figure.

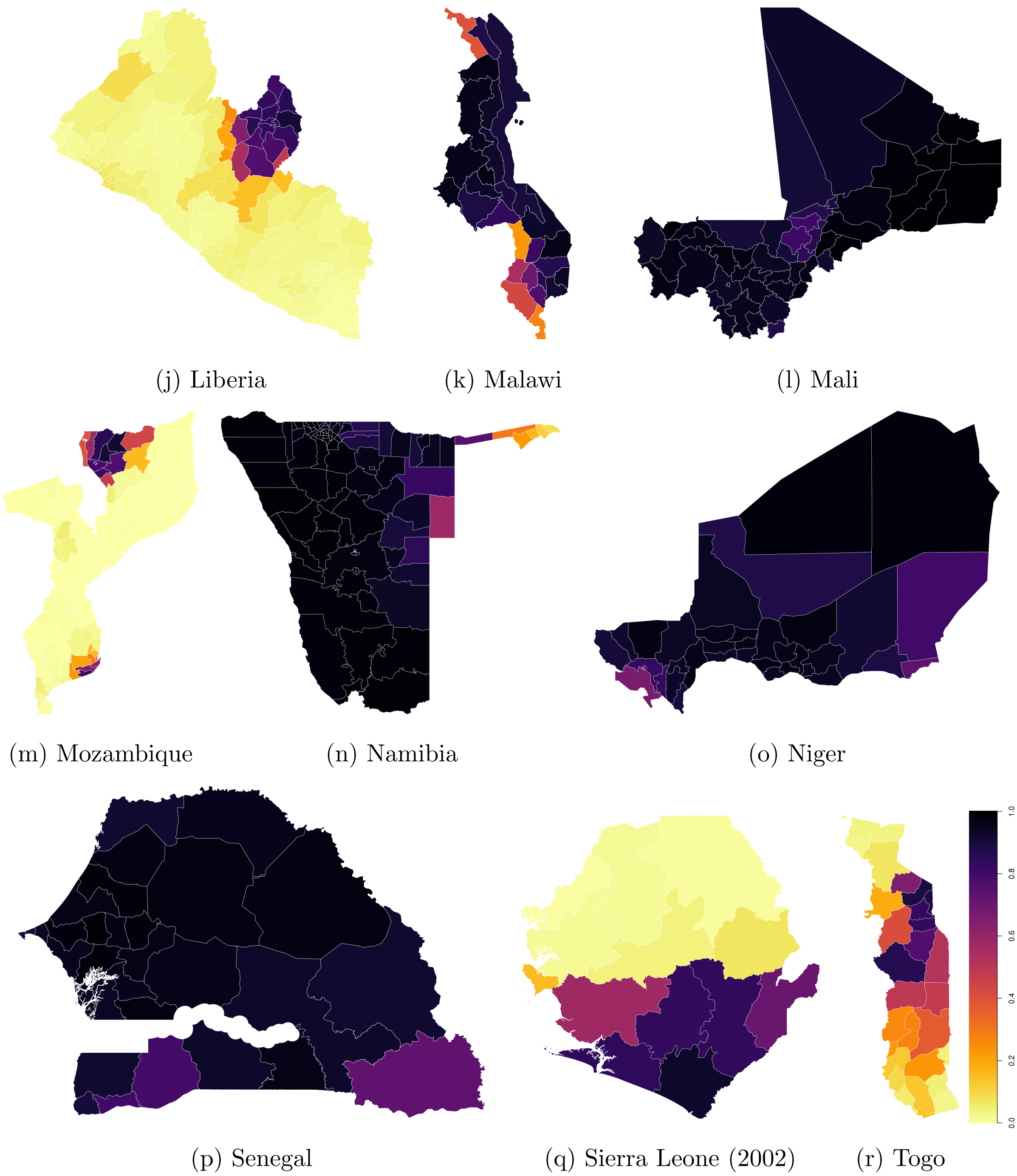


(a) Benin (b) Cameroon (c) Central African Republic

(d) Democratic Republic of the Congo (e) Ethiopia (f) Gabon

(g) Ghana (h) Guinea (i) Kenya

Figure A3: District-level co-ethnicity with the government in 2000 using the most recent available SIDE data.
See Figure 1 in the main article for maps of Côte d'Ivoire, Nigeria, Uganda, and Zambia.



(j) Liberia (k) Malawi (l) Mali

(m) Mozambique (n) Namibia (o) Niger

(p) Senegal (q) Sierra Leone (2002) (r) Togo

Figure A3: Continued

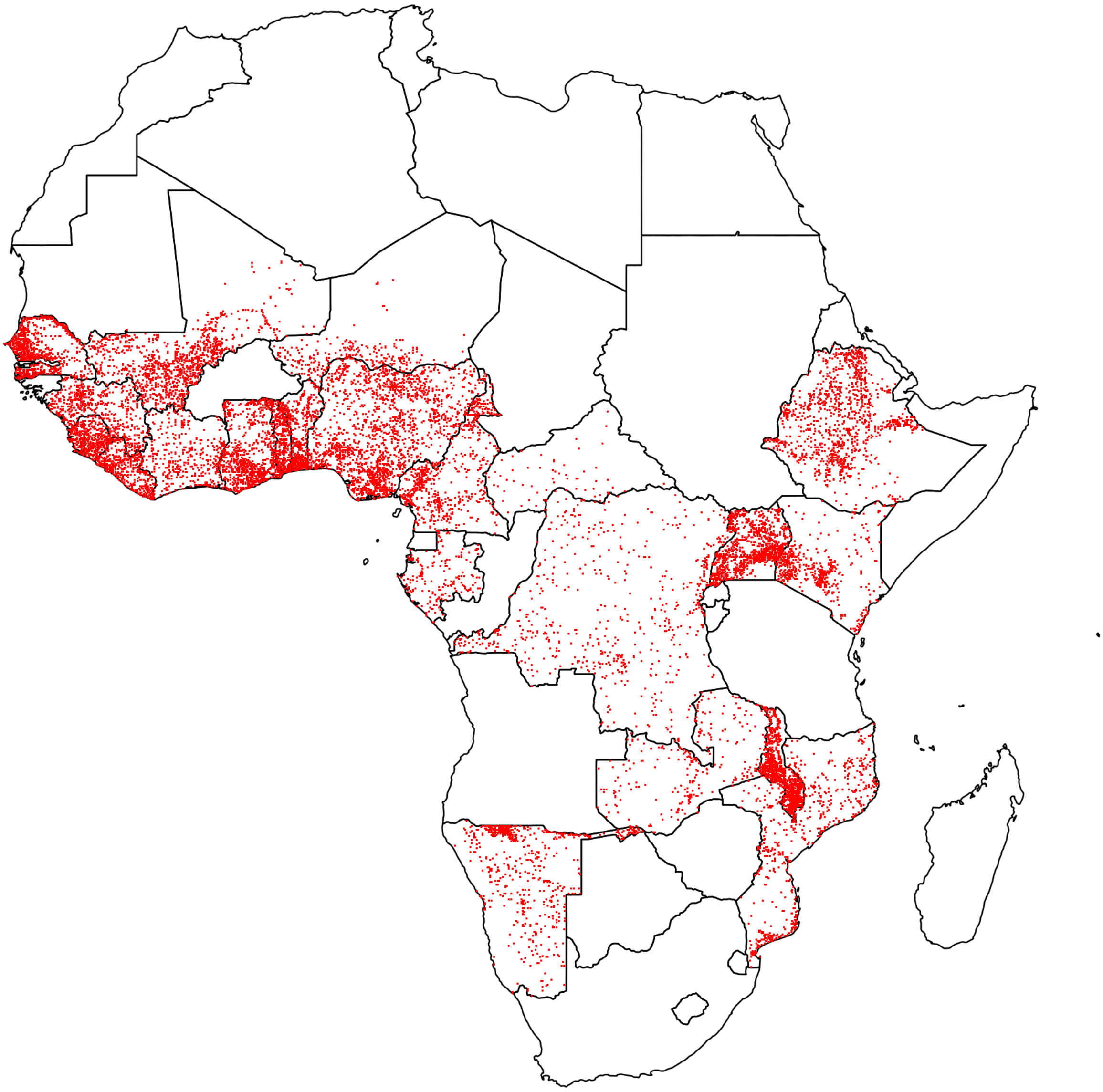


Figure A4: Geocoded DHS respondents across all rounds. Each point corresponds to one sampling cluster

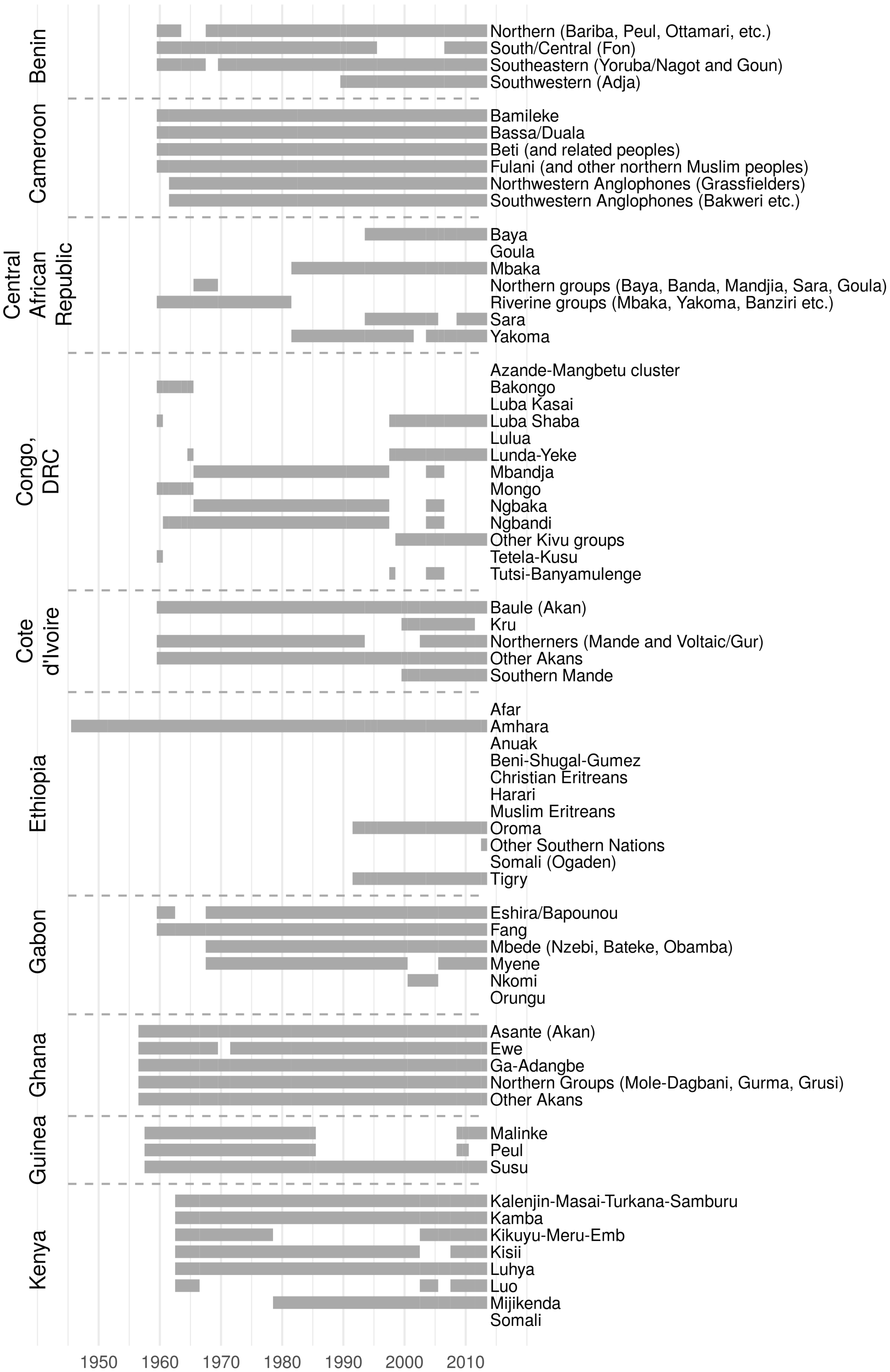


Figure A5: Inclusion of ethnic groups into governments in our sample.
Grey bars show spells within which EPR groups are coded as being included in the national executive. Groups are coded as ‘excluded’ or ‘irrelevant’ during all other spells. The politically relevant constellation of ethnic groups at times changes, with larger clusters of ethnic groups forming or dissolving (see e.g. Uganda).

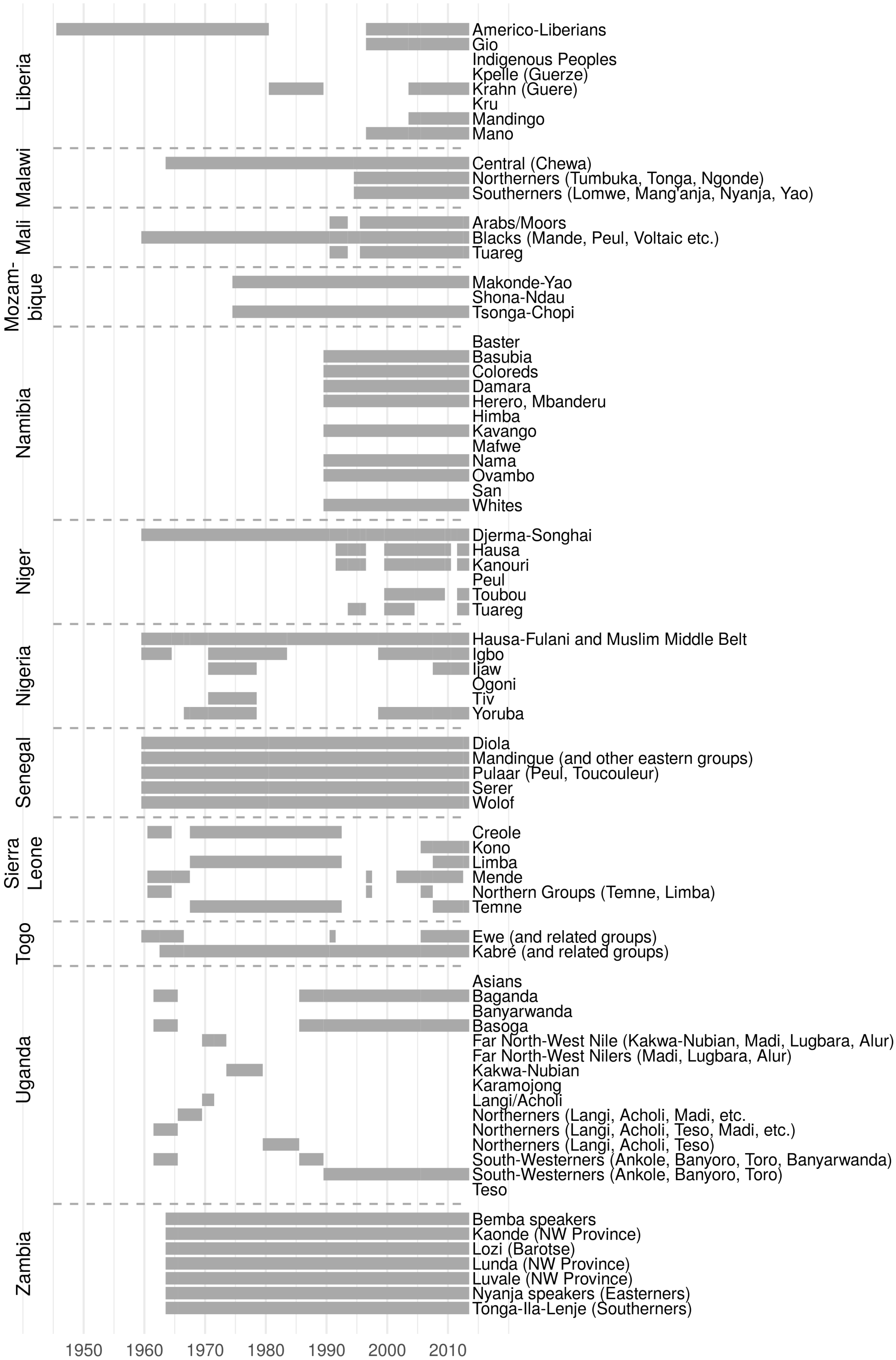


Figure A5: Continued.

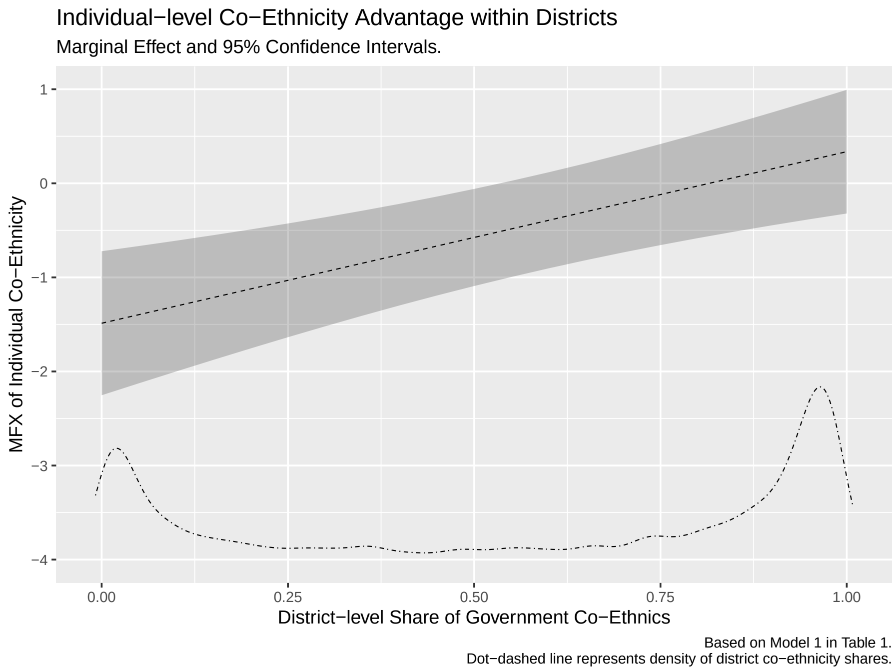


Figure A6: Marginal Effect of Individual-Level Government Co-Ethnicity across District-level Co-Ethnicity Shares

A2 Robustness checks

Choice of interaction model: Hainmueller, Mummolo and Xu (2019) have recently highlighted two problems with conventional multiplicative interaction models. First, the functional form imposes a linear interaction assumption requiring the effect of the treatment to linearly increase/decrease at a constant rate along the range of the moderator. Second, observations at extreme values of the moderator often do not exhibit sufficient variation in the treatment variable (lack of common support) leading to unreliable estimates. As a remedy, Hainmueller, Mummolo and Xu (2019) propose a simple binning estimator. The intuition of this method is to evaluate the marginal effects of a key variable of interest (in our case individual-level government co-ethnicity) at typically low, intermediate, and high values of the continuous moderator (district-level share of co-ethnics). We split our sample in three groups of district-level co-ethnicity using 1/3 and 2/3 as intuitive cut points. We then choose the median within the low (0.05), medium (0.51), and high category (0.94) as evaluation points for less parametrically estimated conditional marginal effects and linear predictions. Figures A7 and A8 plot the binning

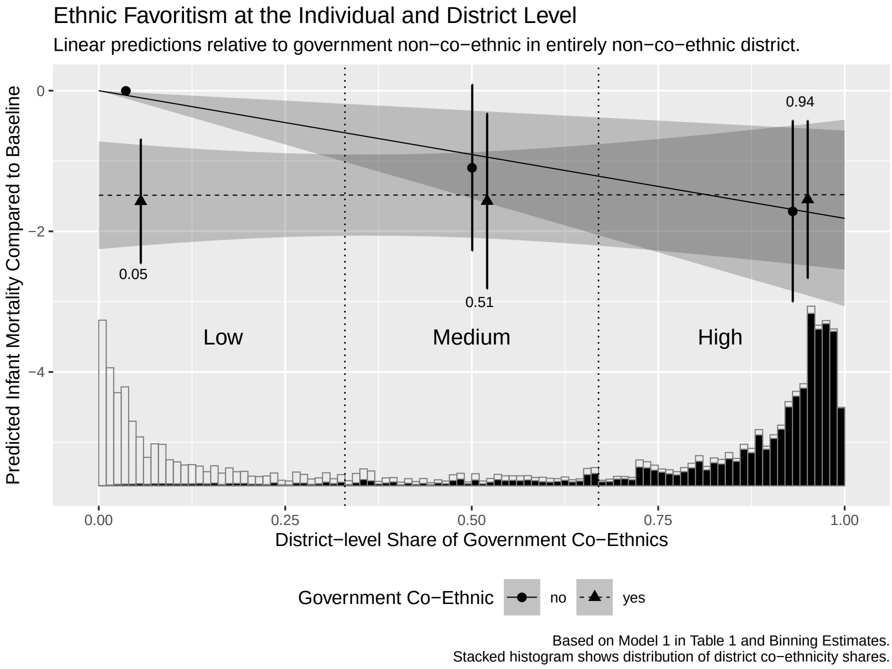


Figure A7: Predictions from Baseline Model & Binning Estimates

results on top of our baseline prediction and marginal effect graphs. The binning estimates align closely with the more conventional estimation strategy of our baseline models suggesting that neither functional form assumptions nor extrapolation to areas without common support explain our findings.

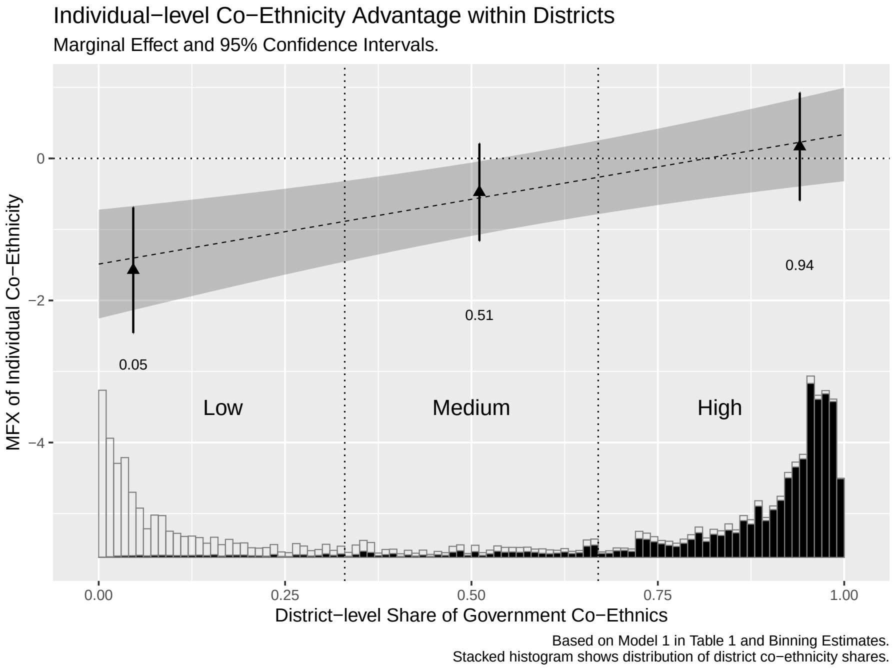


Figure A8: Marginal Effect of Individual Government Co-Ethnicity from Baseline Model & Binning Estimates

Lagged independent variables: To test whether our results are sensitive to various temporal specifications of our indicators for co-ethnicity with the government at the individual- and district-level, Table A3 presents the results of four specifications that run from non-lagged variables to three-year lags. Consistent with the intuition that ethnic favoritism affects infant mortality with a slight but not extensive temporal lag, the effects are strongest in the model with one-year lags, which is our baseline model throughout the paper. However, the results remain consistent in the other specifications. Only the three-year lag of the district-level share of government co-ethnics fails to reach statistical significance.

Table A3: Different Temporal Lags

	Infant Mortality			
	t	t-1	t-2	t-3
Government Co-Ethnic (t)	-1.360^{***}			
	(0.380)			
Dist. Share Government Co-Ethnics (t)	-1.687^{***}			
	(0.628)			
Co-Ethnic × Dist. Share Co-Ethnics (t)	1.936^{***}			
	(0.492)			
Government Co-Ethnic (t-1)		-1.487^{***}		
		(0.391)		
Dist. Share Government Co-Ethnics (t-1)		-1.816^{***}		
		(0.637)		
Co-Ethnic × Dist. Share Co-Ethnics (t-1)		1.824^{***}		
		(0.503)		
Government Co-Ethnic (t-2)			-1.394^{***}	
			(0.377)	
Dist. Share Government Co-Ethnics (t-2)			-1.484^{**}	
			(0.602)	
Co-Ethnic × Dist. Share Co-Ethnics (t-2)			1.824^{***}	
			(0.506)	
Government Co-Ethnic (t-3)				-1.234^{***}
				(0.445)
Dist. Share Government Co-Ethnics (t-3)				-1.103^{*}
				(0.583)
Co-Ethnic × Dist. Share Co-Ethnics (t-3)				1.788^{***}
				(0.534)
Survey-Ethnic FE	yes	yes	yes	yes
Survey-District FE	yes	yes	yes	yes
Survey-Region-Birthyear FE	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Observations	1,512,026	1,503,930	1,497,112	1,490,227
Adjusted R^2	0.060	0.060	0.060	0.060

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.78 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Clustering standard errors: Table A4 reports standard errors clustered at different levels. Here, we cluster standard errors at increasingly large geographical units while also clustering on year:

1. DHS enumeration area (to account for the DHS sampling design) & year
2. Administrative district (2^{nd}-level administrative unit) & year
3. Region (1^{st}-level administrative unit) & year
4. Country & year

The standard errors of our main independent variables remain very close to our baseline models and do not reduce statistical significance.

Table A4: Differently Clustered Standard Errors

	Infant Mortality U1			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	-1.487^{***}	-1.487^{***}	-1.487^{***}	-1.487^{***}
	(0.444)	(0.457)	(0.424)	(0.465)
Dist. Share Gov. Co-Ethnics (t-1)	-1.816^{**}	-1.816^{**}	-1.816^{**}	-1.816^{***}
	(0.821)	(0.816)	(0.747)	(0.662)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.824^{***}	1.824^{***}	1.824^{***}	1.824^{***}
	(0.503)	(0.516)	(0.480)	(0.520)
SE Clustering	EA & Year	Dist. & Year	Region & Year	Country & Year
Survey-Ethnic FE	yes	yes	yes	yes
Survey-District FE	yes	yes	yes	yes
Survey-Region-Birthyear FE	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Observations	1,503,930	1,503,930	1,503,930	1,503,930
Adjusted R^2	0.060	0.060	0.060	0.060

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.78 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared, as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Differently clustered standard errors in parentheses. Significance codes: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Unweighted regressions: Due to the variation in the number of DHS surveys and respondents per country, all main specifications are weighted so that each country receives equal weight. Table A5 tests whether our results are robust to that modelling decision and presents the results from estimating the models from Table 1 without any weights. The coefficients for individual-level co-ethnicity with the government remain stable to that change, while the effect of the district-level share of co-ethnics drops in size and becomes insignificant once we add ethnic birth-year fixed effects. The interaction term remains stable. Such deviations are to be expected if it is indeed the case that those countries that are politically unstable and therefore undersampled by the DHS have a slightly higher propensity for ethnic favoritism.

Table A5: Infant Mortality: Unweighted Regressions

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	-1.332^{***}		-1.455^{***}	
	(0.356)		(0.387)	
Dist. Share Gov. Co-Ethnics (t-1)	-1.327^{**}	-0.889		
	(0.562)	(0.672)		
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.793^{***}	1.920^{***}	1.972^{***}	1.970^{***}
	(0.453)	(0.473)	(0.490)	(0.521)
Survey-Ethnic FE	yes	–	yes	–
Survey-District FE	yes	yes	–	–
Survey-Region-Birthyear FE	yes	yes	–	–
Survey-Ethnic-Birthyear FE	no	yes	no	yes
Survey-District-Birthyear FE	no	no	yes	yes
Controls	no	no	no	no
Observations	1,503,930	1,503,930	1,503,930	1,503,930
Adjusted R^2	0.053	0.053	0.056	0.056

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.77 infant deaths per 100 live births. Control variables include mothers' age and age squared, as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Dropping control variables: As a further robustness test, we check whether results are in any way driven by the specific control variables we include in the models (mothers' age and age squared as well as infants' sex, birth rank, birth rank squared, and a twin dummy). Re-estimating our baseline models without any control variables yields almost identical coefficient estimates and standard errors (Table A6).

Table A6: No Control Variables

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.412***		−1.588***	
	(0.391)		(0.461)	
Dist. Share Gov. Co-Ethnics (t-1)	−1.993***	−1.882**		
	(0.689)	(0.905)		
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.807***	1.978***	1.846***	1.858***
	(0.519)	(0.576)	(0.547)	(0.599)
Survey-Ethnic FE	yes	–	yes	–
Survey-District FE	yes	yes	–	–
Survey-Region-Birthyear FE	yes	yes	–	–
Survey-Ethnic-Birthyear FE	no	yes	no	yes
Survey-District-Birthyear FE	no	no	yes	yes
Controls	no	no	no	no
Observations	1,540,884	1,540,884	1,540,884	1,540,884
Adjusted R^2	0.040	0.042	0.049	0.051

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.77 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Co-ethnicity with senior partners only: To check the robustness of our results with regards to modifying our theoretical assumption that all coalition partners matter equally (see p. 6 in the main paper), Table A7 estimates our baseline model using co-ethnicity with only the senior ethnic groups[34] in government as the main independent variables. While the patterns of ethnic favoritism towards individual co-ethnics in non-co-ethnic districts and favoritism to co-ethnic districts hold, the estimated effects are smaller than at baseline. The reason for these smaller effects is that all members of "junior partner" ethnic groups are now falsely attributed to the control group.

[34] The respective information is coded from the EPR data (Vogt et al., 2015).

Table A7: Government Senior Partners

	Infant Mortality			
	(1)	(2)	(3)	(4)
Senior Government Co-Ethnic (t-1)	-1.038^{***} (0.350)		-1.173^{***} (0.391)	
Dist. Share Senior Gov. Co-Ethnics (t-1)	-1.298^{**} (0.610)	-1.659^{**} (0.762)		
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	2.277^{***} (0.582)	2.582^{***} (0.613)	2.462^{***} (0.643)	2.753^{***} (0.661)
Survey-Ethnic FE	yes	–	yes	–
Survey-District FE	yes	yes	–	–
Survey-Region-Birthyear FE	yes	yes	–	–
Survey-Ethnic-Birthyear FE	no	yes	no	yes
Survey-District-Birthyear FE	no	no	yes	yes
Controls	yes	yes	yes	yes
Observations	1,503,930	1,503,930	1,503,930	1,503,930
Adjusted R^2	0.060	0.061	0.068	0.069

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.78 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Ethnic and district Diff-in-Diffs: To assess whether our results are comparable with the two empirical strands of the ethnic favoritism literature – one focusing on individual-level co-ethnicity, and the other on geographic regions – Table A8 presents results from straightforward ethnic group and district difference-in-differences estimations. Both approaches yield the expected results. Individual-level co-ethnicity with the government increases the expected rate of infant survival by .6 percentage points (Models 1 and 2). This is similar to Franck and Rainer (2012), who estimate an effect of .4 percentage points. Increasing the share of co-ethnics in the district in which an infant is born from 0 to 100 percent is associated with an increase in the infant's chance of surviving by about 1.6 percentage points (Models 3 and 4). Both estimates are highly significant. Note however that the aggregate effects are smaller than estimated in our more complex baseline model that includes both indicators and their interaction. This may be due to these simple models averaging over the heterogeneous strategies of ethnic favoritism targeted to co-ethnic individuals in some places and entire regions in others.

Table A8: Infant Mortality: Ethnic vs. District-level Diff-in-Diff

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−0.641***	−0.678***		
	(0.198)	(0.200)		
Dist. Share Gov. Co-Ethnics (t-1)			−1.594**	−1.443**
			(0.640)	(0.639)
Survey-Ethnic FE	yes	yes	no	no
Survey-District FE	no	no	yes	yes
Survey-Region-Birthyear FE	yes	yes	yes	yes
Controls	no	yes	no	yes
Observations	1,540,884	1,503,930	1,540,884	1,503,930
Adjusted R^2	0.036	0.056	0.040	0.059

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.78 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Controlling for pre-trends: To better gauge whether potential violations of the parallel-trends assumption drive our results, Table A9 adds a series of different pre-treatment indicators to the model. In particular, Model 1 adds two dummies that indicate whether an infant is born within the year of an upgrade to (or downgrade from) power of its ethnic group. Model 2 adds three individual yearly dummies prior to upgrades and downgrades, respectively. Models 3 and 4 follow a similar strategy, but now add a trend-variable that increases towards an upgrade (or downgrade) while being coded 0 for all observations outside the defined pre-trend ranges. These ranges are defined as comprising the 3 and 5 years prior to a change. In line with the definition of our main variables of interest, we lag these trend variables by one year.[35] Only two terms are sizable and significant. First, the downgrade term in Model 1 indicates that represented groups benefit more in the final year of their governing spell than in other years in power. The three-year trend before a downgrade from power indicate the same pattern.

[35] See Hodler and Raschky (2014) for a similar strategy.

Table A9: Robustness: Pre-Trends

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.383***	−1.602***	−1.414***	−1.855***
	(0.399)	(0.424)	(0.423)	(0.463)
Dist. Share Gov. Co-Ethnics (t-1)	−1.798***	−2.065***	−2.058***	−2.379***
	(0.639)	(0.697)	(0.700)	(0.758)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.818***	1.938***	1.944***	2.207***
	(0.503)	(0.516)	(0.517)	(0.552)
Upgrade_t	−0.028	−0.191		
	(0.497)	(0.580)		
Upgrade_{t+1}		−0.669		
		(0.626)		
Upgrade_{t+2}		−0.867		
		(0.573)		
Downgrade_t	−1.635**	−1.143		
	(0.783)	(0.859)		
Downgrade_{t+1}		−1.053		
		(1.024)		
Downgrade_{t+2}		0.951		
		(0.788)		
Pre-Trend $\text{Upgrade}_{t+2 \text{ to } t}$			−0.117	
			(0.302)	
Pre-Trend $\text{Downgrade}_{t+2 \text{ to } t}$			−0.759**	
			(0.359)	
Pre-Trend $\text{Upgrade}_{t+4 \text{ to } t}$				−0.159
				(0.168)
Pre-Trend $\text{Downgrade}_{t+4 \text{ to } t}$				−0.008
				(0.172)
Survey-Ethnic FE	yes	yes	yes	yes
Survey-District FE	yes	yes	yes	yes
Survey-Region-Birthyear FE	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Observations	1,503,930	1,483,404	1,483,404	1,422,737
Adjusted R^2	0.060	0.060	0.060	0.059

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.78 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Is this all due to local ethnic segregation? One potential concern with our empirical analysis is the question whether the district is the right level of aggregation to test the difference between regional and more locally or even individually targeted ethnic favoritism. Within districts, DHS enumeration areas may vary widely in terms of geographic and other baseline conditions, which raises the specter of omitted variable bias. We address this concern by adding DHS enumeration area ("survey cluster") fixed effects to our baseline model. The estimated coefficients are somewhat smaller in size, but our main finding remains intact (Model 1 in Table A10).

Perhaps more worryingly, our results could emerge in the absence of any household-level handouts if governments target the same kind of local public goods to ethnically homogeneous sublocalities and villages regardless of district-level ethnic composition. Such uniform servicing of segregated local strongholds throughout the entire constituency is exactly what Ejdemyr, Kramon and Robinson (2018) find for Malawian MPs' local public goods provision strategies. In other words, our analysis risks boiling down to testing whether this logic travels beyond Malawi and also holds considering co-ethnicity with the national executive rather than more local-level legislative representatives.

Our theoretical argument and focus on the district level, however, yield two empirical implications at odds with a uniform provision of the same type of local public goods to segregated communities throughout the country's territory. First, our reasoning about the more fine-tuned targeting of co-ethnics should, in principle, also apply to spatial units below the district level. In mainly non-coethnic districts, we thus not only expect segregated co-ethnic enclaves, but also co-ethnic individuals residing in predominantly non-coethnic villages or urban neighborhoods to benefit from government favoritism. Second, we argue that broader types of goods benefiting entire districts matter too. Hence, we expect non-coethnics in government majority districts to benefit not only if they happen to live in mainly co-ethnic enumeration areas. Instead and in contrast to Ejdemyr, Kramon and Robinson (2018), we expect *segregated non-coethnic* villages and neighborhoods within mainly co-ethnic districts to profit from district-wide public goods.

We run additional models to test these two implications. First, we probe whether the

Table A10: Robustness: Enumeration Areas & Spatial Segregation

	Infant Mortality		
	(1)	(2)	(3)
Government Co-Ethnic (t-1)	−0.989**	−0.754*	
	(0.399)	(0.409)	
Dist. Share Gov. Co-Ethnics (t-1)	−1.319**		−1.389**
	(0.655)		(0.554)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	0.912*		
	(0.538)		
EA Share Gov. Co-Ethnics (t-1)		−1.284*	
		(0.703)	
Co-Ethnic × EA Share Co-Ethnics (t-1)		0.739	
		(0.594)	
EA Share Co-Ethnics (t-1)			−1.290**
			(0.579)
EA Share × Dist. Share Co-Ethnics (t-1)			1.617*
			(0.866)
Unit of Analysis	Ind.	Ind.	EA-Year
Survey-Ethnic FE	yes	yes	–
Survey-District FE	–	–	–
Survey-Cluster FE	yes	yes	yes
Birthyear FE	–	–	yes
Survey-Region-Birthyear FE	yes	yes	no
Controls	yes	yes	no
SE Clustering	Dist. & Ethn.	DHS EA & Ethn.	EA & Country-YoB
Observations	1,503,930	1,471,204	445,514
Adjusted R^2	0.068	0.069	0.066

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.78 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

individual-level coethnicity effect is a mere artifact of local spatial segregation. Model 1 in Table 2 in the main text includes DHS enumeration area $\times$ birthyear fixed effects. Results indicate that co-ethnic infants in mainly non-coethnic districts survive longer than non-coethnic infants born in the same year and locality. The individual-level co-ethnicity effect is not entirely driven by segregated co-ethnic pockets within mainly non-coethnic districts. Similar results emerge if we define our SIDE-derived regional co-ethnicity variable at the survey cluster instead of at the district level (Model 2 in Table A10). The coefficients are smaller and less precisely estimated but still suggest that even in predominantly non-coethnic enumeration areas, government co-ethnic infants are better off.

Second, we probe whether the district-level coethnicity effect on government non-coethnics is at least partially driven by segregated non-coethnic enclaves plausibly benefiting from district-wide public goods. To that end, we aggregate our observations to enumeration area years. More specifically, we calculate the mean mortality and government co-ethnicity across all infants born in the same year and DHS survey cluster. We then run a model with the mean infant mortality as dependent variable as well as mean co-ethnicity, the SIDE-derived district-level co-ethnicity variable, and their interaction as predictors. Both EA-level and, more importantly, district-level co-ethnicity still enter with large negative and statistically significant coefficients. Within mainly co-ethnic districts, entirely non-coethnic enumeration areas benefit as well. In short, neither our individual-level nor our district-level effects can be explained away by ethnic segregation at the very local level. In our view, these findings lend additional credence to our argument about differentiated goods provision strategies that carefully take into account subnational ethnic demography.

Alternative data on government coethnicity: Which branches and actors within African governments have control over distributive spending and are in a position to favor their co-ethnics? In our theoretical argument and empirical operationalizations, we make two key assumptions in line with previous scholarship on African politics. First, the executive branch has the most spending power in authoritarian and military regimes, electoral autocracies, and democracies alike. Second, leaders are not the only actors with spending power. Instead, they rely on the support of their ruling coalition which is, more often than not, ethnically diverse. As such, we expect both the leader and high-ranking members of the executive government coalition to be able to distribute goods towards their respective ethnic constituencies.

We regard our EPR-derived measure of government co-ethnicity as a useful proxy for 'real' representation in the executive ruling coalition (i.e. representation with some control over distributive spending). According to the EPR codebook, ethnic representation is coded for the executive branch only. EPR explicitly tries to distinguish "substantial" or "meaningful" from mere "token" representation, which we regard as important for our analysis. The codebook further instructs coders to make a qualitative assessment about "where political power is effectively exercised." As we understand the EPR coding instructions, this is, in most cases, the cabinet but allows for some flexibility to also focus on army or governing party elites in military and one-party states. We are fully aware that this flexibility comes at the cost of a somewhat impressionistic coding of our main explanatory variables. This potential for measurement error arguably attenuates our estimates as long as the EPR measure of inclusion does not systematically code growing political representation for groups with improving economic fortunes.

However, we want to make sure that the idiosyncracies of the EPR data are inconsequential for our analysis. Therefore, we check whether our results are robust to an alternative coding of ethnic ruling coalitions in the executive branch of government. Francois, Rainer and Trebbi (2015, henceforth FRT) code African cabinet ministers' ethnic affiliation for 15 countries and the time period 1960-2004. We have DHS data on infant mortality and ethnic maps from SIDE for 13 of these countries (Benin, Côte d'Ivoire, Guinea, Liberia, Sierra Leone, Ghana, Togo, Cameroon, Nigeria, Gabon, Democratic Republic of Congo, Uganda, Kenya). Besides its more limited geographic and temporal scope, the FRT data set differs from EPR in three main ways.

First, FRT exclusively focuses on cabinets. In practice though, this should not matter much as EPR country experts mainly justify their coding decisions with reference to presidents, prime ministers and high-ranking cabinet members (see the detailed case descriptions in the EPR Atlas at growup.ethz.ch) and FRT are flexible enough to accommodate military dictatorships by counting e.g. Military Council members in Nigeria (1966-1979 & 1984-1998) as cabinet ministers. Second, Francois, Rainer and Trebbi (2015) use different ethnic group categories than EPR. They mainly rely on the group lists provided by Alesina et al. (2003) and Fearon (2003) instead of the set of "politically relevant" ethnic groups in EPR. These ethnic group lists contain a greater number of, on average, smaller ethnic groups that are often closer to 'primordial' linguistic categories than in EPR, which frequently codes broader regional coalitions comprising several linguistic groups. Among the 13 countries for which we have data from both EPR and FRT, the average number of ethnic groups is 4.97 in EPR and 17.77 in FRT. The mean ethnic group's share in its country's total population is 14.8% in EPR compared to 5.6% in FRT.

Third, FRT code nominal representation in ministerial cabinets irrespective of individual ministers' effective influence within the coalition. Thus, the FRT data appears more objective but potentially less well suited to identify cabinet ministers with spending power. Fortunately, Francois, Rainer and Trebbi (2015, p. 474) attempt to rule out mere token inclusion as an explanation for their findings and code what they regard as the most influential cabinet positions ("Presidency/Premiership and deputies, Defense, Budget, Commerce, Finance, Treasury, Economy, Agriculture, Justice, and State/Foreign Affairs"). This definition of "top government" representation appears closer to EPR and more in line with our notion of group-level spending power within the executive ruling coalition.

We see advantages and disadvantages in both data sets. FRT offer more precise, objective measures of ethnic representation in ministerial cabinets. This gives us more gradual temporal variation than the EPR measure which seems to mainly/only capture highly visible government changes in the wake of e.g. coups, successful rebellions, and national-level elections. However, not all changes in cabinet composition fundamentally alter control over distributive resources. Cabinet reshuffles are frequent and some cooptation of elites from other than the leader's' ethnic group is mere window-dressing, even where it affects the portfolios that FRT regard as the most important. Appointed elites may well amass great personal rents but do not get enough power to distribute significant resources to their ethnic constituencies.

The, admittedly complex case of the successive military governments in Nigeria between 1984 and 1998 vividly illustrates the differences between both data sets and, more generally, the challenges of coding ethnic representation in African governments. EPR lists only six politically relevant ethnic groups/coalitions in Nigeria (Hausa-Fulani and Muslim Middle Belt, Yoruba, Igbo, Ijaw, Tiv, Ogoni) whereas FRT code cabinet representation (or the absence thereof) for 16 groups (Angas, Bura, Chamba, Edo, Fulani, Gbari, Hausa, Ibibio, Idoma, Igbirra, Igbo, Ijaw, Kanuri, Nupe, Tiv, Yoruba). EPR regards the entire period of military rule after the end of the Second Republic in 1983 as dominated by the Northern Hausa-Fulani and Muslim Middle Belt ethnic cluster and codes all other relevant groups as powerless (Yoruba and Igbo throughout, Ijaw and Ogoni until 1991) or discriminated (Ijaw and Ogoni from 1992 onward). The EPR country expert appears to base this coding on what she sees as Northern and Muslim control of the "ruling military government and the leading positions in the security forces (police, army, navy)." FRT, on the other hand, count members of both the successive military councils ("Supreme Military Council", "Armed Forces Ruling Council", "Provisional Ruling Council") and the parallel 'civilian' cabinets ("Federal Executive Council", "National Council of Ministers") as ministers. We can check minister counts of all FRT ethnic groups clearly overlapping with the EPR cluster "Hausa-Fulani and Muslim Middle Belt" (i.e. Hausa, Fulani, Gbari, Igbirra, Nupe) to see whether FRT also code the Nigerian military governments as dominated by these groups.

According to FRT, Hausa, Fulani and Muslim Middle Belt groups held 439.5 minister years between 1984 and 1998, Yoruba elites 335.5 minister years, and Igbo elites 185 suggesting that groups coded as excluded in EPR enjoyed numerically significant representation in Nigerian cabinets. This pattern is even more pronounced if we restrict comparisons to what FRT regard as the most important portfolios. The Yoruba have only four top minister years less than Hausa, Fulani, and related groups. All but one of FRT's 'top' portfolios (President/Commander-in-Chief) are located in the 'civilian' council that the EPR coder appears to regard as irrelevant. In the plausibly more important military councils, Hausa, Fulani, and the Middle Belt groups always had significantly more members than the Yoruba. However, all military council years between 1984 and 1998 had at least one Yoruba member.

Qualitative accounts of Nigerian military rule under Buhari, Babanginda, and Abacha suggest that their regimes were indeed dominated by the Hausa-Fulani and their Muslim and Northern allies. These groups' elites used e.g. redistricting reforms, federal revenue sharing formulas, and rampant patronage to favor their ethnic and regional peers (Abubakar, 2001; Bah, 2004). From that angle, EPR seems to get the gap in actual spending power between the Northern coalition and other groups roughly right. Whether this justifies to code groups like the Yoruba and Igbo as entirely excluded from 'meaningful' government representation despite their numeric control of important cabinet positions is more of a judgement call. While EPR errs on the side of exclusion and mainly concentrates on the leader and his closest allies, FRT overestimates the number of groups enjoying 'real' representation.

As our discussion of Nigerian military governments demonstrates, coding meaningful ethnic representation is fraught with uncertainties. Both available data sets most likely measure our key theoretical concepts with error. This makes it all the more important to check whether results are similar across independently collected data sets.

To replicate our analysis with the FRT minister data, we first match DHS ethnic categories (those stated by mothers and contained in the SIDE maps) to the ethnic groups in the FRT data. The matching procedure is equivalent to the DHS-EPR match described in the Data section of the main text. We then code both individual and SIDE-derived district-level coethnicity with the cabinet in three different ways: (1) coethnicity with the leader, (2) coethnicity with top cabinet members, and (3) coethnicity with any minister. We use these variables to estimate models that are equivalent to our baseline model 1 in Table 1. All models include control variables as well as FRT ethnic group × survey round, district × survey round, and region × survey round × year-of-birth fixed

Table A11: Robustness: FRT Data

	Infant Mortality		
	(1)	(2)	(3)
Leader Co-Ethnic (t-1)	-0.607		
	(0.524)		
Dist. Share Leader Co-Ethnics (t-1)	-3.089^{**}		
	(1.315)		
Leader Co-Ethnic × Dist. Share Leader Co-Ethnics (t-1)	1.874^{*}		
	(1.107)		
Top Gov. Co-Ethnic (t-1)		-0.729^{*}	
		(0.386)	
Dist. Share Top Gov. Co-Ethnics (t-1)		-1.537^{**}	
		(0.651)	
Top Gov. Co-Ethnic × Dist. Share Top Gov. Co-Ethnics (t-1)		1.515^{**}	
		(0.695)	
Gov. Co-Ethnic (t-1)			-0.868
			(0.554)
Dist. Share Gov. Co-Ethnics (t-1)			-3.295^{**}
			(1.526)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)			2.428^{***}
			(0.801)
Survey-Ethnic FE	yes	yes	yes
Survey-District FE	yes	yes	yes
Survey-Region-Birthyear FE	yes	yes	yes
Controls	yes	yes	yes
Observations	557,532	557,532	557,532
Adjusted R^2	0.055	0.055	0.055

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.04 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

effects. Despite a substantially reduced sample size, different definitions of ethnic groups, and an independent coding of government inclusion, the results align well with our main analyses. As expected, the coefficients for co-ethnicity with the most powerful cabinet members are closest to our EPR-based findings (Model 2 in Table A11). This seems consistent with our assumption that not only the leader but also top cabinet ministers from other ethnic groups are in a position to favor their constituencies. The coefficients for individual-level cabinet co-ethnicity are smaller and less precisely estimated than in our EPR models but the basic pattern remains intact.

Alternative Outcomes: Maternal Care & Child Health: Tables A12 and A13 investigate additional outcomes from the DHS surveys. Specifically, we consider whether a mother received prenatal assistance by a health professional, whether she received prenatal assistance by a doctor, whether she gave birth in a medical facility, whether the birth was assisted by a health professional, whether the birth was assisted by a doctor, and whether the child's birth weight was below 2500g. All these measures can be seen as proximate causes of infant death. Similar to the infant mortality measure, they cannot unambiguously disentangle private from public goods provision. Given, for example, the cost of health care at health centers, private wealth accumulated partly as a consequence of individual-level favoritism through jobs or handouts can increase access to maternal health-care (Theisen, Strand and Østby, 2020). We therefore view the following analyses as complementary to our baseline findings.

The DHS surveys only include questions on pregnancy-related health care and the place and type of assistance received during birth of children born within five years prior to a survey. Using the respective variables thus requires dropping all infants born in the more distant past which has two effects. First, the number of observations that can be included in these analyses (90-400 thousand) is much smaller than in our original analyses on infant mortality (1.5 million). Second and more importantly, the sample of children becomes much more concentrated in time. Since the DHS surveys have been conducted extensively only since the late 1990s, most children for which we have these additional outcomes are born in the years after 2000. Very few children with such information are

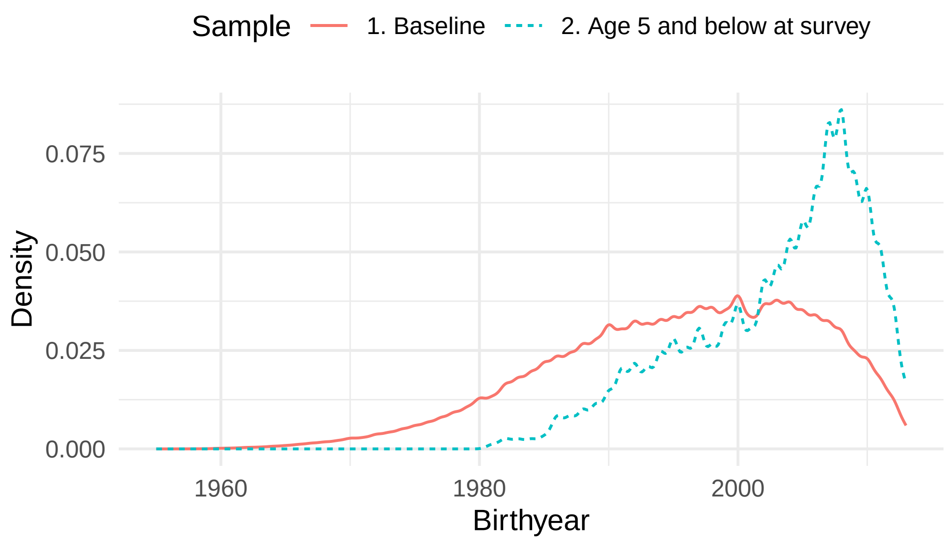


Figure A9: Distribution of birthyears in baseline sample and among infants with additional information

born in the 1980s and none before. This reduction in temporal coverage limits the number of changes in ethnic government composition that our fixed effects models exploit, in particular those changes associated with the third wave of democratization in the 1990s. This makes the inclusion of ethnic group and district fixed effects a much harder test than in the baseline analysis.

For these reasons we estimate the baseline specification with and without district as well as ethnic group fixed effects. Table A12 shows a specification in which we only include survey-region-birthyear fixed effects comparing children born in the same year and region and enumerated in the same survey round *across* districts and different ethnicities. The results on all five outcomes relating to mothers' access to healthcare during pregnancy and birth show very similar patterns as our baseline models and are statistically highly significant. In areas with low proportions of government co-ethnics, individual co-ethnicity improves women's access to care. Women in districts with high proportions of government co-ethnics have better access to care and in those districts the effect of individual co-ethnicity is offset. We also find individual co-ethnicity to have a significantly negative effect on newborns' probability to weigh less than 2500g. This association is offset in districts with high proportions of government co-ethnics. The constitutive term on district level co-ethnicity is negative but not statistically significant. Taken together, these cross-sectional estimates provide additional support for the predictions of our theory.

In Table A13, we replicate the analysis with ethnic group-survey round and district-survey round fixed effects only exploiting variation from temporal changes in ethnic government composition. These specifications yield estimates that support the pattern we find in the original models, are of similar size than those discussed above, but partially come with more statistical uncertainty. In all five models on access to healthcare before and during birth, individual-level co-ethnicity significantly increases access in districts with few co-ethnics and this effect decreases significantly as the proportion of co-ethnics in a district increases. In addition, in all five models on access to health care, the effect of the proportion of co-ethnics in a district is positive and in three models, it remains significant at least at the ten per cent level. In the model on low birthweight, all variables show the expected effect. While the constitutive terms are associated with higher uncertainty, the interaction term is statistically significant.

When we estimate the same specifications using the baseline measure of under-1 mortality as an outcome but restrict the sample to children born within 5 years prior to the survey interview, we find results that are consistent with the baseline analyses but come with greater uncertainty. This suggests that the reduction in sample size and temporal coverage indeed reduces the statistical power we can draw on to estimate the effects of changes in governments ethnic composition.

Interestingly, in some of these models, the interaction effect overcompensates the offsetting of the effect of individual co-ethnicity in districts with high proportions of co-ethnicity. This suggests that in these districts, co-ethnics have less access to care than non-co-ethnics. Under this specification, we exploit much fewer changes in government over time. It is possible that this finding is driven by cases where advantages in access to health care of a previously included small ethnic minority that has benefitted from private goods and has been *downgraded* in power persists at least for some years under a new ethnic coalition because overcoming the minority's privilege by providing public goods such as building hospitals requires some time.

Table A12: Alternative Outcomes, Children Age 5 and Below

	Outcome						
	Prenatal Asst.	Prenatal Doc.	Inst. Birth	Asst. Birth	Asst. Doctor	Low Weight	Dead
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Government Co-Ethnic (t-1)	8.181***	3.467***	9.173***	11.002***	2.632***	−3.390***	−0.877**
	(2.680)	(1.073)	(2.816)	(2.739)	(0.872)	(1.146)	(0.383)
Dist. Share Gov. Co-Ethnics (t-1)	7.646***	5.876***	13.270***	14.797***	4.756***	−1.667	−0.756
	(2.870)	(1.639)	(4.575)	(4.308)	(1.294)	(1.592)	(0.623)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	−9.391**	−4.845***	−12.153***	−15.180***	−4.364***	3.905**	1.304**
	(3.688)	(1.583)	(4.129)	(4.012)	(1.297)	(1.643)	(0.595)
Survey-Ethnic FE	no	no	no	no	no	no	no
Survey-District FE	no	no	no	no	no	no	no
Survey-Region-Birthyear FE	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes
Sample Mean DV	75.93	10.81	48.69	48.85	5.8	17.14	10.78
Observations	298,530	284,892	396,607	395,322	374,908	94,070	505,956
Adjusted R^2	0.295	0.150	0.263	0.266	0.094	0.080	0.071

Notes: OLS linear probability models. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Table A13: Alternative Outcomes, Children Age 5 and Below

	Outcome						
	Prenatal Asst.	Prenatal Doc.	Inst. Birth	Asst. Birth	Asst. Doctor	Low Weight	Dead
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Government Co-Ethnic (t-1)	4.392**	4.997***	8.148***	9.760***	2.598**	−4.913	−0.901
	(2.164)	(1.925)	(2.447)	(2.627)	(1.227)	(3.247)	(0.702)
Dist. Share Gov. Co-Ethnics (t-1)	4.309	3.029	6.476*	11.832***	2.663*	−4.775	−1.040
	(4.035)	(3.064)	(3.633)	(4.315)	(1.498)	(3.790)	(1.401)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	−8.054***	−6.998***	−15.627***	−17.438***	−4.529***	5.440**	0.770
	(2.554)	(1.509)	(3.328)	(3.114)	(1.374)	(2.268)	(0.904)
Survey-Ethnic FE	yes	yes	yes	yes	yes	yes	yes
Survey-District FE	yes	yes	yes	yes	yes	yes	yes
Survey-Region-Birthyear FE	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes
Sample Mean DV	75.93	10.81	48.69	48.85	5.8	17.14	10.78
Observations	298,530	284,892	396,607	395,322	374,908	94,070	505,956
Adjusted R^2	0.355	0.199	0.328	0.332	0.129	0.096	0.075

Notes: OLS linear probability models. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Household and/or Mother Fixed Effects Model 1 in Table A14 includes fixed effects for 314'909 households as well as year fixed effects, thus only exploiting variation in individual and district-level co-ethnicity of children born within the same household, including those born to mothers of subsequent generations. Only 1/3 of all households exhibit variation in the 'treatment' variables among their children, originating from changes in government over time and from mothers of different ethnicities who live together. In this model, all coefficients show the expected effect and are statistically significant ($p < .1$), thus confirming our original findings. When we include survey region birthyear fixed effects alongside the household fixed effects in Model 2 to control for regionally differing changes and shocks over time, point estimates remain similar in size but the coefficient of the variable on the proportion of co-ethnics in a district looses statistical significance. With regional variation absorbed by the fixed effects, there is less temporal variation in districts' co-ethnicity to exploit. It is therefore not too surprising that our estimates become associated with more uncertainty.

Model 3 includes 379'818 mother fixed effects as well as year fixed effects, thus only exploiting changes in government – and as a result co-ethnicity – of children born to the same mother. Because governments tend to be fairly stable over time (see Figure A5) and women give birth to children in a limited period of time, the statistical power to identify the effects of district- and individual-level co-ethnicity is again quite low: Only 1/3 of all mothers have given birth to children under differing ethnic compositions of their government. In addition, because these births typically occur in close temporal proximity to each other, pre- and post-change periods tend to be shorter than with the household fixed effects and reduce the estimates towards zero where ethnic favoritism has no immediate effect on infant survival. Nevertheless, in this model, the coefficients show a very similar pattern to the original results. However, only the coefficient of proportion of co-ethnics in a district remains significant. When we include region birthyear survey fixed effects in addition to mother fixed effects (Model 4), the overall pattern becomes weaker and statistically insignificant but remains clearly discernible and in line with our expectations.

Table A14: Household and Mother Fixed Effects

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.262*	−1.377**	−0.755	−0.635
	(0.690)	(0.671)	(0.720)	(0.717)
Dist. Share Gov. Co-Ethnics (t-1)	−1.879***	−1.513	−1.706**	−0.476
	(0.725)	(1.000)	(0.835)	(1.201)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	2.106*	1.940*	1.186	0.348
	(1.098)	(1.008)	(1.199)	(1.114)
Household FE	yes	yes	–	–
Mother FE	no	no	yes	yes
Birthyear FE	yes	no	yes	no
Survey-Region-Birthyear FE	no	yes	no	yes
Controls	yes	yes	yes	yes
Observations	1,503,930	1,503,930	1,503,930	1,503,930
Adjusted R^2	0.094	0.109	0.103	0.118

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.78 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

A3 Heterogeneous Effects?

Table A15 reports results from the triple interaction models discussed in the robustness section of the main paper. The first two models test whether democratic institutions moderate the benefits of individual- or district-level co-ethnicity with the government. Model 1 uses a dummy coded as one for all country-years with above-median Polity IV values as moderating variables. In our sample, the median Polity IV value is −1. Model 2 uses a similar above-median dummy based on the Varieties of Democracy (VDEM) Polyarchy Index which is bounded between 0 and 1 and, in our sample, has a median of 0.349. Figures A10 and A11 plot predictions and differences derived from these first two triple interaction models. The upper two panels in both of these figures replicate Figure 3 from the main paper and show predictions for government co-ethnics and non-co-ethnics across the observed range of district-level co-ethnicity in less democratic (top-left panel) and more democratic (top-right panel) settings. The bottom two panels plot the estimated differences between these predictions in more and less democratic contexts for government co-ethnics (bottom-left panel) and non-co-ethnics (bottom-right panel).

According to Figure A10, the general pattern of effects remains similar across more

Table A15: Heterogeneity: Regime Type & Electoral System

	Infant Mortality		
	(1)	(2)	(3)
Government Co-Ethnic (t-1)	−1.660***	−1.784***	−2.108**
	(0.467)	(0.471)	(0.845)
Dist. Share Gov. Co-Ethnics (t-1)	−2.540***	−2.794***	−1.954
	(0.894)	(0.812)	(2.000)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	2.152***	1.942***	3.236**
	(0.618)	(0.644)	(1.329)
Co-Ethnic × High Polity IV (t-1)	0.519		
	(0.564)		
Dist. Share × High Polity IV (t-1)	1.396		
	(1.089)		
Co-Ethnic × Dist. Share × High Polity IV (t-1)	−0.866		
	(0.853)		
Co-Ethnic × High VDEM (t-1)		0.697	
		(0.534)	
Dist. Share × High VDEM (t-1)		1.771**	
		(0.869)	
Co-Ethnic × Dist. Share × High VDEM (t-1)		−0.514	
		(0.763)	
Co-Ethnic × Mostly FPTP (t-1)			1.020
			(0.815)
Dist. Share × Mostly FPTP (t-1)			−0.483
			(2.241)
Co-Ethnic × Dist. Share × Mostly FPTP (t-1)			−1.940
			(1.312)
Survey-Ethnic FE	yes	yes	yes
Survey-District FE	yes	yes	yes
Survey-Region-Birthyear FE	yes	yes	yes
Controls	yes	yes	yes
Observations	1,503,778	1,503,836	1,000,980
Adjusted R^2	0.059	0.059	0.057

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.78 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' age and age squared as well as infants' sex, a twin dummy, birth rank, and birth rank squared. Two-way clustered standard errors in parentheses (survey-ethnic group and survey-district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

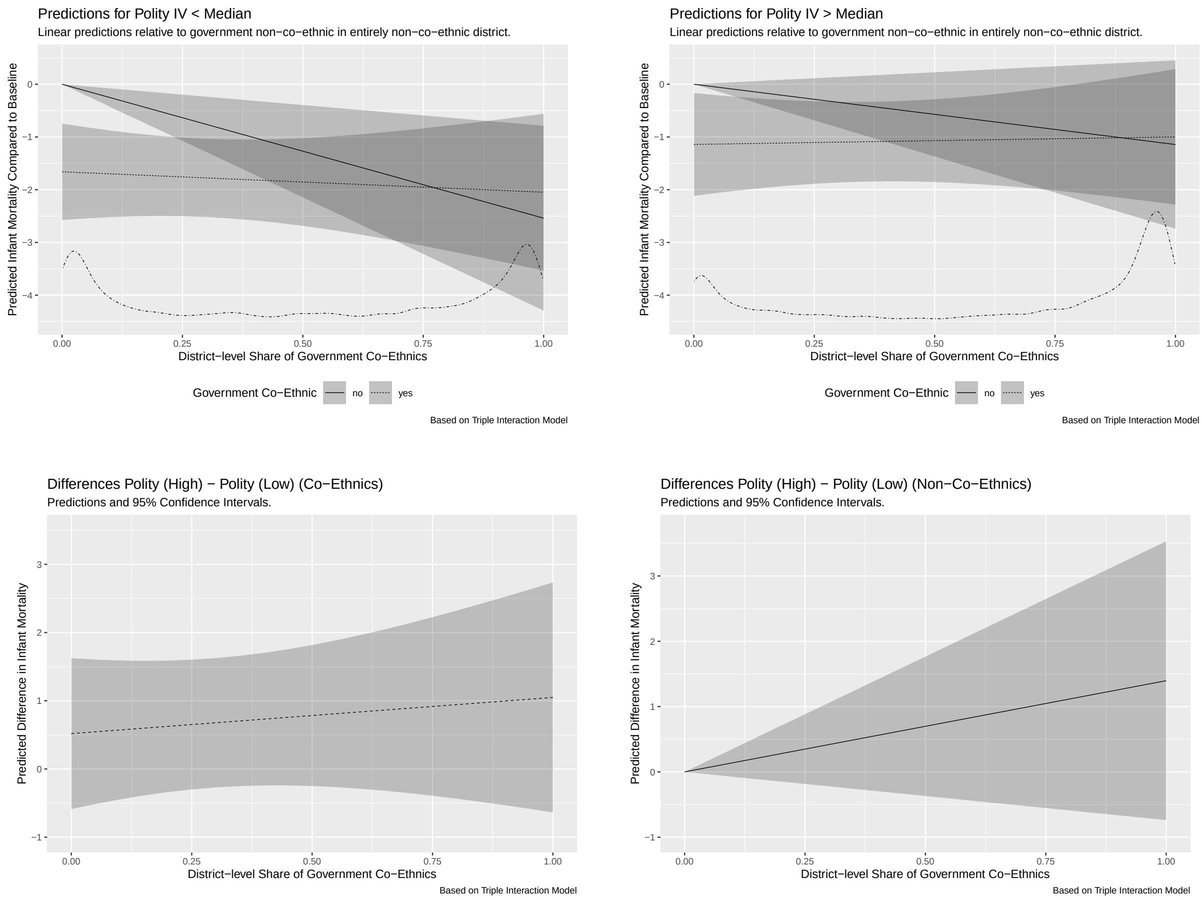


Figure A10: Predictions according to Polity IV Value (below vs. above median)

and less democratic contexts. However, the effect sizes appear larger in less democratic country-years (top-left panel) than in more democratic ones (top-right panel). As illustrated by the bottom two panels of A10, the differences in overall predictions between more and less democratic country-years for both government co-ethnics and non-co-ethnics never reach statistical significance.

A look at marginal effects provides additional insights into the uncertainty surrounding differences in our findings across regime types based on the Polity measure. In less democratic country-years, the marginal effect of individual co-ethnicity is significantly different in districts that are entirely co-ethnic and districts without any co-ethnics. In more democratic contexts, this is not the case. However, the moderating effect of the share of co-ethnics in a district is not significantly different across regime types.

Figure A11 reveals qualitatively very similar results for the VDEM-based interaction models. In contrast to the Polity models, however, the differences in our overall

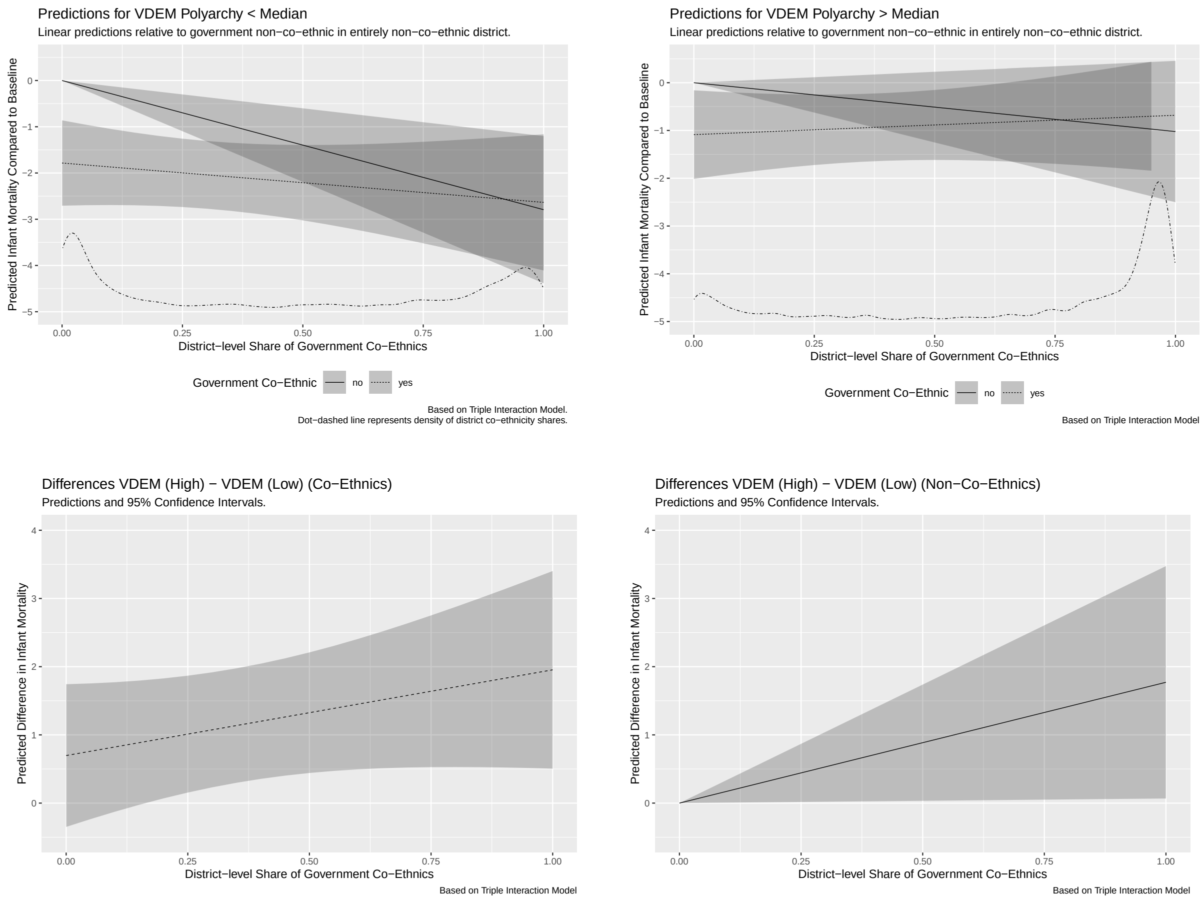


Figure A11: Predictions according to VDEM Polyarchy Value (below vs. above median)

predictions between more and less democratic contexts are somewhat more pronounced and more precisely estimated. The advantage non-co-ethnics enjoy due to their district's level co-ethnicity with the government is significantly lower in country-years with above-median VDEM scores (bottom-right panel). For co-ethnics, the differences in predicted outcomes between regime types are significant for districts with a co-ethnicity share between roughly 0.2 and 1 (bottom-left panel).

Looking at marginal effects, we find that individual co-ethnicity has a significantly different effect in districts with the highest and the lowest possible proportion of co-ethnics irrespective of regime type. The moderating effect of district-level proportion of co-ethnics is larger in non-democratic contexts, but not significantly so.

Model 3 in Table A15 tests whether the effects reported in our baseline models systematically vary across different electoral systems. We use the "HOUSESYS" variable from the Database of Political Institutions as moderator (Cruz, Keefer and Scartascini,

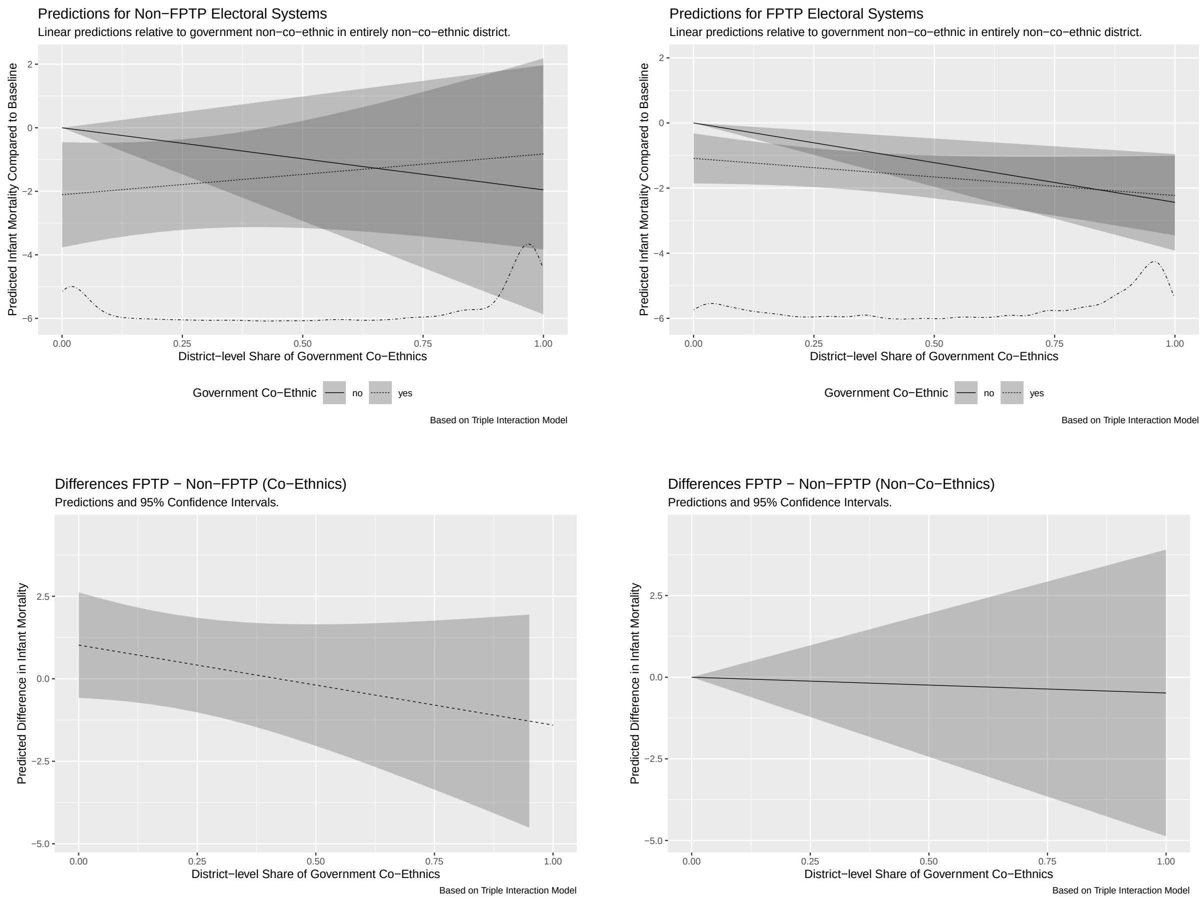


Figure A12: Predictions according to Electoral System (PR and mixed vs. FPTP systems)

2016). This variable is coded as one for all country-years in which a plurality/first-past-the-post/majoritarian rule governs the election of the majority of legislative seats and as zero otherwise. Figure A12 illustrates the results. While the effects for the relatively few PR and mixed systems in our sample are less precisely estimated (top-left panel), there are no statistically significant differences between predictions in FPTP and other electoral systems (bottom two panels). A look at marginal effects confirms this finding: The district share of co-ethnics significantly moderates the effect of individual co-ethnicity in both types of electoral systems and the moderating effect of district level co-ethnicity does not differ significantly between them.

A4 Afrobarometer

Data and empirical strategy

The Afrobarometer surveys (Afrobarometer, 2015), rounds 1–5,[36] cover a wide array of political topics. Among many other issues, respondents are asked about their economic well-being and perceptions of public service provision. We use the related questions to mitigate the shortcomings of the DHS infant mortality measure:

- **Economic hardship:** In all rounds of the Afrobarometer, respondents have been asked how often they had "gone without" food/water/health care/fuel/income over the year prior to the interview. Answers are ordinal and range from 0 (never) to 4 (always). Furthermore, we make use of a binary item indicating whether a respondent is currently employed or not. We combine all items into a principal component (see Table A16). The first component explains the bulk of the variance, and loads on all items except for the employment dummy. In our analyses, we use both the first principal component and the separate items.

- **Ease of accessing public services:** In rounds 2, 3, and 5 of the survey, respondents have been asked about how easy it is to access various public services. These services are: Getting an ID card, a place in primary school, household services such as piped water, medical services, and help from the police. The related question reads: "Based on your experience, how easy or difficult is to obtain the following services?" Answers range between 1 (very difficult) to 4 (very easy). We again conduct a principal component analysis (Table A17). All items heavily load on the first component, which again explains the bulk of the variance of the variables. To distinguish the general ease of public service access from that of particular services, we use the principal component as well as its constitutive parts in our analyses.

To make best use of the Afrobarometer data (Afrobarometer, 2015), we leverage the geocoding of Afrobarometer respondents provided by AidData (Ben Yishay et al., 2017) to

[36] We cannot use round 6 because it was collected after 2013, when the EPR data on ethnic inclusion ends.

Table A16: Principal component analysis: Economic hardship

				Factor loadings					
Component	Eigenvalue	Variance Explained	Variable	PC1	PC2	PC3	PC4	PC5	PC6
Component 1	2.57	0.43	How often gone without: Food	0.46	0.02	-0.27	0.18	0.83	0.01
Component 2	1.01	0.17	— Water	0.43	-0.11	0.34	-0.73	0.03	0.39
Component 3	0.7	0.12	— Health Care	0.49	-0.02	-0.05	-0.18	-0.24	-0.82
Component 4	0.66	0.11	— Fuel	0.41	-0.16	0.62	0.62	-0.15	0.12
Component 5	0.57	0.1	— Income	0.44	0.11	-0.61	0.16	-0.48	0.4
Component 6	0.49	0.08	Any employment	-0.08	-0.97	-0.21	0.01	-0.01	0

Table A17: Principal component analysis: Perceived service accessibility

				Factor loadings				
Component	Eigenvalue	Variance Explained	Variable	PC1	PC2	PC3	PC4	PC5
Component 1	2.24	0.45	Ease of accessing: ID card	0.46	-0.36	0.58	-0.41	-0.4
Component 2	1.03	0.21	— Primary school placement	0.38	0.66	0.25	-0.31	0.51
Component 3	0.67	0.13	— Household services	0.44	-0.53	0.02	0.41	0.59
Component 4	0.55	0.11	— Medical services	0.48	0.38	0	0.64	-0.46
Component 5	0.51	0.1	— Police services	0.47	-0.07	-0.78	-0.4	-0.12

link them with our district-level measure of ethnic inclusion. Using their home language, we also link respondents with the EPR data using the same procedure as applied to the DHS data. We thus match based on the names of ethnic groups. When no such link can be established between an Afrobarometer group and any EPR-group, we make use of information on the respective ethnic groups assembled by encyclopedias such as `ethnologue.com`, `wikipedia.com`, and `joshuaproject.org`. With the linked dataset, summarized in Table A18, we then proceed to estimating a linear relationship between individual- and district-level co-ethnicity with the government and our outcome measures as:

$$Y_{iedst} = \alpha_{es}\beta_1 \,\textsf{Co-Ethnic Government}_{et-1} + \beta_2 \,\textsf{District Share Co-Ethnic}_{dt-1} \\ + \beta_3 \,\textsf{Co-Ethnic Government}_{et-1} \times \textsf{District Share Co-Ethnic}_{dt-1} + \delta X_{iedst} + \epsilon_{iedst}$$

where respondent i is interviewed in year t, speaks language e which is associated with an EPR power status, and resides in district d which has a distinct share of co-ethnics to the government. As visible from the specification, all coefficients are affected by cross-sectional variation across ethnic groups and districts of the same country. This gives rise to potential omitted variable bias which we cannot strictly control using district- and group-fixed effects due to a lack of power and inter-temporal information available in the surveys.

Table A18: Afrobarometer: Summary statistics

Statistic	N	Mean	St. Dev.	Min	Max
Government Co-Ethnic (t-1)	83106	0.57	0.50	0	1
Dist. Share Gov. Co-Ethnics (t-1)	83018	0.69	0.35	0.00	1.00
Female	111936	0.50	0.50	0	1
Age	110273	35.59	13.92	17	130
Urban	111581	0.62	0.49	0	1
Education	100907	2.33	0.97	1	4
Economic hardship (principal component)	86603	−0.00	1.60	−2.42	4.62
How often gone without: Food	109446	1.09	1.21	0	4
— Water	109463	1.15	1.36	0	4
— Health Care	109239	1.26	1.29	0	4
— Fuel	100635	0.88	1.20	0	4
— Income	104561	2.12	1.34	0	4
Any employment	87960	0.42	0.49	0	1
Service access (principal component)	29716	−0.00	1.50	−3.39	4.00
Ease of accessing: ID card	62116	2.32	0.96	1	4
— Primary school placement	62356	2.74	0.92	1	4
— Household services	48714	2.11	0.94	1	4
— Medical services	58075	2.49	0.92	1	4
— Police services	50792	2.29	0.92	1	4

Robustness checks

Beyond the main results reported in Table 3 in the main paper, Tables A19 and A20 report the results from disaggregating the principal components. Both sets of results show very similar patterns as the main results. With regard to economic hardship, it is visible that co-ethnic districts and co-ethnics in non-co-ethnic districts are better off than non-co-ethnics living in non-co-ethnic districts. All effects are substantive in size and statistically significant. From Table A20 it emerges that respondents who live in co-ethnic districts report most ease to access public services (except for police services). No individual-level effect of co-ethnicity with the government is apparent, suggesting that these items capture *public* service provision.

Table A19: Economic hardship indicators: Cross-sectional OLS

	How often have you gone without (0–4):					Employment
	Food	Water	Medical treat.	Fuel	Income	
	(1)	(2)	(3)	(4)	(5)	(6)
Government Co-Ethnic (t-1)	−0.258**	−0.184**	−0.239***	−0.239**	−0.150**	0.079***
	(0.108)	(0.074)	(0.088)	(0.097)	(0.071)	(0.024)
Dist. Share Gov. Co-Ethnics (t-1)	−0.349***	−0.469***	−0.445***	−0.389***	−0.421***	0.126***
	(0.085)	(0.075)	(0.072)	(0.082)	(0.078)	(0.027)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	0.257**	0.324***	0.307***	0.310***	0.233**	−0.082***
	(0.128)	(0.101)	(0.106)	(0.115)	(0.096)	(0.029)
Individual-level covariates:	yes	yes	yes	yes	yes	yes
Country-survey fixed effects:	yes	yes	yes	yes	yes	yes
Observations	70,590	70,605	70,432	70,321	70,265	65,046
Adjusted R^2	0.100	0.075	0.123	0.058	0.171	0.189

Notes: OLS linear models. Control variables include 4 levels of education, age and age squared, as well as a female dummy. Two-way clustered standard errors in parentheses (language group and district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Table A20: Ease of accessing services: Cross-sectional OLS

	Ease to access public serivces (1–4):				
	ID card	Prim. school placement	Household services	Medical services	Police
	(1)	(2)	(3)	(4)	(5)
Government Co-Ethnic (t-1)	0.0002	0.012	0.042	0.056	−0.030
	(0.060)	(0.064)	(0.045)	(0.088)	(0.052)
Dist. Share Gov. Co-Ethnics (t-1)	0.113**	0.138**	0.238***	0.165***	0.055
	(0.057)	(0.058)	(0.056)	(0.063)	(0.056)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	0.017	−0.007	−0.093	−0.076	0.069
	(0.077)	(0.078)	(0.067)	(0.107)	(0.072)
Individual-level covariates:	yes	yes	yes	yes	yes
Country-survey fixed effects:	yes	yes	yes	yes	yes
Observations	47,278	47,995	40,725	38,950	43,347
Adjusted R^2	0.073	0.119	0.104	0.055	0.066

Notes: OLS linear models. Control variables include 4 levels of education, age and age squared, as well as a female dummy. Two-way clustered standard errors in parentheses (language group and district clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

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