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

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Abstract

This article investigates how local ethnic demography affects strategies of favoritism in Africa. Recent studies of ethnic favoritism report advantages of either presidents' ethnic peers or home regions. As regions in multi-ethnic states are rarely perfectly homogeneous, these studies cannot disentangle whether favoritism targets entire regions or co-ethnic individuals. We argue that governments' provision of goods depends on local ethnic demographies. Where government co-ethnics are in the majority, ethno-regional favoritism benefits all locals regardless of their ethnic identity. Outside of these strongholds, incumbents pursue discriminatory strategies and only their co-ethnics gain from favoritism. We use fine-grained geographic data on ethnic demography and infant mortality to test these hypotheses. Results from rigorous fixed effects specifications that exploit temporal changes in the ethnic composition of governments support our theoretical claims. Our findings have important implications for theories of distributional politics and conflict in multi-ethnic societies.

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Introduction

Where ethnicity is a salient political cleavage, governments often gear the provision of goods and services towards their ethnic peers. Biased goods provision tends to slow economic growth and leads to inter-group inequalities that spur distributional and sometimes violent ethnic conflict (Easterly and Levine, 1997; Alesina, Baqir and Easterly, 1999; Cederman, Weidmann and Gleditsch, 2011). A growing number of studies presents evidence for such ethnic favoritism (Burgess et al., 2015; Kramon and Posner, 2016; Dreher et al., 2019; Jablonski, 2014). However, studies usually operationalize government co-ethnicity either at individual or subnational levels and remain largely agnostic as to whether governments target co-ethnic individuals (e.g. Franck and Rainer, 2012) or geographic regions (e.g. De Luca et al., 2018; Hodler and Raschky, 2014).

This question is particularly relevant in Africa where most towns and villages are neither entirely homogenous nor fully diverse. Citizens' ethnic identity correlates with their place of residence, but not perfectly so. Thus, individual and regional ethnic favoritism become observationally equivalent if tested with conventional empirical designs. Distinguishing the two strategies requires information on the fate of 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, on the other hand, follows a strictly regional logic, both government co-ethnics and non-co-ethnics benefit in co-ethnic strongholds but not elsewhere.

In this paper, we develop and test a more nuanced theory of ethnic favoritism. We argue that local ethnic demography affects national governments' choice between broad regional and more individual-level provision strategies. In areas mainly populated by government non-co-ethnics, incumbent coalitions specifically target co-ethnic individuals. Because individual-level targeting is more expensive than providing local public goods, however, governments pursue less discriminatory strategies in co-ethnic majority regions. Here, governments provide locally non-excludable goods that benefit all residents, including non-co-ethnic minorities.

We use rich geographic data on local ethnic demographies and infant mortality to test the distributional consequences of our theory across 22 African countries. Infant mortality is a broad 'catch-all' proxy for favoritism as it is affected by goods provided to individuals (e.g. government jobs), 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 share of the population that is co-ethnic with the national government. In combination with data on ethnic ruling coalitions from the Ethnic Power Relations (EPR) dataset (Vogt et al., 2015), we test how the interplay of individual- and district-level co-ethnicity with the government affects patterns of ethnic favoritism.

More specifically, we estimate the effect of changing co-ethnicity with the government at the individual and district levels on infant mortality rates. We identify these effects by estimating models that only exploit temporal variation in government co-ethnicity within ethnic groups and districts. We find support for our hypotheses: Being born to a mother ethnically represented in the national government increases an infant's chance of survival by about 1.4 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 co-ethnics of the government similarly increases infant survival rates by 1.0–1.9 percentage points, irrespective of individual ethnic identities. In these districts, individual co-ethnicity with the government does not increase infant survival rates beyond the effect of the district-level share of co-ethnics. This is consistent with the provision of locally non-excludable goods that also benefit residents from powerless ethnic groups.

A number of robustness checks shows that our results are not due to reverse causality, systematic migration patterns, or segregation of ethnic groups within districts. Additional analyses show that neither regime type nor the type of electoral system moderate the findings on our mechanism. Correlational evidence from the Afrobarometer surveys 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 (1983, 152), "ethnic groups represent, in essence, coalitions which have been formed as part of rational efforts to secure benefits created by the forces of modernization – benefits which are desired but scarce." Ethnic markers such as individuals' language or phenotype enable an ethnically biased targeting of 'pork' (Fearon, 1999) and thus provide incentives for the formation and persistence of ethnic coalitions that benefit their members at the expense of other groups (Padró i Miquel, 2007; Wantchekon, 2003).

Empirical studies provide substantive evidence that governments indeed favor their co-ethnic individuals and home regions (Franck and Rainer, 2012; Burgess et al., 2015; Hodler and Raschky, 2014). A first group of studies investigates whether presidents provide benefits to individual citizens who share their ethnic identity. Franck and Rainer (2012) find that presidents' co-ethnics have better access to primary education and lower infant mortality rates than citizens from other ethnic groups. Taking a more sceptical stance, Kramon and Posner (2013) show that the effect of co-ethnicity with the president varies across countries and the specific goods that are studied.

A second body of work operationalizes ethnic favoritism as a regional or ethno-regional phenomenon. Burgess et al. (2015) report that Kenyan road investments during autocratic spells favor districts where the majority of the population shares the president's ethnicity. Hodler and Raschky (2014) as well as De Luca et al. (2018) use nighttime luminosity as a measure of local development and show that presidents' administrative and ethnic home regions shine brighter at night than other areas. Dreher et al. (2019) find that Chinese-funded aid projects tend to go to African presidents' home regions.[1]

[1] See also Jablonski (2014) on multilateral aid in Kenya.

Kramon and Posner (2016) use individual- and district-level co-ethnicity with the president as alternative operationalizations of ethnic favoritism and provide evidence that both increase educational attainment in Kenya.

While sufficient to prove the existence of favoritism per se, these two empirical approaches are unable to distinguish ethnic from geographic drivers of favoritism. Subnational regions in multi-ethnic countries are rarely perfectly homogeneous and many members of ethnic groups live outside their ethnic home region. The empirical inability to differentiate the forms of ethnic favoritism comes with theoretical imprecision. Do we expect governments to target individuals or regions? Or do they follow a more nuanced strategy aimed at minimizing costs in the face of regionally varying local ethnic demographies?

Recent work on African voting behavior and distributive politics has started to disentangle the interplay between individuals' identities and their local ethnic environment. In Ghana, Ichino and Nathan (2013) find that rural voters from local minority groups support non-co-ethnic majority candidates who promise locally non-excludable goods that benefit all residents. Similarly, Carlson (2015) and Nathan (2016) find that local minority voters tend to expect private goods from co-ethnic governments in Uganda and Ghana, respectively. Where one ethnic group dominates, however, residents expect public goods from politicians aligned with the local majority. Taken together, these results suggest that African voters believe their politicians to distribute goods with an eye on local demographics. However, neither study tests whether these expectations line up with the actual distribution of benefits among governments' co-ethnics and non-co-ethnics.

Two recent articles on local public goods provision in Malawi and Kenya provide first evidence that local demography matters for distributive politics. Ejdemyr, Kramon and Robinson (2017) argue that the provision of local club goods can only serve as a favoritism device if a politician's co-ethnics live in geographically segregated enclaves. In line with this notion, ethnically segregated Malawian districts receive more boreholes and within these districts, MPs target mainly co-ethnic localities.[2] Harris and Posner (2019) study

[2]Using the provision of private goods as a placebo test, they find no effect of local ethnic segregation.

the distribution of Kenyan local development projects within districts. Using similarly fine-tuned targeting strategies, MPs tend to favor their co-ethnics and spatially segregated political supporters.

Our paper joins these studies in highlighting the fundamental role of local ethnic demography. At the same time, we move beyond previous work in several important ways. First, our theoretical framework predicts governments to target not only geographically segregated co-ethnic strongholds but also co-ethnic minorities in largely 'opposition' districts. Our fine-grained data on ethnic demography allows us to test this expectation. Secondly, we test whether government non-co-ethnics indeed benefit from living in co-ethnic strongholds as assumed by Ichino and Nathan (2013) and Ejdemyr, Kramon and Robinson (2017). Finally, our analysis covers 22 countries and more than 50 years. This allows for more general conclusions than studies that focus on single countries, specific goods, or exploit cross-sectional variation only.

Theoretical argument

We argue that areas where the majority population shares the national government'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. In other words, goods provision is less discriminatory in co-ethnic strongholds than in government minority regions. As a result, government co-ethnics benefit from favoritism everywhere whereas non-co-ethnics only benefit if they reside in their government's ethnic strongholds. These predictions rest on two key assumptions. First, governments have intrinsic or instrumental reasons to favor co-ethnic citizens. Second, the per-beneficiary cost of government-provided goods is higher the more narrowly they are targeted to individuals. In addition, we treat multi-ethnic governing coalitions as unitary actors assuming the most senior group to continuously depend on its junior partners's support. As such, the president has no inherent advantage over other cabinet members when it comes to favoring her ethnic community.[3] To develop our argument, we first discuss why governments favor co-ethnic citizens at all before we proceed to the question of how they tailor distributive efforts to local ethnic demographies.

Why do co-ethnics benefit?

Incumbents' motivations. Our theoretical reasoning draws on two alternative logics of favoritism highlighted in the literatures on ethnic and distributive politics. First, incumbent governments may derive intrinsic utility from seeing their own rather than other ethnic groups thrive (Franck and Rainer, 2012). Under such conditions, ethnic favoritism implies serving the highest number of co-ethnics at lowest possible cost. Second, governments want to stay in power and instrumentally target benefits to important constituencies in order to ensure political survival.[4] As long as in-group support is cheaper to buy than out-group backing, governments will thus engage in ethnic favoritism. In reality, of course, both intrinsic biases and support-buying considerations may simultaneously motivate governments to favor their co-ethnics.

Buying support from co-ethnics. There are several reasons to believe that strategic governments prefer to buy support from their ethnic peers. Citizens who derive some 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 lower transaction costs and facilitate information flows and in-group policing (Fearon and Laitin, 1996; Larson and Lewis, 2017; Robinson, 2017). Better information about co-ethnics' preferences and more reliable local intermediaries enable

[3]Previous research on Kenya has found mixed evidence on cabinet portfolio-specific ethnic favoritism (Burgess et al., 2015; Kramon and Posner, 2016).

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

governments to more efficiently allocate benefits (Dixit and Londregan, 1996; Kasara, 2007; Baldwin, 2016). Lastly, co-ethnic promises of future favoritism and political support are often perceived as more credible (Carlson, 2015).

From the government's perspective, 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 backs our assumption that governments can more cost-effectively buy political backing from their ethnic peers than from other groups.

Winner-takes-all contests at the national level. In the context of multi-ethnic developing countries, political survival does not exclusively depend on an electoral formula that requires to win the support of a clearly defined minimum threshold of citizens or districts. Instead, governments need enough support to ward off both electoral and non-electoral challenges such as revolutions, rebellions, and coups. At the national level, both electoral and non-electoral contests for control of the central government follow a winner-takes-all logic. Political survival thus requires the support of a sufficient number of citizens regardless of where they live.[5] This logic differs markedly from majoritarian legislative elections where winning a certain number of districts decides the election and politicians face strong incentives to target a handful of contested swing districts (Lindbeck and Weibull, 1987; Casey, 2015).[6]

Taken together, these arguments suggest that both intrinsically biased and support-buying central governments face strong incentives to favor their co-ethnics. In what follows, we discuss how governments adjust their goods provision strategy to local-level ethnic demographies in order to optimally serve co-ethnic constituents.

[5] Non-constitutional power struggles evidently differ in many respects from presidential elections in consolidated democracies. Nonetheless, national incumbents have similar incentives as long as they can reduce the risk of non-electoral removal by ensuring sufficient popular support.

[6] As our argument focuses on the executive government, we do not consider different legislative electoral systems in our theory. Nevertheless, we empirically test whether the electoral formula moderates our findings (see A14).

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 benefits 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, or ultimately money (Albertus, 2013; Alesina, Baqir and Easterly, 1999; Besley et al., 2004). In between these two extremes, we situate local public goods that are targeted at ever smaller localities, for example a road that connects a smaller part of a village with its center or a local source of water. Despite this extensive gray zone, goods can be distinguished by the degree to which their provision can exclude (groups of) citizens that are not intended to benefit.

Our crucial assumption is that the per-beneficiary cost of providing goods increases in their excludability. Locally non-excludable goods and infrastructures often require high initial investments but the variable usage costs typically remain low. Once set up, a high number of local beneficiaries profits over extended periods. As a result, the provision cost per user is lower than in the case of 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 these constituencies' specific needs (Armesto, 2010; Ichino and Nathan, 2013). At the same time, governments may transform local public goods into private ones by restricting access to co-ethnic individuals, which is costly. 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 is higher the more of the local recipients the government wants to reach (Ejdemyr, Kramon and Robinson, 2017).

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 – either for intrinsic reasons or because co-ethnic support is cheaper to secure. Second, they 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 extent to which there is a trade-off between these two logics.

Homogenous co-ethnic areas constitute the easiest case. In these areas, governments want to reach almost all residents and achieve this goal by providing locally non-excludable goods. The dominant majority population in these government strongholds and the small local minority of non-co-ethnics benefit in equal measure. Specifically targeting co-ethnics in predominantly non-co-ethnic 'opposition' districts requires the less cost-effective provision of narrower club and private goods. Governments deriving high intrinsic utility from serving 'their own' willingly incur these costs. Doing so represents the only way to reach co-ethnic communities outside of core ethnic homelands without at the same time serving non-co-ethnic majority populations.

More strategical incumbents only target co-ethnics scattered across mainly non-co-ethnic areas if their political support is needed. Where public goods provision to mainly co-ethnic regions ensures political survival, co-ethnics outside of these areas may end up getting nothing and favoritism indeed 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 target them with private goods to save costs. Ruling coalitions with a relatively small ethnic support base, on the other hand, may need the support of at least some non-co-ethnics. To minimize costs, such governments target non-co-ethnics with 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 still respond more readily to favoritism, however, targeted strategies remain rational in districts with sufficiently low proportions of government co-ethnics.

Taken together, our theoretical arguments suggest that support-buying and intrinsically biased governments allocate non-excludable goods to districts with a high share of government co-ethnics. In areas demographically dominated by politically non-represented ethnic groups, governments target their co-ethnics with localized 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 individual's ethnic identity has no effect on the likelihood of receiving goods from the government (*regional ethnic favoritism*).

Data

We test our theoretical argument with a large, individual-level dataset on infant mortality covering 22 African countries and 54 years (1960-2013). Understanding distributional politics in multi-ethnic African states is crucial given the high prevalence of ethnic favoritism and its divisive consequences on the continent.[7]

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 (see Figures 1 and 2).[8] The surveys include each mother's ethnic identity, her current location, and all infants' birth years. This information and the large sample size allow us to not only exploit cross-sectional variation of mortality rates within countries, but also temporal variation within districts and ethnic groups. We are not aware of any other individual-level socio-economic data that would allow for similarly rigorous empirical tests.[9]

[7]We do not claim that ethnic favoritism is unique to Africa (see e.g. De Luca et al., 2018).

[8]We drop all infants born less than 12 months before the mother's DHS interview.

[9]In addition, the data comes with more statistical power than DHS data on education rates.

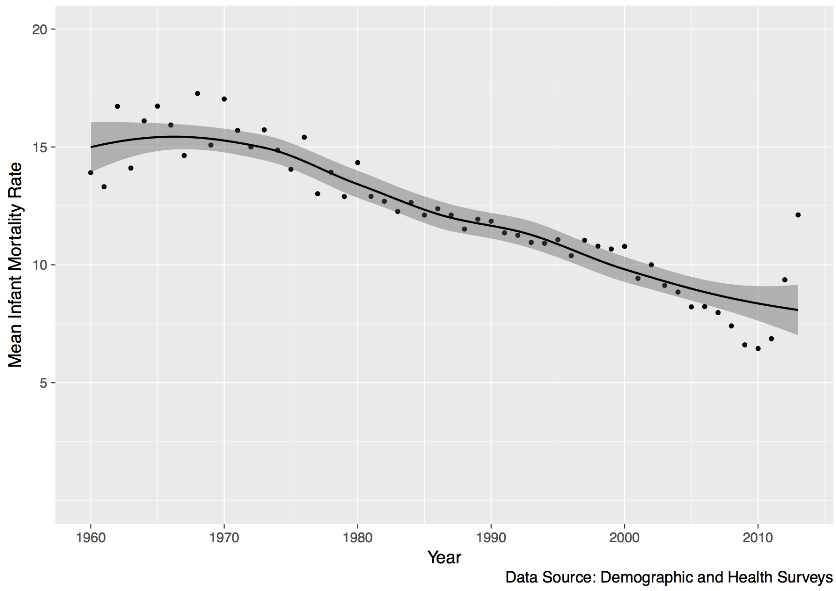


Figure 1: Annual infant mortality per 100 births across the 22 African countries in our sample (mean, LOESS curve and 95% confidence interval)

Our argument assumes that governments provide either local public goods with varying geographic reach or narrowly targeted private handouts. Infant mortality has multiple causes at the family, community, and national levels and is thus plausibly affected by different types of goods. Previous research suggests that infant mortality responds to family income (Charmarbagwala et al., 2004), nutrition (Pelletier et al., 1995), or direct cash transfers (Barham, 2011; Rasella et al., 2013), all of which governments can manipulate with targeted transfers. Other studies emphasize the importance of community-level factors and local public goods such as infrastructure (Fay et al., 2005), especially with regards to public health care (Gruber, Hendren and Townsend, 2014) and sanitation systems (Galiani, Gertler and Schargrodsky, 2005).

While data on infant mortality capture various types of government favors, they do not allow us to directly distinguish excludable private from non-excludable public goods. We address this question in two additional tests. First, we restrict comparisons between co-ethnic and non-co-ethnics to individual survey locations the size of villages or urban neighborhoods in order to test whether governments are able to narrowly target their co-ethnics with favors. Second, we use cross-sectional data from the Afrobarometer surveys on citizens' access to local public services.

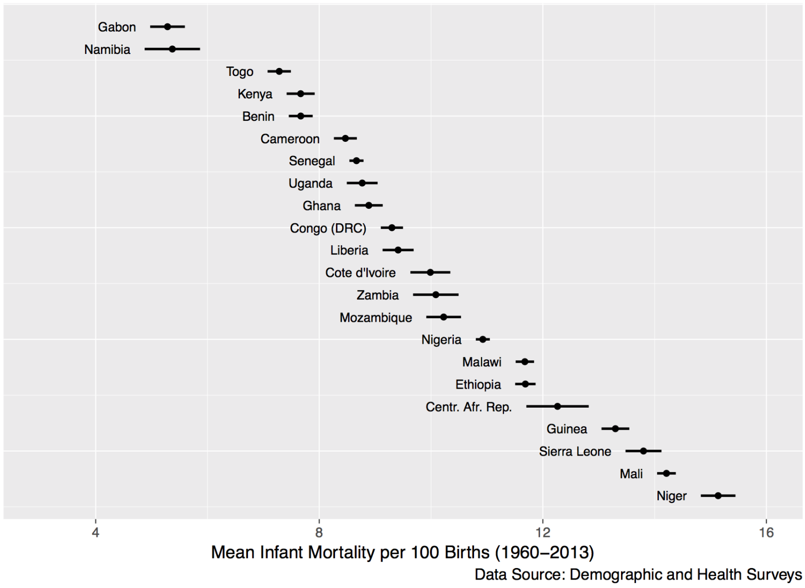


Figure 2: Mean infant mortality in the 22 countries in our sample, 1960–2013 (means and 95% confidence intervals)

To test for individual-level favoritism, we measure infants' co-ethnicity with the government by linking their mothers' ethnic identities to the ruling ethnic group(s) in the respective country and birth year. Information on the ethnic make-up of governments comes from the Ethnic Power Relations dataset (Cederman, Wimmer and Min, 2010; Vogt et al., 2015).[10] EPR lists all politically relevant ethnic groups represented in national executives,[11] thus capturing multi-ethnic coalitions, which are frequent in Africa (Arriola, 2013). If government elites from other than the president's group also engage in favoritism, the coalition-based approach yields more precise measures of government co-ethnicity.[12]

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 country-specific DHS survey, SIDE predicts cell-level ethnic

[10]The matching is based on ethnic group names and additional information assembled by encyclopedias such as `ethnologue.com`, `wikipedia.com`, and `joshuaproject.org`.

[11]According to EPR, ethnic groups (or clusters thereof) are politically relevant if they are politically mobilized at the national level, or if they are discriminated against by the government (Cederman, Wimmer and Min, 2010).

[12]But see robustness check A11 where we use the EPR-coding of "senior partnership", which is in most cases equivalent to the ethnic group of the president.

compositions on a raster with about 1 × 1 km resolution.[13]

Linking the SIDE data to EPR and aggregating it at 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,[14] and each cell has a population of pop_{ct}.[15] 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. As Figure 3 shows, our measure exhibits high levels of spatial variation within countries. Similarly, changes in the ethnic composition of governments produce large variation within districts over time (Figure 4).

Finally, we assign $\mathsf{District\ Share\ Co\text{-}Ethnic}_{dts}$ to infants via mothers' geocoded enumeration area, survey round s, and each recorded 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 (via its mother's identity) to the district-level data.[16]

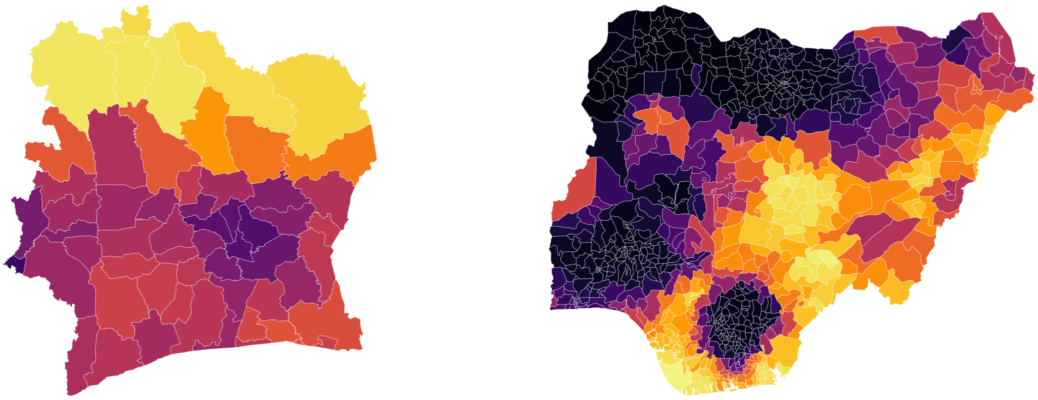
Our approach does not capture unobserved migration patterns. We thus take each mother's place of residence at the time of the survey as all her children's birthplace. In parallel, we assign each district a constant ethnic demography. To validly identify effects of ethno-regional favoritism, we therefore assume that, after the inclusion of fixed effects, unobserved migration is orthogonal to spatio-temporal dynamics of ethnic inclusion and

[13] Müller-Crepon and Hunziker (2018) demonstrate that their interpolation is quite precise and superior to the raw DHS data in predicting ground-truth census data. Since no non-ethnic covariates affect the predictions, interpolation errors are random noise with an expected mean of 0.

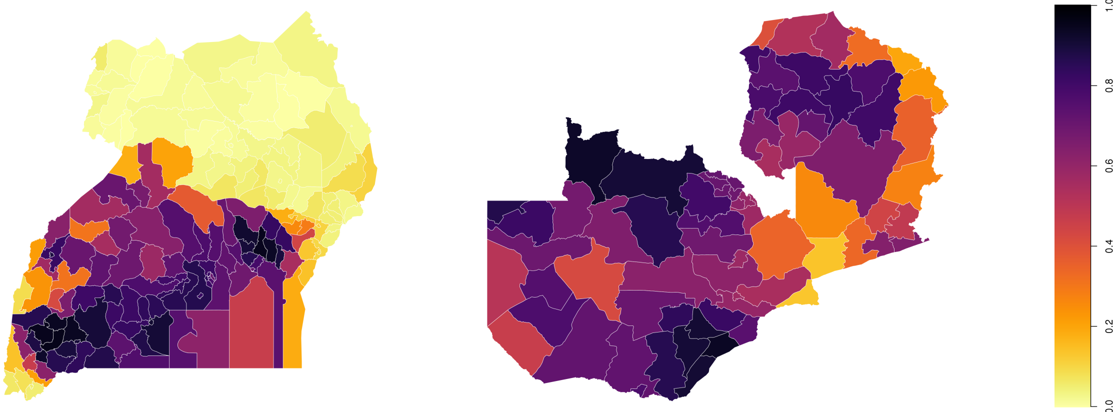
[14] District borders are from 2000 throughout. Data comes from FAO (2014).

[15] 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 the population distribution within a district negligibly affect the ethnic composition we compute.

[16] See Table A2 in the Online Appendix for a reliability check that suggests that both measures coincide.

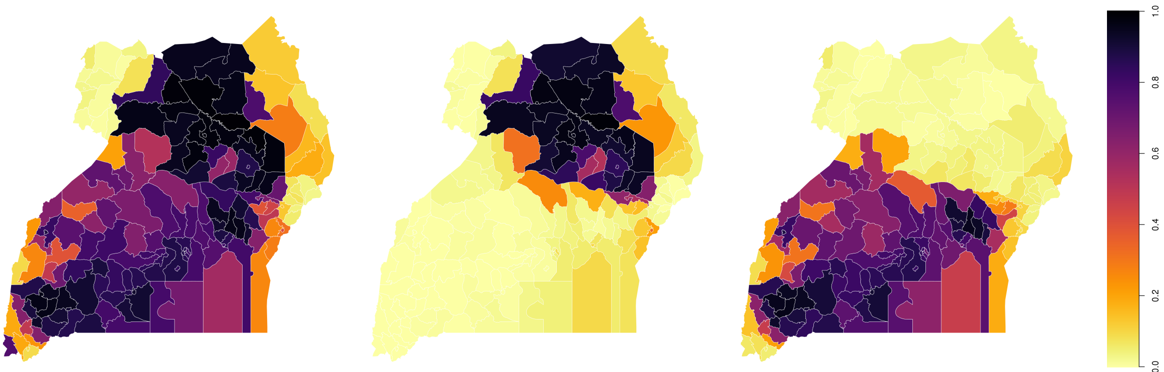


(a) Côte d'Ivoire (b) Nigeria



(c) Uganda (d) Zambia

Figure 3: District-level co-ethnicity with the government in 2000 using the most recent available SIDE data



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

Figure 4: District-level co-ethnicity with the government over time; SIDE data from 2011

changes in infants's mortality. We further probe this assumption in our empirical robustness checks.

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.[17]

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. We temporally lag these predictors by one year to allow some time for the hypothesized effects to unfold.[18] Our hypothesis stipulates that β_1, the coefficient of our variable on individual-level co-ethnicity, is negative because governments favor their co-ethnics everywhere. Co-ethnics in government strongholds receive local

[17]In the absence of well-founded assumptions about the "true" functional form, there is not much reason to expect the estimates from the LPM to be any better or worse than results from a logit or probit specification (Angrist and Pischke, 2008; Beck, 2015).

[18]Our results are robust to using alternative time lags (Table A7).

public goods whereas co-ethnic minorities in largely opposition districts are targeted in more discriminatory fashion. Following this logic, β_2, the coefficient of the variable on the district-level share of co-ethnics, should be negative as well. This is because the likelihood of public service provision rises in the proportion of co-ethnics in a district. Such public goods do in turn also benefit non-co-ethnic residents. Finally, because co-ethnics in mainly co-ethnic districts receive the same public goods as non-co-ethnics and no additional private handouts, we predict that the interaction effect β_3 is positive. This would reflect our expectation that government co-ethnics are only better off than non-coethnics when they live in districts where governing groups are in the minority.

In order to credibly identify the effects of our three variables of interest, the model only exploits temporal variation that originates from changes in the ethnic compositions of governments.[19] 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 the likelihood of being represented in national governments. We use separate ethnicity fixed effects for each DHS country-survey round to make sure that mothers' ethnic identities line up with the SIDE district shares derived from the very same survey. Second, λ_{ds} is a vector of district-survey-round fixed effects that controls for unobserved, time-invariant differences between districts such as soil fertility or 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 subnational regions, i.e. the first-level administrative unit below the central government. Such subnational shocks, for example

[19] Tables A3 to A6 in the Online Appendix provide a list of all such changes in our sample.

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). Note that the 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 ath 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.[20] All observations are weighted to make sure that the 22 countries in our sample contribute equally to the estimated effects.[21]

Results

In this section we present our main findings. 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.

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 1.4 percentage points lower mortality risk than non-co-ethnic infants in these districts. This amounts to 13.5% of the mean mortality rate in our sample (10.6%). The coefficient on district-level government co-ethnicity is also significantly negative. In-

[20] Our results are robust to clustering standard errors at coarser resolutions (see Table A8).

[21] We assign the inverse of the number of observations from country c as weight to each observation from that country. This avoids undue influence of countries that are (a) more frequently surveyed by the DHS or (b) have particularly high fertility rates. The former applies to more developed, and the latter to less developed countries. Our results remain similar if we do not weight observations (Table A9).

Table 1: Main Specifications

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.434***		−1.618***	
	(0.390)		(0.462)	
Dist. Share Gov. Co-Ethnics (t-1)	−1.944***	−1.836**		
	(0.637)	(0.880)		
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.855***	1.882***	1.802***	1.652***
	(0.496)	(0.550)	(0.529)	(0.575)
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	1,485,226	1,485,226	1,485,226	1,485,226
Adjusted R^2	0.060	0.062	0.069	0.070

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.62 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

creasing 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 1.0 percentage points. Substantively, these effects are comparable in size to the survival advantage of female babies in our sample.[22]

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 5 visualizes how the interplay of individually and regionally targeted ethnic favoritism affects infants' death before age one. 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.[23] The x-axis of the figure 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 significantly lower mortality than their non-co-ethnic counterparts (solid line). As the share of government co-ethnics in a district rises, co-ethnic infants' estimated survival advantage over babies in the baseline

[22]In Model 1 of Table 1, the female dummy enters with a highly significant coefficient of –1.43.

[23]Visualizing these predictions allows us to present our results on the notion of a substitution effect between individual and district-level co-ethnicity in a straightforward way. In the Online Appendix, we also show conventional marginal effect plots of individual co-ethnicity across the range of district-level government co-ethnicity (see Figure A3). The predictions for co-ethnics are calculated as $\beta_1 + a_d\,\beta_2 + a_d\,\beta_3$; the ones for non-co-ethnics as $a_d\,\beta_2$ where a_d is the district share of government co-ethnics in hypothetical district d.

category (non-co-ethnic infants in non-co-ethnic districts) remains similar, ranging from -1.43 to -1.52 percentage points. On the other hand, mortality estimates for non-co-ethnic infants show a markedly different pattern. With increasing shares of government co-ethnics in a district mortality rates of non-co-ethnic infants decrease. At very high district shares of government co-ethnics, non-co-ethnic infants' predicted mortality rate is two 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 no longer have an advantage over non-co-ethnic children.[24]

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 a more discriminatory provision of excludable goods and handouts in government minority districts.

Models 2–4 in Table 1 add increasingly strict temporal fixed effects that account for unobserved temporal shocks to districts and ethnic groups, which may affect the ethnic composition of governments and bias our estimates. Model 2 includes survey-ethnic-group-year-of-birth fixed effects. These fixed effects control for yearly events at the level of groups such as changes in economic development or the outbreak of violence that might cause ethnic groups to rise to or fall from national power. These fixed effects fully absorb variation in individual-level government co-ethnicity and the respective constitutive term thus 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 year. 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 increasingly conservative

[24] Hainmueller, Mummolo and Xu (2019) highlight potential problems with conventional multiplicative interaction models. In the Online Appendix, we show results from their proposed binning estimator demonstrating that our results are no artifact of linearity assumptions or lack of common support. See Figures A4 and A5.

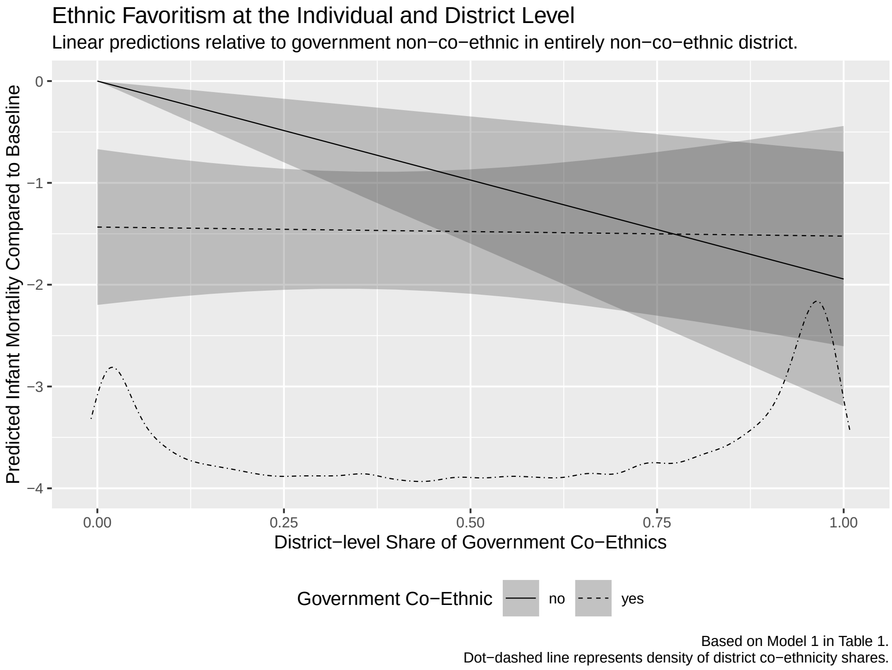


Figure 5: Predictions for government co-ethnics and non-co-ethnics conditional on local ethnic demography

specifications, the identified coefficients of interest remain stable and statistically significant. These results suggest that the estimates in Model 1 are not due to time-variant omitted variables at the level of ethnic groups or districts.

Alternative explanations and sensitivity analyses

Interpreting the reported results as causal requires some crucial assumptions. We check the robustness of our results against three main threats to inference. We provide evidence that the baseline results are not due to (1) within-district ethnic segregation, (2) non-parallel trends between ethnic groups, (3) differential migration rates in our sample. Lastly, our heterogeneity analysis fails to detect moderating effects of regime type or electoral systems on our mechanism.

Ethnic segregation within districts. The effect of individual-level government co-ethnicity in the main analyses may not result from private goods provision but from ethnic segregation within districts and public goods provision on the sub-district level (Ejdemyr, Kramon and Robinson, 2017). Homogeneous co-ethnic villages may be the primary beneficiaries of ethnic favoritism in mainly non-co-ethnic districts. To rule out this threat to our interpretation of the results, we include fixed effects for each DHS survey location-birth year combination. The resulting estimates only exploit differences between government co-ethnics and non-co-ethnics born in the same year within the same village or urban neighborhood. The point estimates in Model 1 of Table 2 are somewhat smaller and less precisely estimated, which is likely due to a dramatic loss of statistical power. Nonetheless, government co-ethnics born in survey clusters in non-co-ethnic districts have, on average, better survival chances than neighboring non-co-ethnic infants born in the same year and location. These highly localized differences make it unlikely that the effect of individual-level co-ethnicity is due to local public goods provision to ethnically segregated areas within diverse districts. The results suggest instead that governments target favors at individual households.

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 in ethnic group-specific infant mortality rates and their causes. If governments systematically include political elites from groups that are economically on the rise and exclude groups with declining fortunes, reverse causation biases our findings. To assess whether such a scenario is plausible, we run additional models that include dummies or linear trends for the period briefly before an ethnic group gets upgraded to or excluded from the national executive. Model 2 in Table 2 presents results from a model that looks at the three years prior to such up- and downgrades. The estimates do not show any evidence of reduced infant mortality in the three-year period prior to a group's rise to power. Neither is there any indication of groups with temporarily high mortality rates losing their political power. 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 Online Appendix Table A13. All but one of these terms remain small and insignificant or point in the direction that supports our argument. These findings reduce our concerns about simultaneity bias.

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

	Infant Mortality			
	Cluster FE	Time Trend	Pre-Trends	Less Educated
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.437**	−1.249***	−1.579***	−0.940**
	(0.691)	(0.475)	(0.409)	(0.434)
Dist. Share Government Co-Ethnics (t-1)		−2.001***	−2.448***	−1.780**
		(0.768)	(0.710)	(0.703)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	0.962	1.912***	2.126***	1.215**
	(0.793)	(0.526)	(0.540)	(0.593)
$\text{Upgrade}_{t+1 \text{ to } t+3}$			−0.189	
			(0.337)	
$\text{Downgrade}_{t+1 \text{ to } t+3}$			0.402	
			(0.468)	
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	1,485,226	1,485,226	1,435,906	1,299,489
Adjusted R^2	0.103	0.061	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 10.62 in columns 1–3 and 11.20 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

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 all her children. We therefore measure the birthplace of infants born to migrant mothers with error. Similarly, the ethnic group shares from SIDE are constant for all years in a district and survey round. All temporal variation in the district share of government co-ethnics thus comes from changes in the ethnic composition of governments. If politically excluded groups have different fertility rates or migration patterns than included groups, our measure of district-level co-ethnicity will be slightly off. Given the strict fixed effects in Table 1, these patterns only bias our results if they systematically co-vary with government changes in a way not captured by ethnic group- and district-year fixed effects.

However, relatively well-off non-co-ethnics from otherwise poor areas may systematically migrate to areas that are strongholds of a *new* government in order to access public goods and other economic opportunities. Similarly, if the *incoming* government coalition favors co-ethnics in public sector employment, newly appointed and comparatively wealthy state bureaucrats may be posted to non-co-ethnic districts. To threaten 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 at all relevant, such migration patterns most likely apply to relatively educated strata of the population. We therefore re-estimate our baseline model on a sample that excludes all mothers with more than primary education (about 13% of our sample). The results are reported in the last column of Table 2 and remain similar to previous specifications. The coefficient sizes are somewhat smaller than in our baseline specification yet remain significant in substantive and statistical terms.[25]

[25]The lower estimates may also be driven by a potentially lower likelihood of less educated respondents to report infant deaths.

Heterogeneous effects. Finally, we investigate whether democratic institutions moderate our findings as they constrain executive governments.[26] The analysis presented in the Online Appendix A3 finds mixed evidence suggesting that democratic institutions may reduce the severity of favoritism, but does not find that democratic institutions eliminate ethnic favoritism or moderate the findings on our mechanism. Furthermore, an exploration of the impact of varying electoral systems on ethnic favoritism suggests that there is no statistical difference between PR and first-past-the-post electoral systems. These findings are consistent with our theoretical argument 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 non-discriminatory public goods in predominantly co-ethnic districts and target co-ethnics in mainly non-co-ethnic districts with private goods. While the main analysis tests the distributional consequences of this logic, our data on infant mortality is unable to distinguish between public and private goods provision. To explore what types of goods governments provide in different localities, we turn to Afrobarometer (2015) survey data. These data contain information on governments' provision of public services, thus allowing to test our conjecture that governments provide non-excludable goods to districts with large shares of co-ethnics. In addition, we use information on households' economic well-being to test whether it coincides with our findings on infant mortality.

We link the geocoded Afrobarometer data from 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 Online 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. Below, we report effects on princi-

[26]Previous studies provide mixed findings on any moderating effect of democracy on ethnic or regional favoritism (Burgess et al., 2015; Hodler and Raschky, 2014; Franck and Rainer, 2012; Kramon and Posner, 2016).

pal components that summarize information across several items in both categories, and those items that directly relate to health care which we expect to most directly affect infant mortality. The Online Appendix reports disaggregated analyses and all details on the components. Because the Afrobarometer data lacks sufficient temporal depth to estimate difference-in-differences models, we exploit cross-sectional information within survey rounds in the same country. Omitted variables at the ethnic groups and/or district-level might therefore bias the following results.

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, primary schooling, and household services are more accessible for respondents in co-ethnic districts. However, we find no individual-level association between government co-ethnicity and public service access. This pattern also holds with regard to accessing medical (Model 2), and most other public services (Table A19). In line with our argument, these results suggest that largely co-ethnic districts receive more public goods and services.

Turning to indicators of 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 the infant mortality 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 have gone without medicines or medical treatment during the past year (Model 4) and all other survey items related to economic hardship (Table A18). These findings suggest that our main results on infant mortality are indeed driven by changes in households' economic status. Combined with the results from Models 1 and 2, they also show 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, likely as a result of *private goods* provided by their government.

Conclusion

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

We take these predictions to a large, geocoded dataset on infant mortality and district-level ethnic demography in Sub-Saharan Africa. These data allow us to disentangle the effects of individual and district-level government co-ethnicity. Our results are consistent with fine-tuned favoritism strategies that provide non-excludable public goods to ethnic government strongholds but ethnically targeted local or individual-level handouts to ethnic 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 demography affects strategies of favoritism has broader implications for the study of distributional politics in multi-ethnic societies. First, we highlight the importance of the ethnic composition of local populations 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, we are not yet able to optimally operationalize these different types in empirical terms. Better data on governments' public and private goods provision would allow for more explicit studies of redistributive strategies. Third and in line with Ichino and Nathan (2013), voters may adjust their voting behavior in anticipation of governments' goods provision strategies, thus at times voting for non-co-ethnic candidates. Non-segregated ethnic geographies or other incentives for the provision of regionally non-excludable public goods may thus serve to reduce the political salience of ethnic identities and identity-based distributive conflicts.

The dire effects of ethnic favoritism on children's lives suggest that sustainable and 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 have access to distributive goods provided by governments that can have large and direct implications for their lives and livelihoods. This information is crucial in order to target aid at the most vulnerable and alleviate local 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	Max
Infant Death	1,537,633	10.617	30.806	0	100
Government Co-Ethnic (t-1)	1,522,398	0.574	0.494	0	1
District Share Government Co-Ethnics (t-1)	1,535,859	0.578	0.397	0.000	1.000
Senior Government Co-Ethnic (t-1)	1,529,349	0.296	0.456	0	1
District Share Senior Government Co-Ethnics (t-1)	1,535,859	0.296	0.357	0.000	0.993
Upgrade to Political Inclusion	1,530,198	0.009	0.095	0	1
Downgrade to Political Exclusion	1,530,198	0.004	0.059	0	1
Education	1,537,633	1.541	0.750	1	4
Age	1,501,139	24.460	6.521	10	49
Birthorder	1,537,633	3.404	2.304	1	18
Female	1,537,633	0.489	0.500	0	1
Twin or Higher Multiple Birth	1,537,633	0.034	0.181	0	1
Polity IV > Median	1,536,585	0.443	0.497	0	1
VDEM Polyarchy > Median	1,537,531	0.487	0.500	0	1
FPTP Electoral System	1,030,832	0.678	0.467	0	1

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 due to limited amounts of measurement error or slightly different fertility rates[27] 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	1.024***	1.067***	1.049***	1.025***
	(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	1,604,560	1,604,560	1,604,560	1,604,560
Adjusted R^2	0.666	0.668	0.673	0.688

Notes: OLS linear probability models. The sample mean of the dependent variable is 0.586. 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 A1 maps the district-level co-ethnicity variable in 2000 for all 22 countries in our sample. Figure A2 depicts the spatial distribution of all 19'622 geocoded DHS survey clusters that we use in our analyses to match infants to districts.

Tables A3, A4, A5, and A6 list all changes in the ethnic composition of government coalitions happening between 1960 and 2013 in any of the 22 countries for which we have geocoded DHS data. The data comes from the Ethnic Power Relations database (Vogt et al., 2015). The last column in these tables contains the Polity IV democracy score for the respective country-year (Marshall, Jaggers and Gurr, 2016) and shows that variation in ethnic government coalitions is quite frequent in both autocratic and democratic contexts. 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 listed in Tables A3, A4, A5, and A6 below.

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

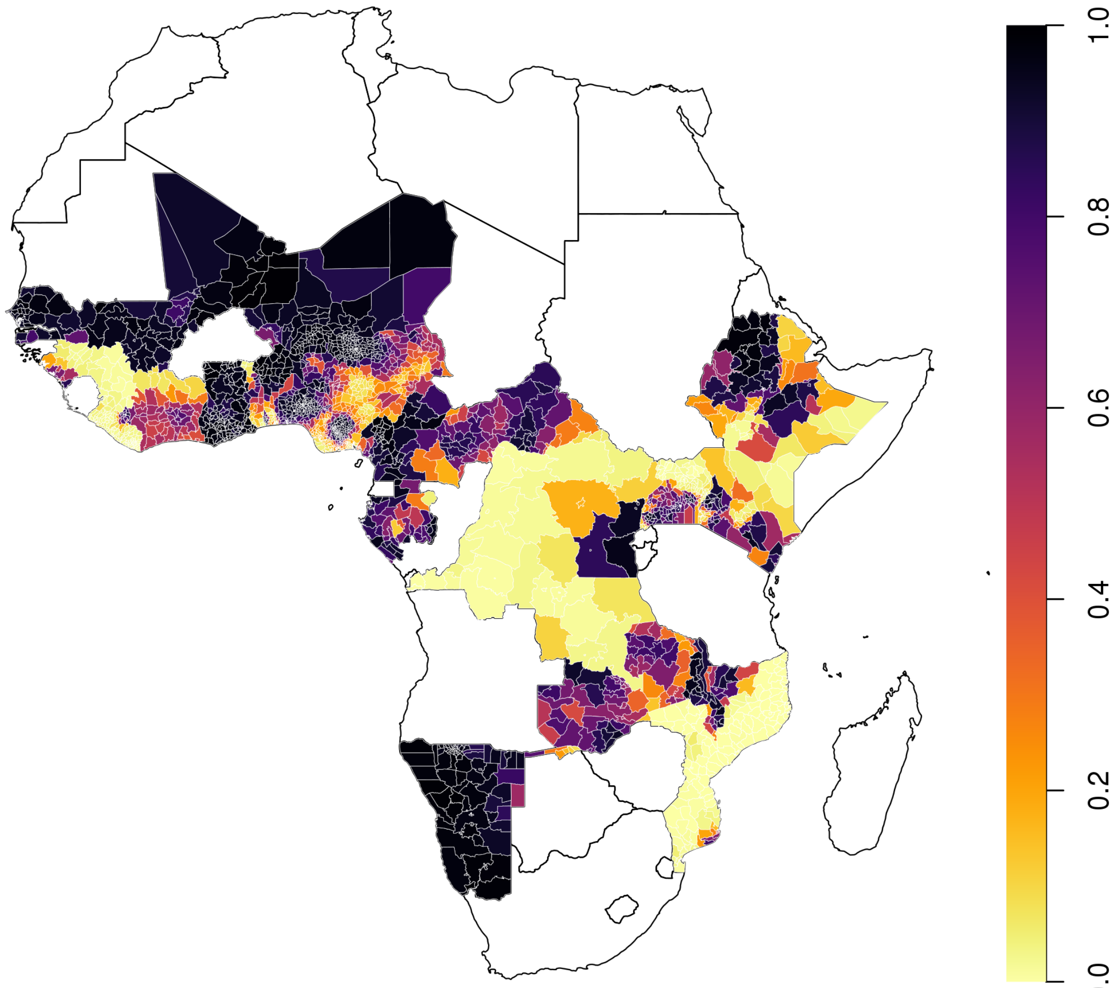

Figure A1: District-level co-ethnicity with the government in 2000; for each country, the most recent SIDE data is used

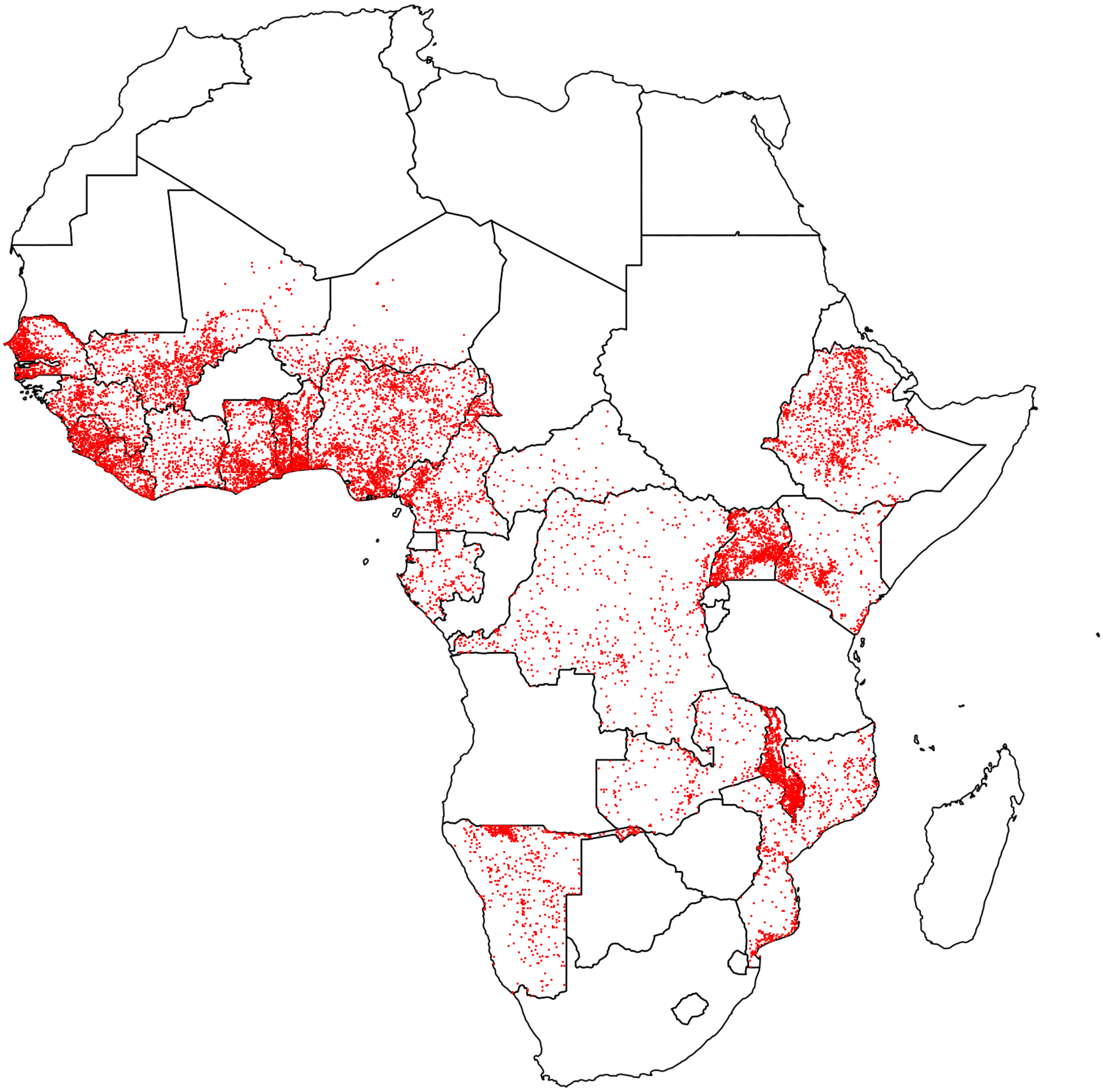

Figure A2: Geocoded DHS respondents across all rounds. Each point corresponds to one sampling cluster

Table A3: Ethnic Groups Upgraded to Political Power

Country	Year	EPR Group	Polity IV
Benin	1968	Northern (Bariba, Peul, Ottamari, etc.)	-7
Benin	1970	Southeastern (Yoruba/Nagot and Goun)	-2
Benin	2007	South/Central (Fon)	7
CAR	1966	Northern groups (Baya, Banda, Mandjia, etc.)	-7
CAR	2004	Yakoma	-1
CAR	2009	Sara	-1
DRC	1961	Ngbandi	0
DRC	1965	Lunda-Yeke	-9
DRC	1998	Luba Shaba	0
DRC	1998	Tutsi-Banyamulenge	0
DRC	1999	Other Kivu groups	0
DRC	2004	Mbandja	3
DRC	2004	Ngbaka	3
DRC	2004	Ngbandi	3
DRC	2004	Tutsi-Banyamulenge	3
Cote d'Ivoire	2000	Kru	4
Cote d'Ivoire	2000	Southern Mande	4
Cote d'Ivoire	2003	Northerners (Mande and Voltaic/Gur)	0
Ethiopia	1992	Oroma	0
Ethiopia	1992	Tigry	0
Ethiopia	2013	Other Southern Nations	-3
Gabon	1968	Eshira/Bapounou	-9
Gabon	1968	Myene	-9
Ghana	1972	Ewe	-7
Guinea	2009	Malinke	-1
Guinea	2009	Peul	-1
Kenya	1979	Mijikenda	-6
Kenya	2003	Kikuyu-Meru-Emb	8
Kenya	2003	Luo	8
Kenya	2008	Kisii	7
Kenya	2008	Luo	7
Liberia	2004	Krahn (Guere)	3
Liberia	2004	Mandingo	3
Malawi	1995	Southerners (Lomwe, Mang'anja, Nyanja, Yao)	6
Malawi	1995	Northerners (Tumbuka, Tonga, Ngonde)	6
Mali	1991	Tuareg	0
Mali	1991	Arabs/Moors	0
Mali	1996	Tuareg	7
Mali	1996	Arabs/Moors	7

Table A4: Ethnic Groups Upgraded to Political Power (continued)

Country	Year	EPR Group	Polity IV
Niger	1992	Hausa	8
Niger	1992	Kanouri	8
Niger	1994	Tuareg	8
Niger	2000	Hausa	5
Niger	2000	Kanouri	5
Niger	2000	Toubou	5
Niger	2000	Tuareg	5
Niger	2012	Hausa	6
Niger	2012	Kanouri	6
Niger	2012	Toubou	6
Niger	2012	Tuareg	6
Nigeria	1967	Yoruba	-7
Nigeria	1971	Igbo	-7
Nigeria	1971	Ijaw	-7
Nigeria	1971	Tiv	-7
Nigeria	1999	Igbo	4
Nigeria	1999	Yoruba	4
Nigeria	2008	Ijaw	4
Sierra Leone	1968	Creole	1
Sierra Leone	2006	Kono	5
Sierra Leone	2006	Northern Groups (Temne, Limba)	5
Togo	1963	Kabré (and related groups)	-6
Togo	1991	Ewe (and related groups)	-5
Togo	2006	Ewe (and related groups)	-4
Uganda	1986	Baganda	-7
Uganda	1986	Basoga	-7
Uganda	1986	South-Westerners (Ankole, Banyoro, Toro, Banyarw.)	-7

Table A5: Ethnic Groups Downgraded from Political Power

Country	Year	EPR Group	Polity IV
Benin	1964	Northern (Bariba, Peul, Ottamari, etc.)	-4
Benin	1968	Southeastern (Yoruba/Nagot and Goun)	-7
Benin	1996	South/Central (Fon)	6
CAR	1970	Northern groups (Baya, Banda, Mandjia, etc.)	-7
CAR	2002	Yakoma	5
CAR	2006	Sara	-1
DRC	1961	Luba Shaba	0
DRC	1961	Tetela-Kusu	0
DRC	1966	Bakongo	-9
DRC	1966	Mongo	-9
DRC	1998	Mbandja	0
DRC	1998	Ngbaka	0
DRC	1998	Ngbandi	0
DRC	1999	Tutsi-Banyamulenge	0
DRC	2007	Mbandja	5
DRC	2007	Ngbaka	5
DRC	2007	Ngbandi	5
DRC	2007	Tutsi-Banyamulenge	5
Cote d'Ivoire	1994	Northerners (Mande and Voltaic/Gur)	-6
Cote d'Ivoire	2012	Kru	4
Gabon	1963	Eshira/Bapounou	-7
Ghana	1970	Ewe	3
Guinea	1986	Malinke	-7
Guinea	1986	Peul	-7
Guinea	2011	Peul	1
Kenya	1967	Luo	0
Kenya	1979	Kikuyu-Meru-Emb	-6
Kenya	2003	Kisii	8
Kenya	2006	Luo	8
Liberia	1981	Americo-Liberians	-7
Mali	1994	Tuareg	7
Mali	1994	Arabs/Moors	7

Table A6: Ethnic Groups Downgraded from Political Power (continued)

Country	Year	EPR Group	Polity IV
Niger	1997	Hausa	-6
Niger	1997	Kanouri	-6
Niger	1997	Tuareg	-6
Niger	2005	Tuareg	6
Niger	2010	Toubou	3
Niger	2011	Hausa	6
Niger	2011	Kanouri	6
Nigeria	1965	Igbo	7
Nigeria	1979	Ijaw	7
Nigeria	1979	Tiv	7
Nigeria	1979	Yoruba	7
Nigeria	1984	Igbo	-7
Sierra Leone	1965	Creole	6
Sierra Leone	1965	Northern Groups (Temne, Limba)	6
Sierra Leone	1968	Mende	1
Sierra Leone	2013	Mende	7
Togo	1967	Ewe (and related groups)	-7
Togo	1992	Ewe (and related groups)	-3
Uganda	1966	Baganda	0
Uganda	1966	Basoga	0
Uganda	1966	South-Westerners (Ankole, Banyoro, Toro, Bany.)	0
Uganda	1972	Langi/Acholi	-7

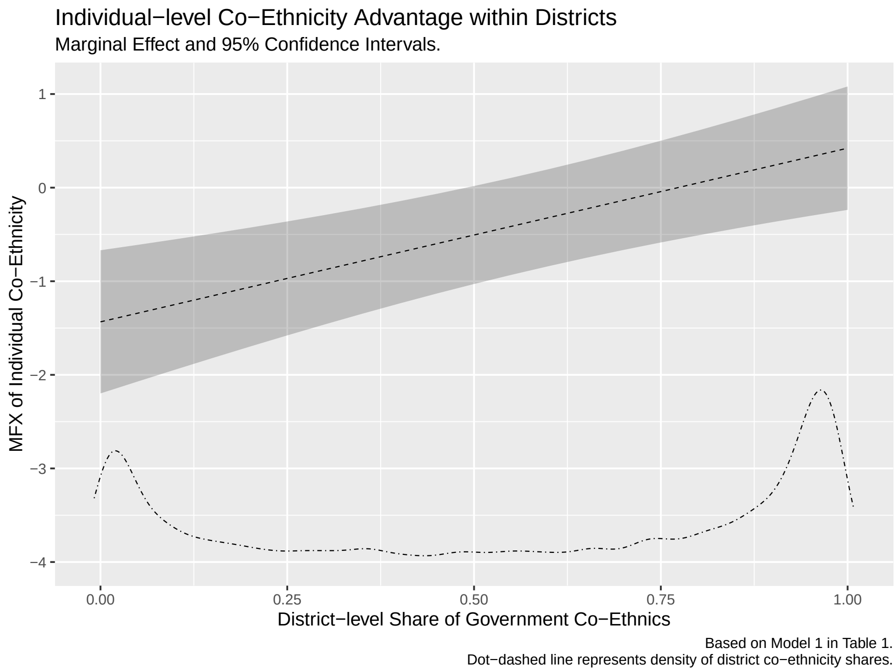

Figure A3: Marginal Effect of Individual-Level Government Co-Ethnicity across District-level Co-Ethnicity Shares

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 A4 and A5 plot the binning

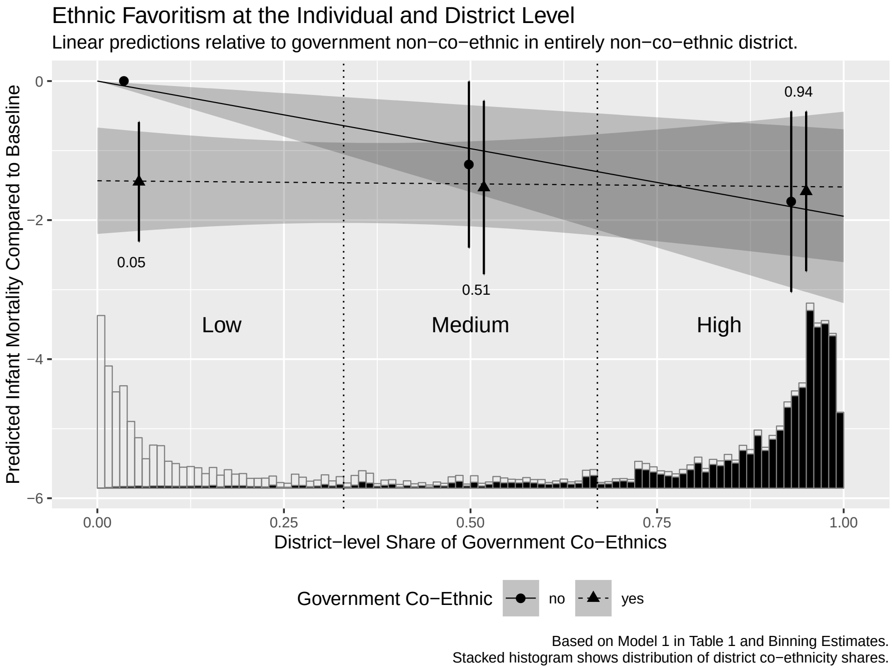

Figure A4: Predictions from Baseline Model & Binning Estimates

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.

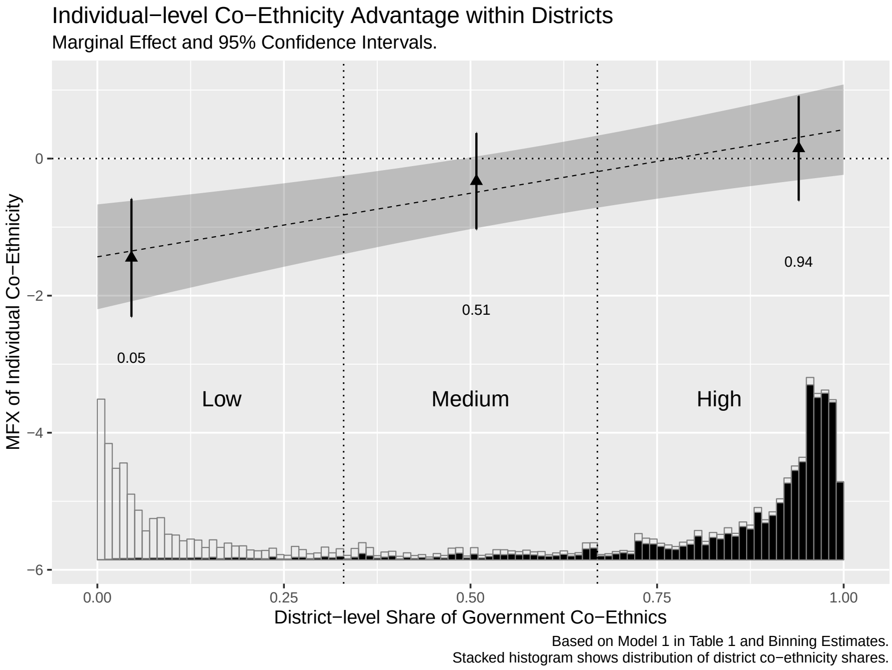

Figure A5: Marginal Effect of Individual Government Co-Ethnicity from Baseline Model & Binning Estimates

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 A7 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 A7: Different Temporal Lags

	Main explanatory variables measured at . . .			
	t	t-1	t-2	t-3
Government Co-Ethnic	-1.135^{***}	-1.434^{***}	-1.511^{***}	-1.371^{***}
	(0.365)	(0.390)	(0.385)	(0.430)
Dist. Share Government Co-Ethnics	-1.779^{***}	-1.944^{***}	-1.383^{**}	-0.934
	(0.615)	(0.637)	(0.643)	(0.669)
Co-Ethnic × Dist. Share Co-Ethnics	1.875^{***}	1.855^{***}	1.903^{***}	1.817^{***}
	(0.477)	(0.496)	(0.502)	(0.546)
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	1,501,139	1,485,226	1,478,089	1,470,979
Adjusted R^2	0.057	0.057	0.057	0.057

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.62 infant deaths per 100 live births. Observations are weighted to ensure equal weights for each country. Control variables include mothers' education, 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

Clustering standard errors: Table A8 reports differently clustered standard errors. In particular, we cluster standard errors at increasingly large units:

1. Districts and ethnic groups
2. Survey-regions (1^{st}-level administrative units)
3. Regions (1^{st}-level administrative units)
4. Survey-countries
5. Countries

The standard errors of our main independent variable increase only marginally in size as a result without however causing any loss of statistical significance.

Table A8: Robustness: Different Standard Error Clustering

	Infant Mortality U1				
	(1)	(2)	(3)	(4)	(5)
Government Co-Ethnic (t-1)	-1.434^{***}	-1.434^{***}	-1.434^{***}	-1.434^{***}	-1.434^{***}
	(0.383)	(0.414)	(0.412)	(0.445)	(0.454)
Dist. Share Gov. Co-Ethnics (t-1)	-1.944^{***}	-1.944^{***}	-1.944^{***}	-1.944^{***}	-1.944^{***}
	(0.690)	(0.734)	(0.711)	(0.636)	(0.683)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.855^{***}	1.855^{***}	1.855^{***}	1.855^{***}	1.855^{***}
	(0.494)	(0.488)	(0.493)	(0.511)	(0.537)
SE Clustering	Dist. & Ethn.	ADM1-Survey	ADM1-Region	Country-Survey	Country
Survey-Ethnic FE	yes	yes	yes	yes	yes
Survey-District FE	yes	yes	yes	yes	yes
Survey-Region-Birthyear FE	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes
Observations	1,485,226	1,485,226	1,485,226	1,485,226	1,485,226
Adjusted R^2	0.060	0.060	0.060	0.060	0.060

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.62 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 A9 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 A9: Infant Mortality: Unweighted Regressions

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.284***		−1.430***	
	(0.356)		(0.387)	
Dist. Share Gov. Co-Ethnics (t-1)	−1.388**	−0.968		
	(0.565)	(0.677)		
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.775***	1.891***	1.933***	1.931***
	(0.453)	(0.475)	(0.489)	(0.524)
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	1,485,226	1,485,226	1,485,226	1,485,226
Adjusted R^2	0.054	0.054	0.057	0.057

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.62 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 variabels yields almost identical coefficient estimates and standard errors.

Table A10: No Control Variables

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.363***		−1.556***	
	(0.389)		(0.459)	
Dist. Share Gov. Co-Ethnics (t-1)	−2.108***	−2.024**		
	(0.683)	(0.903)		
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.831***	1.976***	1.804***	1.789***
	(0.510)	(0.569)	(0.537)	(0.590)
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	1,521,673	1,521,673	1,521,673	1,521,673
Adjusted R^2	0.041	0.043	0.051	0.052

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.62 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. 7 in the main paper), Table A11 estimates our baseline model using co-ethnicity with only the senior ethnic groups[28] 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.

Table A11: Government Senior Partners

	Infant Mortality			
	(1)	(2)	(3)	(4)
Senior Government Co-Ethnic (t-1)	−0.988***		−1.121***	
	(0.356)		(0.394)	
Dist. Share Senior Gov. Co-Ethnics (t-1)	−1.118*	−1.321*		
	(0.592)	(0.727)		
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	2.158***	2.342***	2.412***	2.609***
	(0.570)	(0.605)	(0.641)	(0.654)
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	1,491,961	1,491,961	1,491,961	1,491,961
Adjusted R^2	0.061	0.063	0.070	0.071

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.62 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 – the one that models it with regard to individual-level co-ethnicity, and the one that focuses on geographical regions – Table A12 presents the results of 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 survival of an infant by .6 percentage points (Models 1 and 2). This is similar to Franck and Rainer (2012),

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

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 is because the simpler models average over the heterogeneous strategies of ethnic favoritism.

Table A12: Infant Mortality: Ethnic vs. District-level Diff-in-Diff

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−0.603***	−0.633***		
	(0.203)	(0.206)		
Dist. Share Gov. Co-Ethnics (t-1)			−1.640**	−1.515**
			(0.658)	(0.656)
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	1,522,398	1,485,951	1,535,859	1,499,402
Adjusted R^2	0.037	0.057	0.041	0.061

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.62 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 A13 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 before 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.[29] Only two terms are sizable and significant. First,the one-year downgrade lead in Model 1 indicating that represented groups benefit more in

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

the final year of their governing spell than in other years in power. Secondly, the two-year downgrade lead in Model 2 is positive and significant which is potentially more worrying. However, the sum of all three downgrade leads remains insignificant and none of the lienar trends in columns 3 and 4 is significant.

Table A13: Robustness: Pre-Trends

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.368***	−1.585***	−1.443***	−1.792***
	(0.395)	(0.414)	(0.422)	(0.448)
Dist. Share Gov. Co-Ethnics (t-1)	−1.877***	−2.429***	−2.295***	−2.533***
	(0.649)	(0.710)	(0.733)	(0.786)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	1.932***	2.122***	2.015***	2.296***
	(0.502)	(0.540)	(0.522)	(0.573)
Upgrade_{t+1}	−0.234	−0.411		
	(0.599)	(0.588)		
Upgrade_{t+2}		−0.410		
		(0.558)		
Upgrade_{t+3}		0.258		
		(0.575)		
Downgrade_{t+1}	−1.763*	−0.579		
	(0.993)	(1.161)		
Downgrade_{t+2}		1.409*		
		(0.789)		
Downgrade_{t+3}		0.317		
		(0.545)		
Pre-Trend $\text{Upgrade}_{t+3 \text{ to } t+1}$			−0.176	
			(0.318)	
Pre-Trend $\text{Downgrade}_{t+3 \text{ to } t+1}$			−0.305	
			(0.478)	
Pre-Trend $\text{Upgrade}_{t+5 \text{ to } t+1}$				−0.069
				(0.117)
Pre-Trend $\text{Downgrade}_{t+5 \text{ to } t+1}$				0.174
				(0.204)
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	1,477,920	1,435,906	1,461,818	1,398,519
Adjusted R^2	0.060	0.060	0.060	0.060

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.62 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

Table A14: Heterogeneity: Regime Type & Electoral System

	Infant Mortality		
	(1)	(2)	(3)
Government Co-Ethnic (t-1)	−1.640***	−1.694***	−1.987**
	(0.463)	(0.458)	(0.849)
Dist. Share Gov. Co-Ethnics (t-1)	−2.554***	−2.916***	−2.375
	(0.898)	(0.801)	(1.892)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	2.179***	1.973***	3.321**
	(0.609)	(0.626)	(1.310)
Co-Ethnic × High Polity IV (t-1)	0.587		
	(0.561)		
Dist. Share × High Polity IV (t-1)	1.198		
	(1.071)		
Co-Ethnic × Dist. Share × High Polity IV (t-1)	−0.880		
	(0.844)		
Co-Ethnic × High VDEM (t-1)		0.627	
		(0.518)	
Dist. Share × High VDEM (t-1)		1.774**	
		(0.844)	
Co-Ethnic × Dist. Share × High VDEM (t-1)		−0.494	
		(0.759)	
Co-Ethnic × Mostly FPTP (t-1)			0.948
			(0.815)
Dist. Share × Mostly FPTP (t-1)			−0.060
			(2.128)
Co-Ethnic × Dist. Share × Mostly FPTP (t-1)			−1.990
			(1.284)
Survey-Ethnic FE	yes	yes	yes
Survey-District FE	yes	yes	yes
Survey-Region-Birthyear FE	yes	yes	yes
Controls	yes	yes	yes
Observations	1,485,117	1,485,132	995,718
Adjusted R^2	0.060	0.060	0.058

Notes: OLS linear probability models. The sample mean of the dependent variable is 10.62 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 A14 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 A6 and A7 plot predictions and differences derived from these first two triple interaction models. The upper two panels in both of these figures replicate Figure 5 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 A6, the general pattern of effects remains similar across more 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 A6, 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 A7 reveals qualitatively very similar results for the VDEM-based interaction models. In contrast to the Polity models, however, the differences in our overall predic-

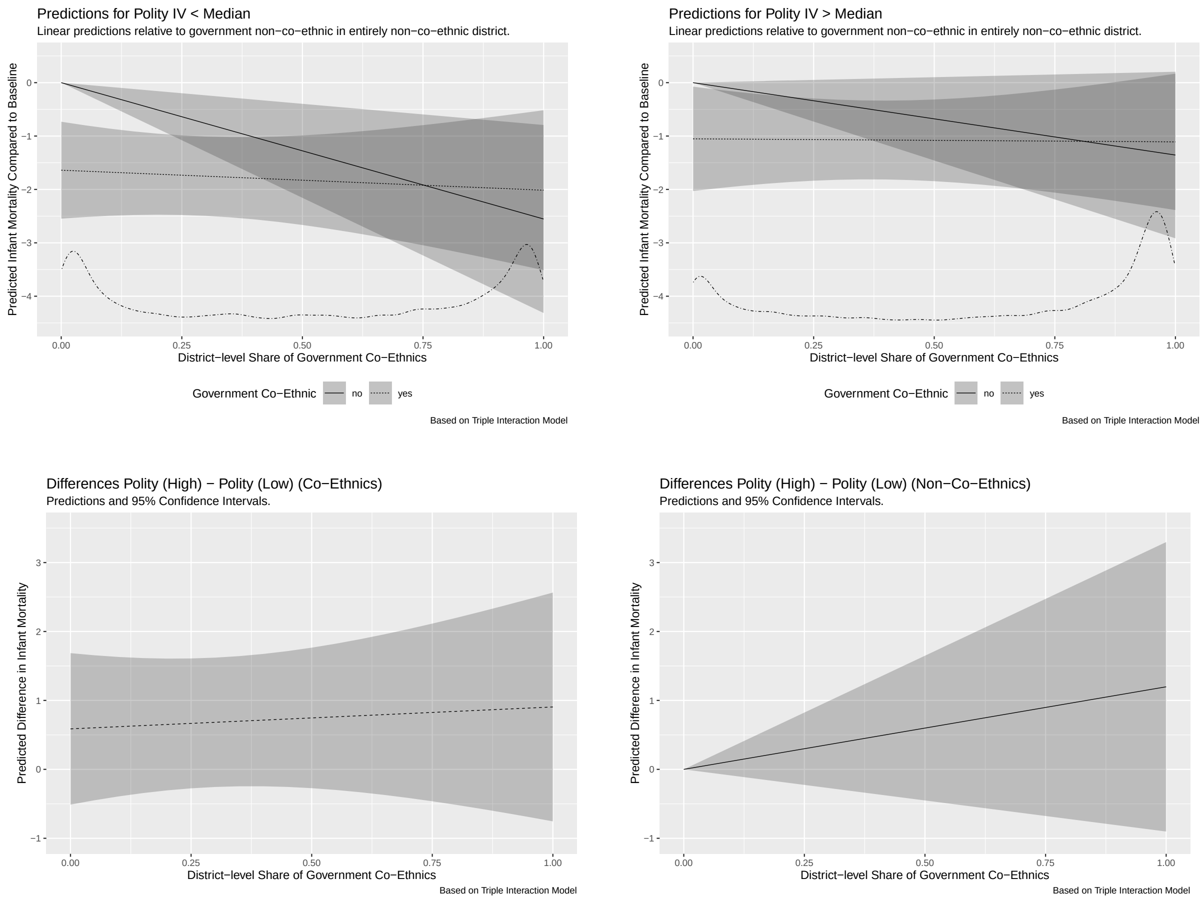

Figure A6: Predictions according to Polity IV Value (below vs. above median)

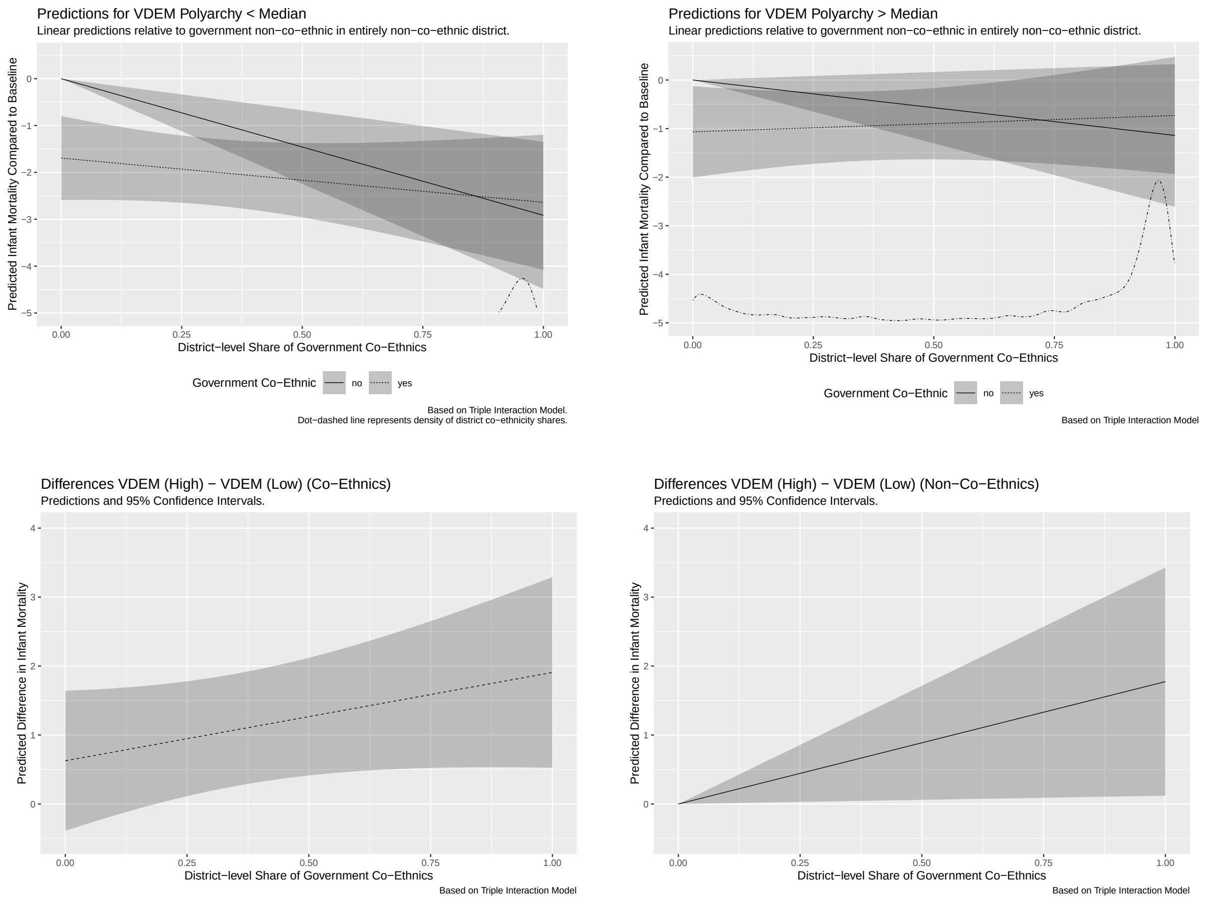

Figure A7: Predictions according to VDEM Polyarchy Value (below vs. above median)

tions 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 A14 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,

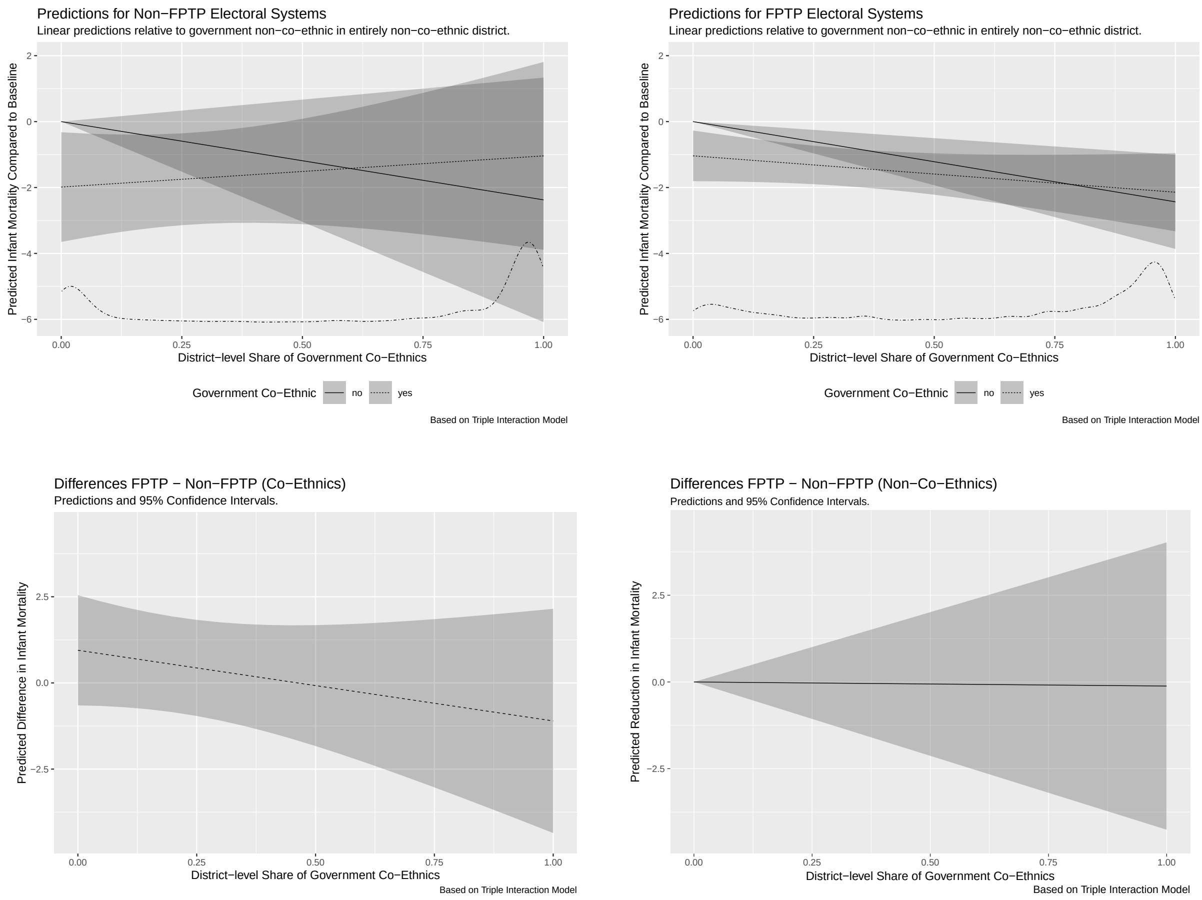

Figure A8: Predictions according to Electoral System (PR and mixed vs. FPTP systems)

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 A8 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,[30] 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 A15). 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 A16). 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

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

Table A15: Principal component analysis: Economic hardship

Component	Eigenvalue	Variance Explained	Variable	Factor loadings 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 A16: Principal component analysis: Perceived service accessibility

Component	Eigenvalue	Variance Explained	Variable	Factor loadings 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 A17, 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 A17: 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 A18 and A19 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 A19 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 A18: 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 A19: 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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