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

ONLINE APPENDIX

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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.

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A1 Summary statistics

Table A1: Summary Statistics (DHS Data)

Statistic	N	Mean	St. Dev.	Min	Max
Infant Death	1,540,884	10.749	30.973	0	100
Government Co-Ethnic (t-1)	1,540,884	0.575	0.494	0	1
District Share Government Co-Ethnics (t-1)	1,540,884	0.579	0.396	0.000	1.000
Senior Government Co-Ethnic (t-1)	1,540,884	0.296	0.456	0	1
District Share Senior Government Co-Ethnics (t-1)	1,540,884	0.298	0.357	0.000	0.993
Upgrade to Political Inclusion	1,540,884	0.009	0.095	0	1
Downgrade to Political Exclusion	1,540,884	0.004	0.060	0	1
Mother's education	1,540,884	1.538	0.749	1	4
Mother's age	1,503,930	24.463	6.526	10	49
Birthorder	1,540,884	3.406	2.305	1	18
Female	1,540,884	0.460	0.498	0	1
Twin or Higher Multiple Birth	1,540,884	0.034	0.181	0	1
Co-Ethnic President (t-1)	621,073	0.121	0.327	0	1
Dist. Share Co-Ethnics President (t-1)	621,073	0.118	0.233	0.000	0.968
Co-Ethnic Top Minister (t-1)	621,073	0.546	0.498	0	1
Dist. Share Co-Ethnics Top Min. (t-1)	621,073	0.498	0.384	0.000	1.000
Co-Ethnic Any Minister (t-1)	621,073	0.745	0.436	0	1
Dist. Share Co-Ethnics Any Min. (t-1)	621,073	0.646	0.361	0.0002	1.000
Polity IV > Median (t-1)	1,540,731	0.439	0.496	0	1
VDEM Polyarchy > Median (t-1)	1,540,790	0.487	0.500	0	1
FPTP Electoral System (t-1)	1,030,592	0.681	0.466	0	1
Prenatal Assistance	237,975	76.831	42.191	0	100
Prenatal Asst. by Doctor	227,151	10.796	31.033	0	100
Institutional Birth	341,189	49.095	49.992	0	100
Assisted Birth	339,839	49.284	49.995	0	100
Birth assisted by Doctor	322,265	5.824	23.419	0	100
Low Birthweight ($\leq$ 2500g)	132,713	17.206	37.744	0	100
Year	1,540,884	1,995.157	9.940	1955	2013

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[1] between different ethnic groups.

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

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

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

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

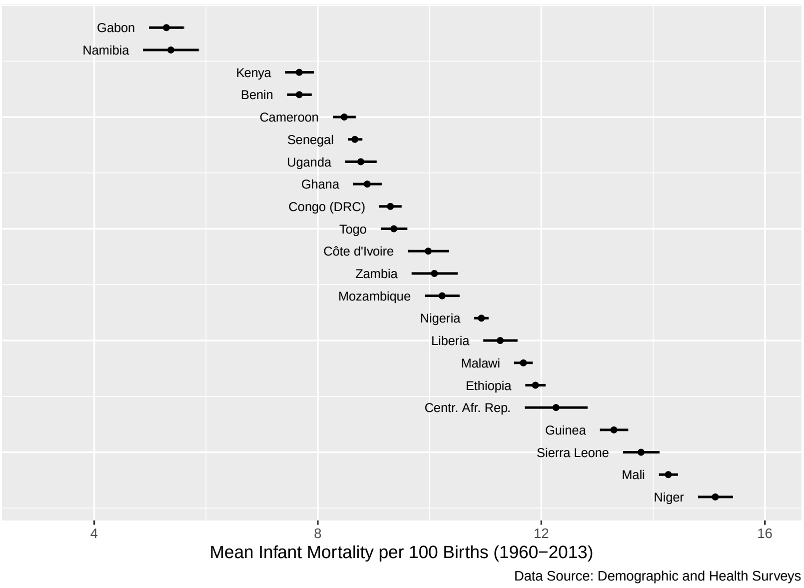


Figure A1: Mean infant mortality in the 22 countries in our sample, 1960–2013 (means and 95% confidence intervals)

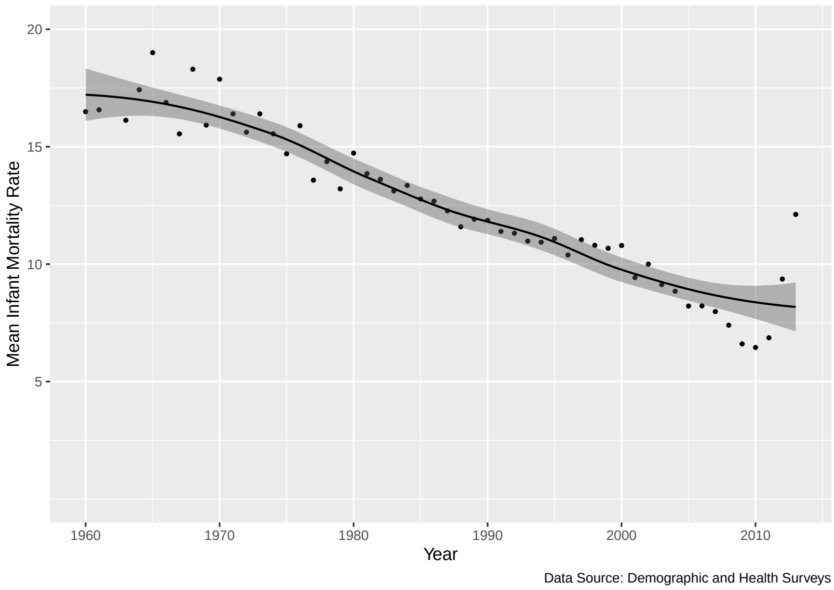
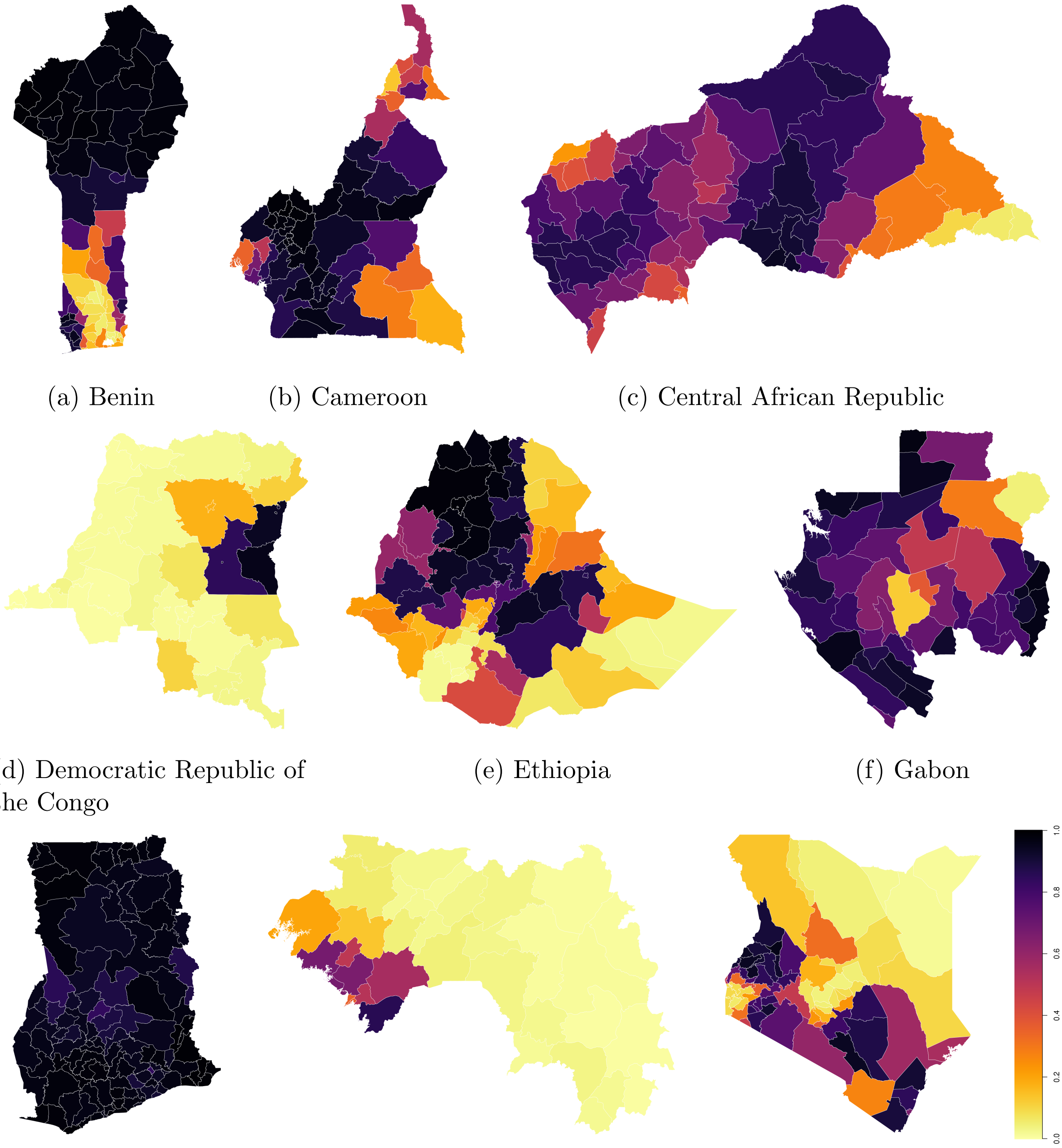


Figure A2: Annual infant mortality per 100 births across the 22 African countries in our sample (mean, LOESS curve and 95% confidence interval)

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

Figure A5 provides information on which ethnic groups are coded by the Ethnic Power Relations database (Vogt et al., 2015) as being included in the government for every year in the 22 countries for which we have geocoded DHS data. Since our main models only exploit variation within ethnic groups and districts, all effects are identified from temporal changes in individual and district-level co-ethnicity with the government due to the political upgrades and downgrades visualized by the Figure.

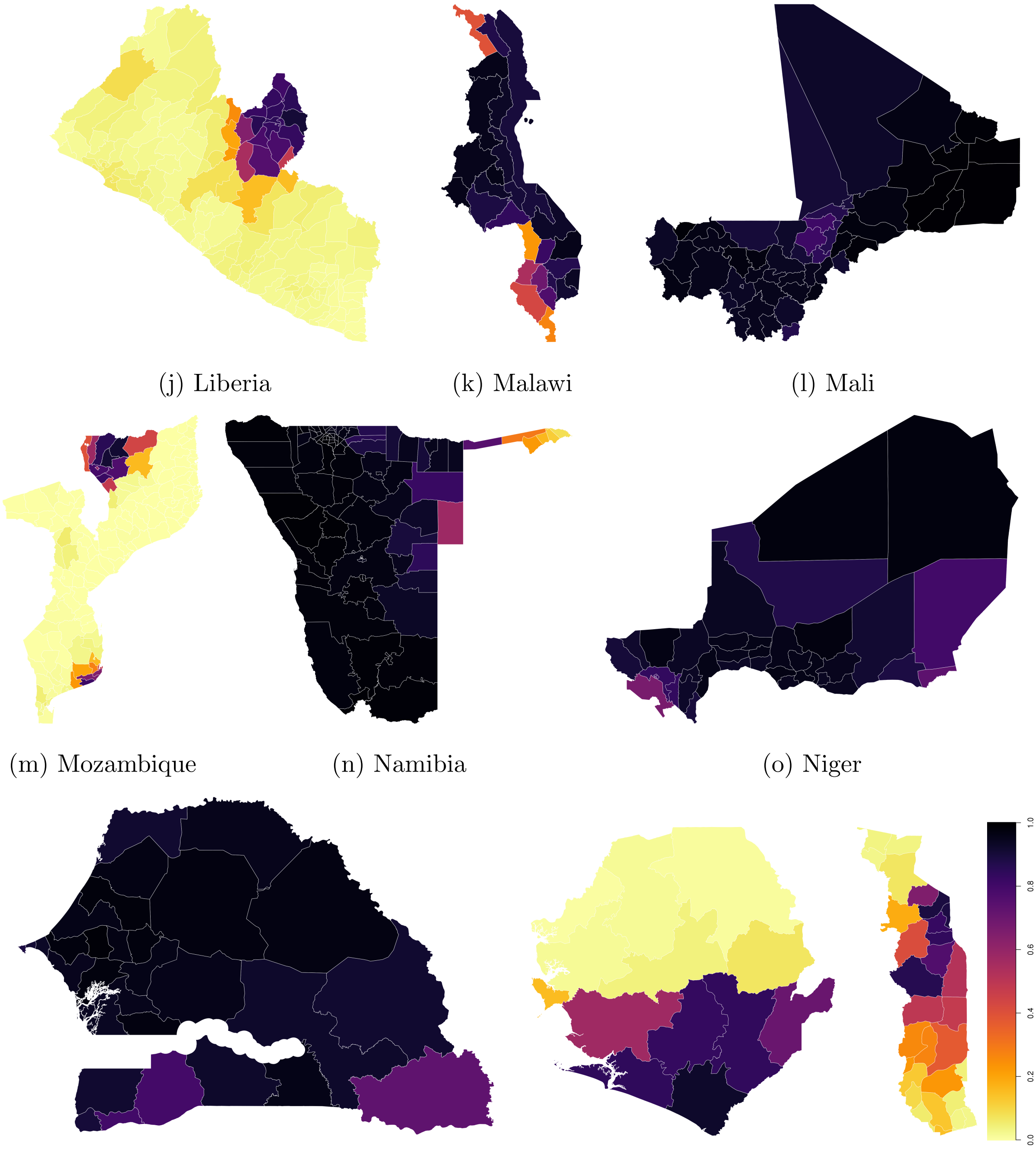


(a) Benin (b) Cameroon (c) Central African Republic

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

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

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



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

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

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

Figure A3: Continued

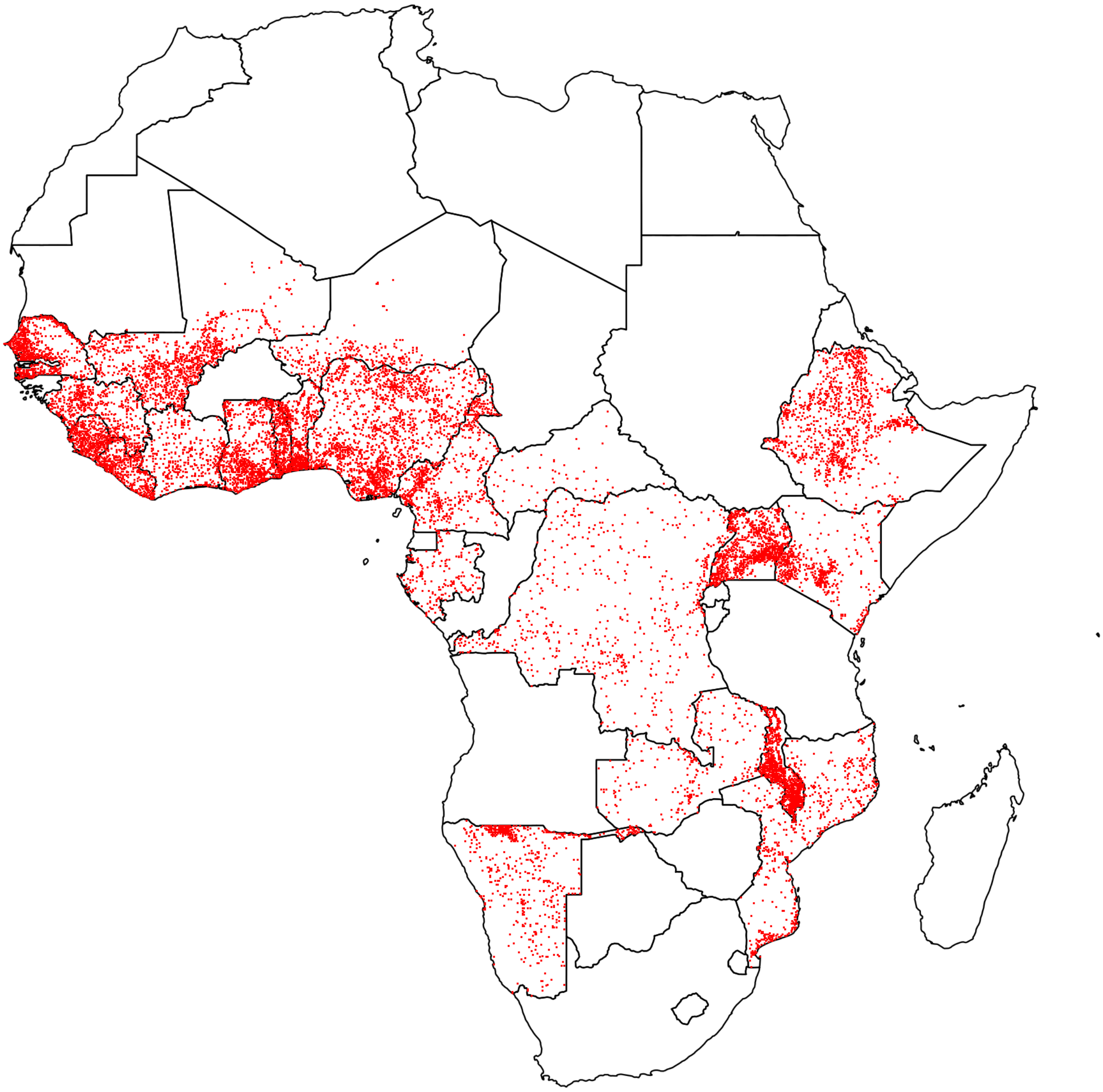


Figure A4: Geocoded DHS respondents across all rounds. Each point corresponds to one sampling cluster

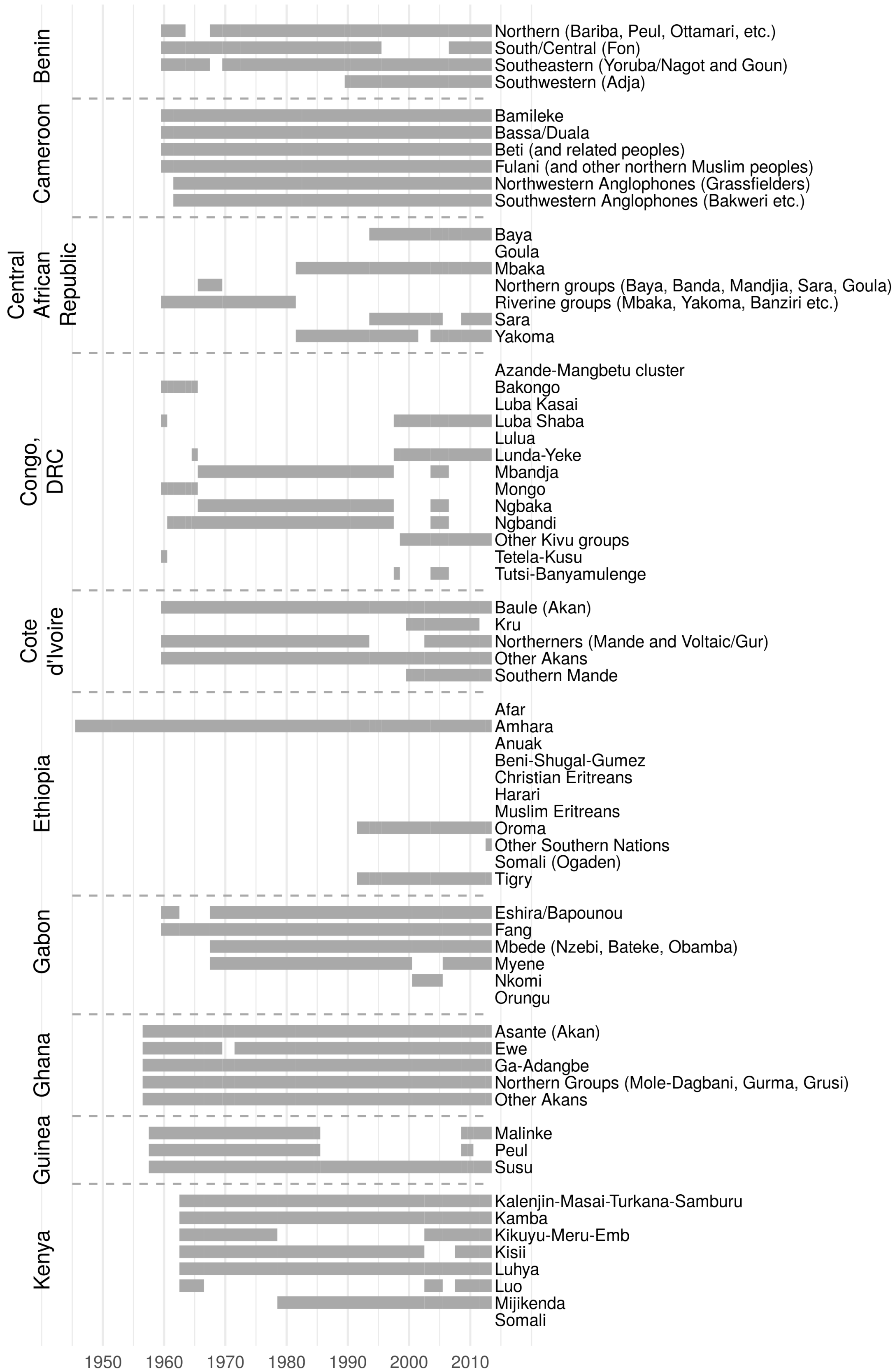


Figure A5: Inclusion of ethnic groups into governments in our sample.
Grey bars show spells within which EPR groups are coded as being included in the national executive. Groups are coded as 'excluded' or 'irrelevant' during all other spells. The politically relevant constellation of ethnic groups at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

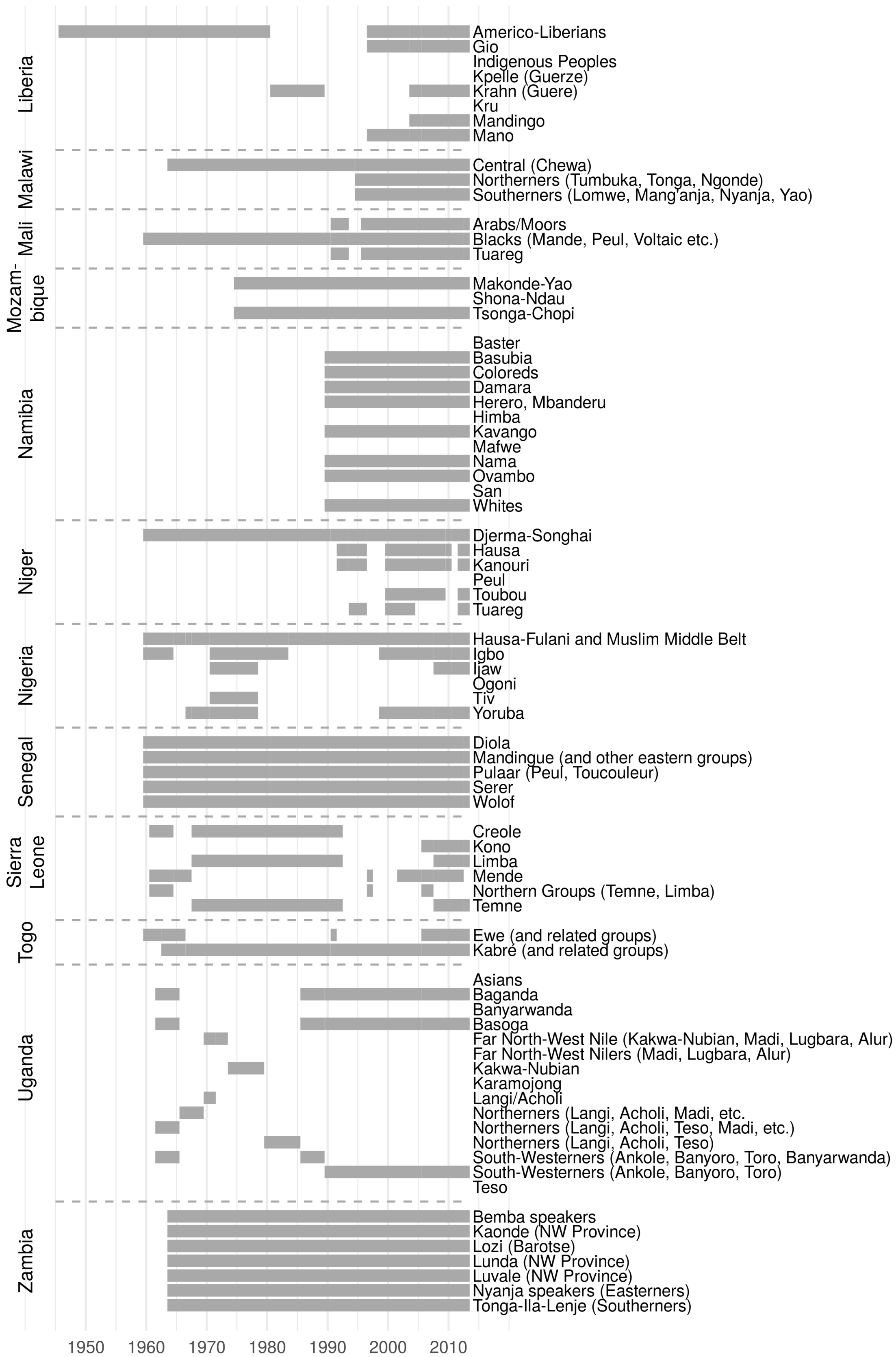


Figure A5: Continued.

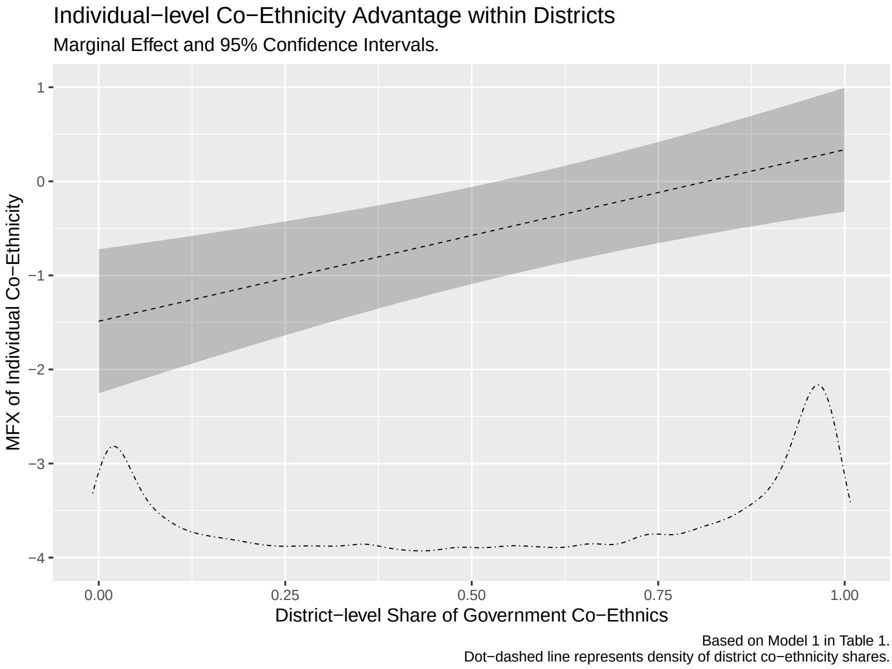


Figure A6: Marginal Effect of Individual-Level Government Co-Ethnicity across District-level Co-Ethnicity Shares

A2 Robustness checks

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

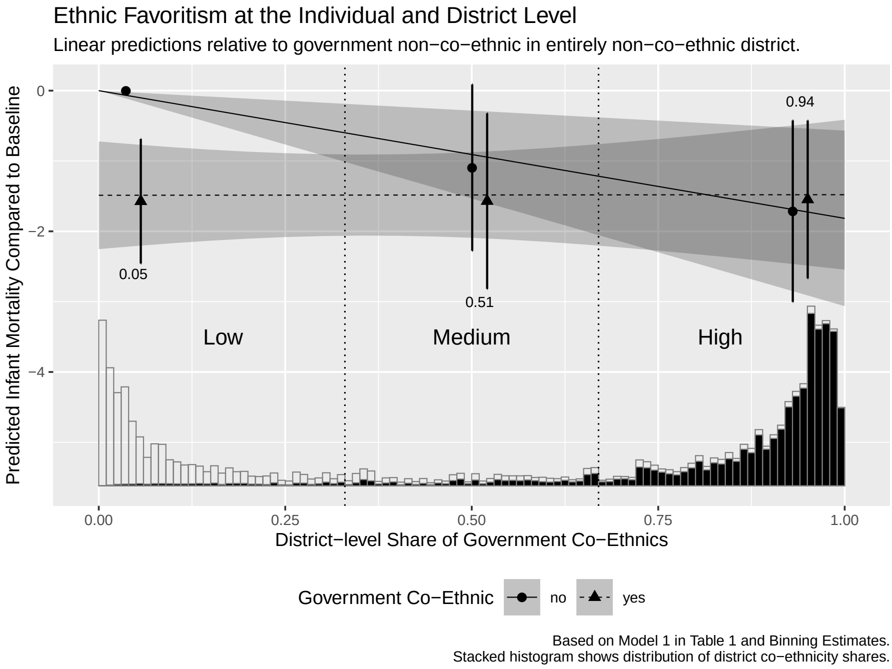


Figure A7: Predictions from Baseline Model & Binning Estimates

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

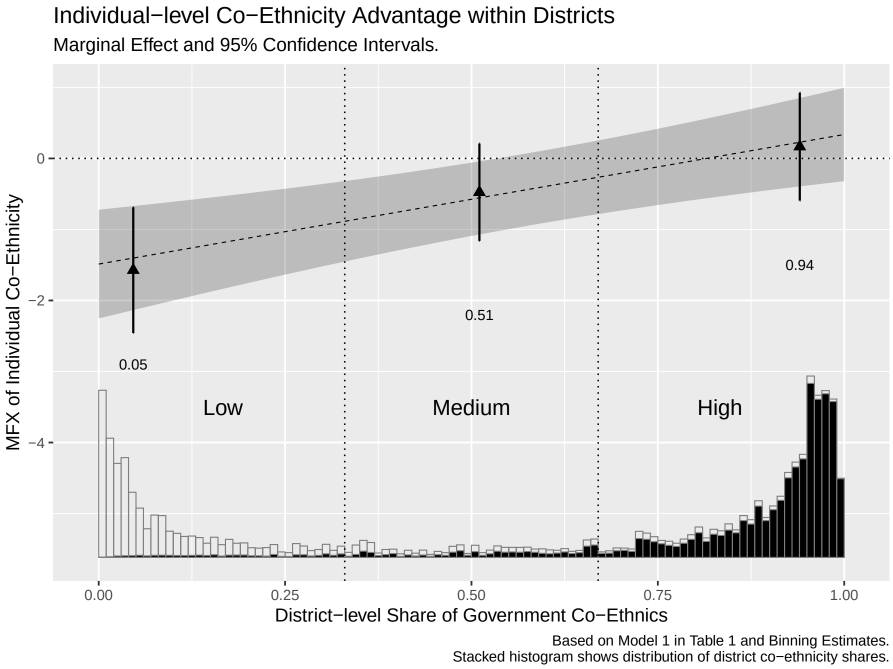


Figure A8: Marginal Effect of Individual Government Co-Ethnicity from Baseline Model & Binning Estimates

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

Table A3: Different Temporal Lags

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

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

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

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

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

Table A4: Differently Clustered Standard Errors

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

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

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

Table A5: Infant Mortality: Unweighted Regressions

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

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

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

Table A6: No Control Variables

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

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

Co-ethnicity with senior partners only: To check the robustness of our results with regards to modifying our theoretical assumption that all coalition partners matter equally, Table A7 estimates our baseline model using co-ethnicity with only the senior ethnic groups[2] 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.

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

Table A7: Government Senior Partners

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

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

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

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

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

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

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

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

Table A9: Robustness: Pre-Trends

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

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

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

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

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

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

Table A10: Robustness: Enumeration Areas & Spatial Segregation

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Table A11: Robustness: FRT Data

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

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

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

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

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

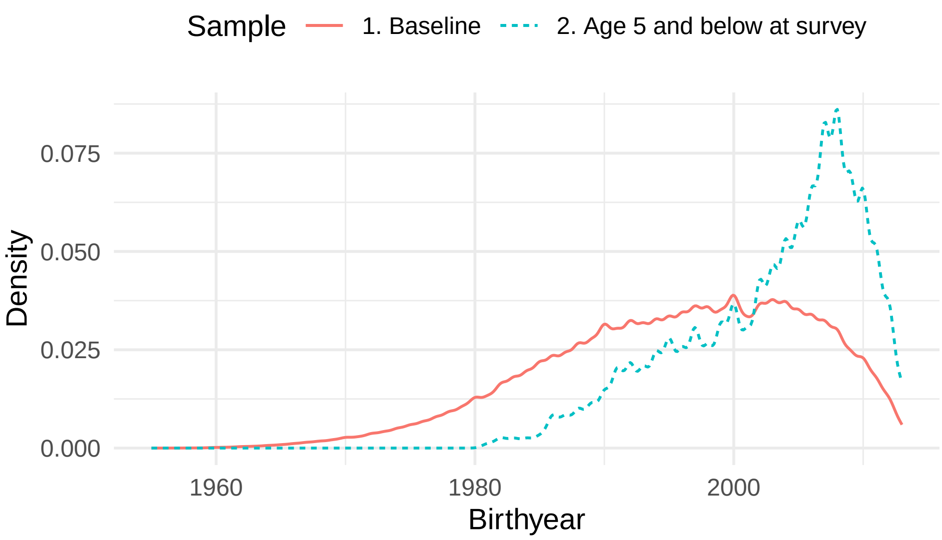


Figure A9: Distribution of birthyears in baseline sample and among infants with additional information

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

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

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

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

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

Table A12: Alternative Outcomes, Children Age 5 and Below

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

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

Table A13: Alternative Outcomes, Children Age 5 and Below

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

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

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

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

Table A14: Household and Mother Fixed Effects

	Infant Mortality			
	(1)	(2)	(3)	(4)
Government Co-Ethnic (t-1)	−1.262**	−1.377**	−0.755	−0.635
	(0.615)	(0.593)	(0.625)	(0.616)
Dist. Share Gov. Co-Ethnics (t-1)	−1.879***	−1.513*	−1.706**	−0.476
	(0.646)	(0.883)	(0.724)	(1.032)
Co-Ethnic × Dist. Share Co-Ethnics (t-1)	2.106**	1.940**	1.186	0.348
	(0.979)	(0.891)	(1.040)	(0.957)
Household FE	yes	yes	–	–
Mother FE	no	no	yes	yes
Birthyear FE	yes	no	yes	no
Survey-Region-Birthyear FE	no	yes	no	yes
Controls	yes	yes	yes	yes
Observations	1,503,930	1,503,930	1,503,930	1,503,930
Adjusted R^2	0.094	0.109	0.103	0.118

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

A3 Heterogeneous Effects?

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

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

Table A15: Heterogeneity: Regime Type & Electoral System

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

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

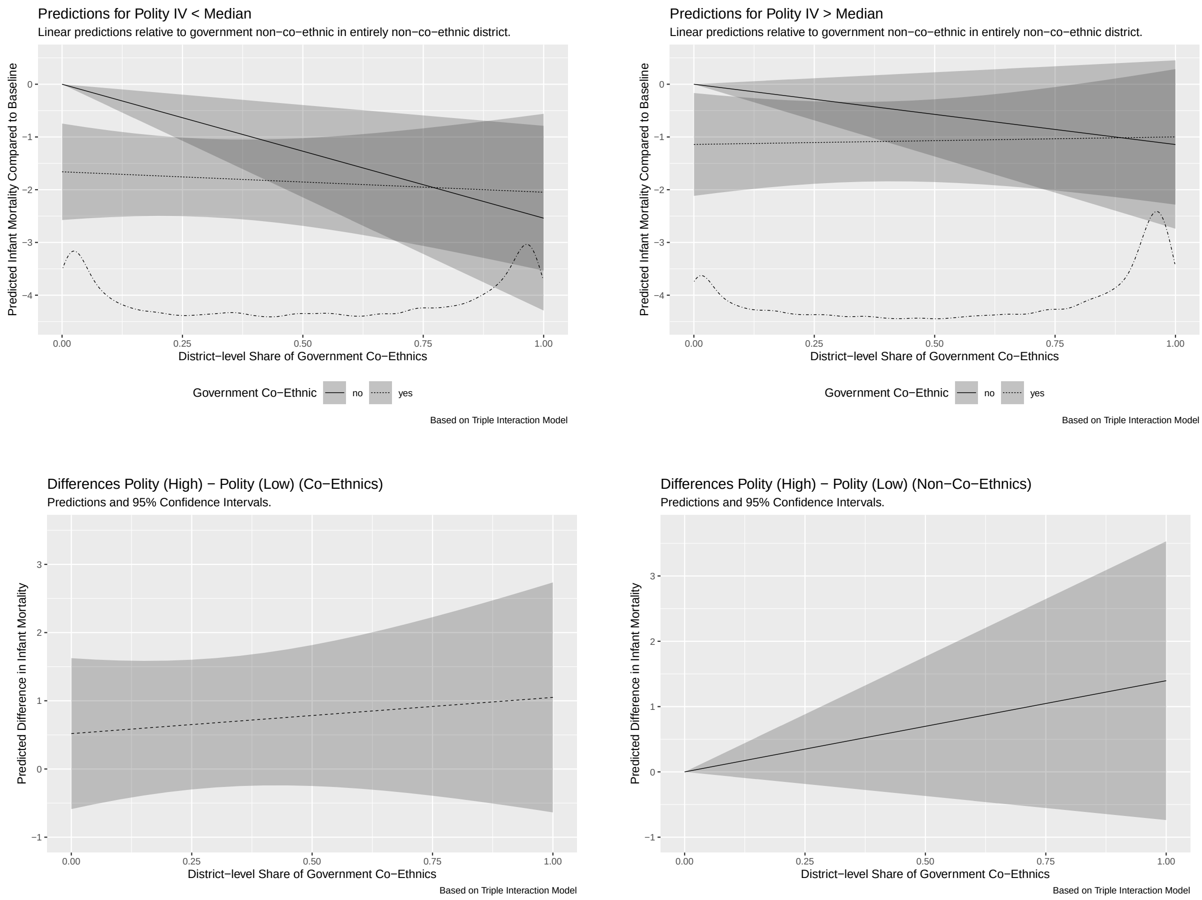


Figure A10: Predictions according to Polity IV Value (below vs. above median)

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

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

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

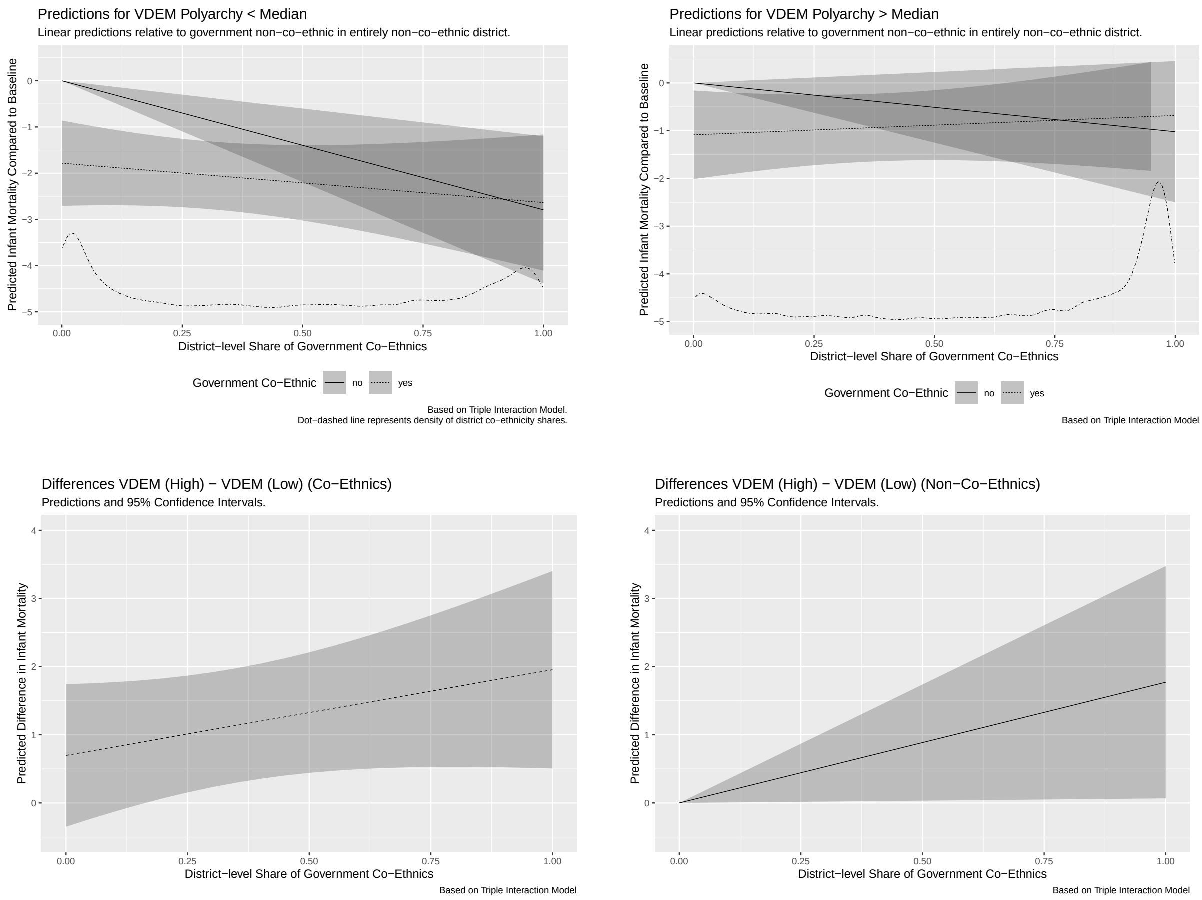


Figure A11: Predictions according to VDEM Polyarchy Value (below vs. above median)

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

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

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

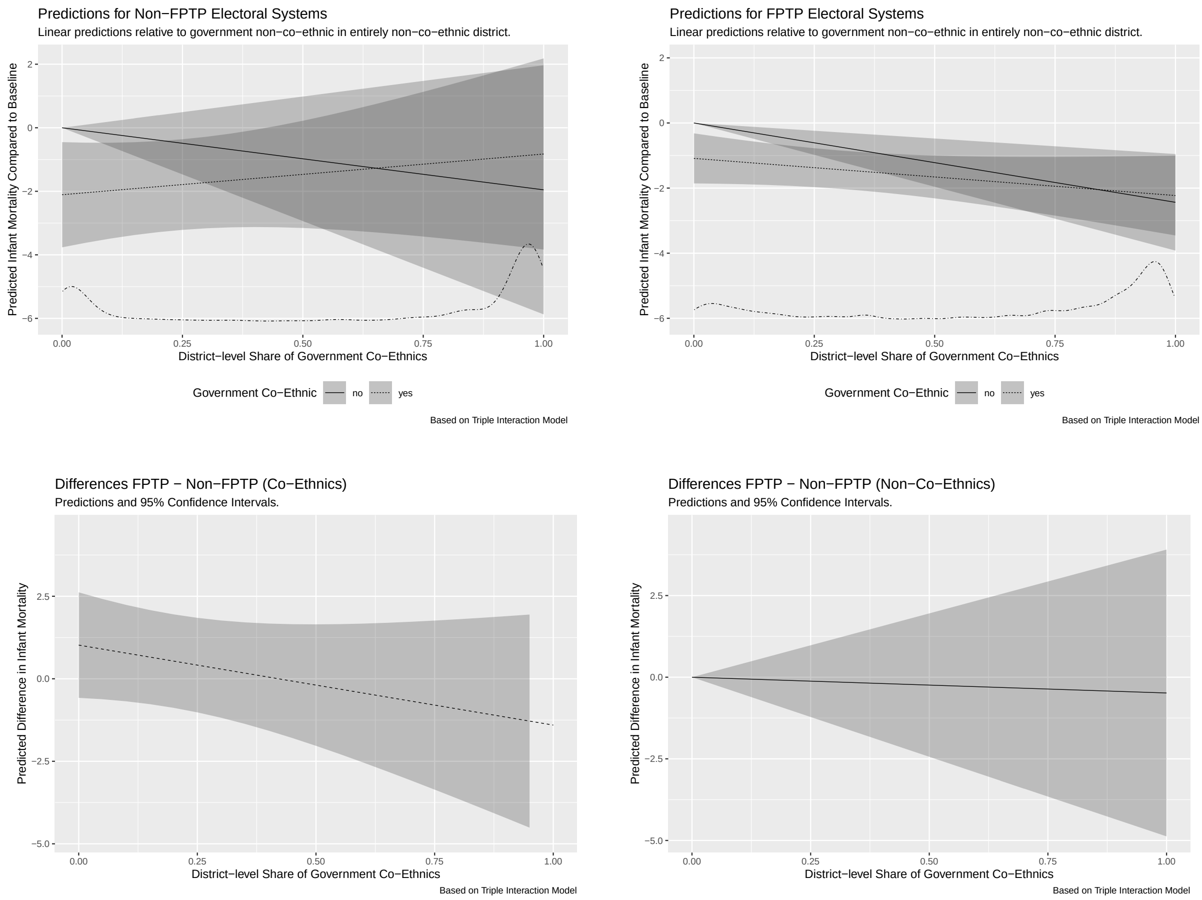


Figure A12: Predictions according to Electoral System (PR and mixed vs. FPTP systems)

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

A4 Afrobarometer

Data and empirical strategy

The Afrobarometer surveys (Afrobarometer, 2015), rounds 1–5,[4] cover a wide array of political topics. Among many other issues, respondents are asked about their economic well-being and perceptions of public service provision. We use the related questions to mitigate the shortcomings of the DHS infant mortality measure:

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

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

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

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

Table A16: Principal component analysis: Economic hardship

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

Table A17: Principal component analysis: Perceived service accessibility

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

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

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

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

Table A18: Afrobarometer: Summary statistics

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

Robustness checks

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

Table A19: Economic hardship indicators: Cross-sectional OLS

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

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

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

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

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

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