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Abstract

We study favoritism by cabinet members in 36 African countries and hand-collect birthplace information for all cabinet members (2001-2015). We focus on health outcomes and provide causal evidence of favoritism by health ministers and, to a lower degree, key ministers. Neonates' and infants' mortality is lower when the current health minister originates from their region. Increased healthcare access can partly explain this effect. Moreover, we find some evidence that health aid flows to ministers' regions increase. However, our results imply that health ministers also channel other resources to their region. We conclude that ministers' favoritism manifests itself in diverse ways.

Keywords

infant mortality, child health, favoritism, political capture, patronage, clientelism, corruption, aid allocation, Africa, World Bank, georeferenced data, spatial analysis

JEL Classification

D73, F35, I10, J13, I18, R11

1 Introduction

A widely held view of African politics is the "big man theory," according to which country leaders are relatively unconstrained in their exercise of power. This view is supported by previous work showing that country leaders in Africa and elsewhere distort the allocation of public funds to favor their own birth regions and ethnic groups (e.g., Franck and Rainer, 2012; Hodler and Raschky, 2014; Burgess et al., 2015; Kramon and Posner, 2016; De Luca et al. 2018; Dickens, 2018). At the same time, Francois et al. (2015) provide evidence showing that ethnic groups are represented in the cabinet according to their population share, suggesting that power is more widely distributed than often assumed. However, whether cabinet positions translate into actual power or whether broad representation in a cabinet is merely symbolic remains unclear. Motivated by this question, we examine whether cabinet members can engage in favoritism targeting their birth region.

Focusing on favoritism related to health, we study whether children born in the same region as a current health minister are less likely to die before reaching the age of one month or one year (World Health Organization, 2019a; World Health Organization, 2019b). Our focus on health comes with several advantages. Standardized subnational health data is available for various African countries in different years, as provided by the Demographic and Health Surveys (DHS) Program. We focus on neonatal and infant mortality as they are often used as proxies for population health outcomes in settings where health data is scarce. Also, importantly, as the DHS program contains retrospective information on the health of the interviewed women and all their children, we can construct a dataset where a mother is the panel unit and each of her children is one unit of observation. We hence compare siblings, which allows us to study favoritism in a quasi-experimental setting.

We compile a novel dataset on cabinet members in 36 African countries between 2001 and 2015. We cover all African countries for which DHS data is available, except for those with less than 1 million inhabitants. We extract a list of all cabinet members and their designations (e.g., health minister, finance minister) from the CIA World Factbook. Then, we hand-collect birthplace information for all cabinet members. We find birthplace information for 73% of health ministers (and 76% of all cabinet members). To our knowledge, we are the first to offer data on the birthplaces of African cabinet members with close to continent-wide coverage.¹

¹While we focus on the 36 countries for which DHS data is available in this study, our replication dataset comprises ministers' birthplace information for all African countries with more than 1 million inhabitants (47 countries). Francois et al. (2015) also collect information on the regional affiliation of ministers in Africa. Instead of birthplaces, they are interested in the ethnicity of ministers. They focus on 15 countries (from independence to 2004).

We match the birthplace information to administrative boundaries on the first subnational level (ADM1) according to the GADM database of Global Administrative Areas.² Based on this data, we build a panel dataset of ADM1 regions, indicating which area is the birth region of which cabinet member in a given year. We combine our new birthplace data with the geocoded neonatal and infant health data from the DHS. We thus know for every child of all interviewed women whether or not it was born while a minister originated from the same ADM1 region.

In our analysis, we distinguish between the country leader, key ministers (such as economics and finance ministers), health ministers, and other cabinet members. We can thus investigate whether being in charge of the relevant portfolio (in this case, health) increases the likelihood that a cabinet member can influence policy outcomes.

We find that children born in the same region as the current health minister are less likely to die before reaching one month or one year of life than their siblings born in other years: infant and neonatal mortality rates are 3.2 and 4.9 live births lower, respectively, in health ministers' birth regions. Hence, we provide evidence that health ministers influence health outcomes in their birth regions. We also find a negative effect for key ministers and leaders, but it is smaller than the health ministers' effect, and only the key ministers' effect on infant mortality is statistically significant. We do not find consistent patterns for other cabinet members.

We believe that a causal interpretation of our findings is warranted. First, given our mother fixed effects strategy, we effectively compare siblings. Hence, there is likely little room for confounders such as regional or socio-demographic characteristics. We also include country-year fixed effects to account for shocks and trends affecting the whole country (such as economic downturns). Second, we show that children born in years right before their birth region sends a (health) minister are not less likely to die. This result suggests the appointment of (health) ministers influences health outcomes, and not vice versa.

Our results are robust to a multitude of robustness checks. Among other, our checks cover different definitions of health ministers (e.g., we include or exclude public health ministers or sanitation ministers in our health minister definition) and different definitions of ministers in general (e.g., we include or exclude ministers of state or vice ministers as cabinet members).

In a next step, we investigate possible mechanisms for the health ministers' effect on mortality rates. First, we provide causal evidence that children born in the birth region of last year's health minister are more

²ADM1 regions correspond to provinces in many countries.

likely to be delivered in a health facility and/or in the presence of a professional birth attendant. In line with evidence from the public health literature, we show that children born in a health facility and/or in the presence of a professional are less likely to die. Taken together, these results suggest that increased access to healthcare at birth is a relevant channel for health ministers' influence.

Arguably, increased healthcare access requires increased health-related funding. Therefore, we analyze whether lower mortality in health ministers' birth regions can be attributed to higher health aid flows.³ We combine our new data on the ministers' birthplaces with geocoded data on health-related financial flows from the World Bank, which is available from AidData. We thus capture if and how much health aid flows to an ADM1 area in a given year. Our results suggest that more health-related aid flows to health and key ministers' birth regions, hence providing evidence for an additional channel through which ministers can favor their regions. We do not find descriptive evidence that World Bank health aid explains the mortality-lowering effects of health ministers, but we find evidence consistent with this mechanism for key ministers. Our findings that health ministers' have an effect on health outcomes but not (strongly so) through aid, hint at health ministers also channeling resources other than World Bank health aid to their region.

In sum, our work warrants the conclusion that not only do country leaders but so do cabinet members engage in favoritism. We document that health and key ministers can directly lower mortality, increase access to health care, and allocate more health aid to their regions. A growing body of literature emphasizes that the circumstances in a child's early life can have a lasting impact on health and human capital formation (Almond and Currie, 2011; Adhvaryu et al., 2019). Thus, increased access to health care and increased aid flows for children born while a minister originates from their region likely have implications beyond reducing mortality rates in early life.

Our work contributes to various strands of literature. By showing that cabinet members engage in favoritism, our findings support research by Francois et al. (2015), who find that power may be shared more evenly in Africa than the "big man theory" suggests. Specifically, by showing that cabinet members can influence the allocation of funds and health outcomes, we add to this debate by highlighting that such power-sharing is more than symbolic and translates into actual policy outcomes.

Moreover, we contribute to the broader literature on ethnic and regional favoritism. To the best of our knowledge, one other contribution investigates favoritism and health outcomes in a cross-country setting, namely Franck and Rainer (2012). They report lower infant mortality (and higher primary school attendance)

 $^{^{3}}$ We focus on foreign aid flows as cross-country data on the subnational allocation of public funds in Africa is not available.

among co-ethnics of the country leader in 18 African countries. In the public health and medical literature, several case studies draw attention to the within-country political and social determinants of health-related outcomes.⁴ We extend previous work on health-related favoritism in several respects: First, our work covers 36 African countries and includes cabinet members other than country leaders. Second, we consider various components of health-related favoritism (i.e., mortality, access to healthcare, and aid).⁵

Regarding other policy outcomes, there is evidence that nighttime light is more intense in regions when they are the birth region (Hodler and Raschky, 2014) or the ethnic homeland of the current country leader and even in regions inhabited by linguistically similar groups (De Luca et al., 2018; Dickens, 2018). In Kenya, districts inhabited by the ethnic kin of the current president receive more road-building expenditure during periods of higher autocracy (Burgess et al., 2015), and co-ethnics of the current president and education minister acquire more schooling (Kramon and Posner, 2016). There is also evidence of favoritism in the allocation of foreign aid. Dreher et al. (2019) show that more aid flows from China to the birth regions of the current country leader, but they do not find a similar effect for aggregate World Bank aid. Bommer et al. (2019) find evidence that after disasters, US aid is directed primarily to the birth region of the head of state. Maffioli (2020) finds that during the Ebola outbreak in Liberia in 2014, resources were misallocated towards electoral swing villages but not towards villages inhabited by the president's ethnicity.⁶ We complement the literature on favoritism in aid allocation and the broader literature on ethnic and regional favoritism by considering cabinet members with large-scale coverage.

The remainder of the paper is structured as follows. In the next section, we describe our data. In Section 3, we detail the estimation strategy. The results follow in Section 4. In Section 5, we discuss potential mechanisms, before concluding in Section 6.

2 Data and Data Processing

We present a dataset on the birthplaces of cabinet members in 36 African countries between 2001 and 2015. We include all African countries with more than 1 million inhabitants for which Demographic and Health

⁴See Montoya-Williams and Fuentes-Afflick (2019), Stephens et al. (2017), Neerup Handlos et al. (2016), or Rahmani and Brekke (2013). The question of favoritism concerning access to healthcare also receives widespread attention in public debates (e.g., Transparency International, 2019).

⁵In contrast to Franck and Rainer, we focus on regional instead of ethnic favoritism.

 $^{^{6}}$ More generally, there is a substantial body of work on political motives affecting the allocation of aid. This strand of the literature starts with Alesina and Dollar (2000) and has since then been extended, both for across- and within-country allocation.

Surveys (ICF, 2001-2015) data are available. Appendix A lists all countries in our sample.⁷ We then combine our birthplace data with the individual-level data provided by the DHS to build a panel dataset with children as the units of observation.

In our study of mechanisms through which favoritism could operate, we additionally combine our birthplace data with georeferenced data on World Bank aid projects from AidData (2017) and construct two different datasets: a panel dataset with ADM1 regions as units of observation and a panel dataset with individual ministers as units of observation.

In the following, we present our data in more detail. Our observation period covers 2001 through to 2015.⁸

2.1 Data Collection: Birthplaces of Cabinet Members

Data on cabinet members at a given point in time comes from the World Factbook. For almost all countries, the World Factbook provides monthly lists of all cabinet members in pdf format, indicating their name and their designation, since 2001. Based on this data, we build a year-level panel of all cabinet members and their designations. We only include cabinet members if they held office for six months or more. To construct the panel, we algorithmically parse the monthly World Factbook files. We then employ string-matching algorithms and conduct some manual cleaning to identify duplicate names.⁹ We are left with 4,603 unique individuals.¹⁰

We tag each designation according to two dimensions: first, subject matter (*health*, *economics*, etc.) and, second, ministerial status (*minister* and *other*). First, for the subject matter tag, we apply a mix of automated keyword searches and manual checks to map the cabinet members' designation string variables from the World Factbook (which vary across and within countries over time) to a set of designation indicators (like *health*, *economics*, *trade*, etc.).¹¹ Often, cabinet members are assigned to more than one indicator in a

⁷Our replication data contains birthplace information for all African countries with more than 1 million inhabitants, also for those without DHS information. Appendix A also lists these 11 additional countries. In sum, our birthplace information is available for 47 countries. The coverage statistics presented in this Section remain almost unchanged when calculated for all 47 countries instead of only 36.

⁸The observation period is given by the CIA World Factbook (2018). The Factbook has been providing lists of all cabinet members and their designations since 2001. As of our data collection, the lists were available up to and including 2015.

⁹Inconsistencies in the spelling of an individual's name over time appear very often, to the extent that some individuals appear with a handful of different spellings.

¹⁰Without the six-month threshold that we apply, we would be left with 4,966 individuals.

¹¹For example, to tag ministers as health ministers, we first filter out all designations containing the string "health" and then confirm for each match if it is a health minister. Our procedure is designed such that each unique designation meets the human eye at least once. This is to ensure that designations that would not be filtered out by the exact keyword search could be assigned manually. For example, the misspelling of "minister of health" as "minister of healt" would not remain unnoticed.

given year:¹² Many usual designations map to more than one indicator (such as *Minister of Economics and Development* mapping to *economics* and *development*), and cabinet members sometimes hold more than one position at a time. As an example, the Malawian cabinet member Khumbo Hastings Kachali is coded as both Vice-President and Health Minister in 2012-2013. Second, for ministerial status, we distinguish between ministers in the narrow sense (*minister*) and other cabinet members, like vice-ministers (*other*). Our example, Khumbo Hastings Kachali, is hence tagged as *health_minister* and *president_other* in 2012-2013. Note that the relationship between the *minister* and *other* extension is not necessarily ordinal: For instance, depending on the context, a minister of state may be more or less powerful than a minister. For this reason, we do not exploit the distinction between *minister* and *other* in our main analysis (but we do in robustness checks).

Given that it is not obvious from the World Factbook which cabinet position represents the effective leader in a given country and year, we also use the Archigos database by Goemans et al. (2009). For our sample, the president of a country is its effective leader in most cases: over 90% of the cabinet member-year observations that we tag as presidents based on the World Factbook are tagged as heads of state based on Archigos.

We manually search for birthplace information for each cabinet member. We use a variety of (especially online) resources in various languages. A frequent source is newspaper articles. The vast majority of our sources are in English, French, and Arabic. To ensure the quality of the data collection process, the information gathered by one data collector is reviewed by at least one other collector. In the interest of coverage, we restrict our precision to the first subnational administrative level (ADM1) using the GADM database of Global Administrative Areas (2018). For many ministers, birthplace information is challenging to find. We are able to cover the birthplaces of 76% of all cabinet members in our sample (3,507 out of 4,603). If we identify a member's birthplace, but the member is foreign-born, we reset the birthplace information to missing (2% of cabinet members). The coverage rates for all countries are shown in Figure C.1 in Appendix B. Concerning health ministers, the coverage is 73% (97 out of 132). Figure 1 shows the ADM1 regions that were the birth region of a health minister at least once in our sample period. There are 92 changes in health ministers', 37 in country leaders', 280 in key ministers', and 332 in other cabinet positions' birth regions.

¹²This applies to around one-third of all cabinet member-years.



Notes: ADM1 regions (dark) in the 36 countries in our sample that were the birth region of a health minister at least once, 2001-2015.

Figure 1: Birthplaces (ADM1) of health ministers, 2001-2015

2.2 Construction of Birth Panel Dataset

With the information on the cabinet members' birth regions and the location of individuals interviewed by the DHS, we build a panel data set with children as the units of observation. For the 36 countries we study, the data is taken from 94 surveys.¹³ The DHS provides information on an interviewed mother's children. For children that are no longer alive, the age at death is indicated. Hence, we can construct indicator variables

¹³See Appendix A for a list of countries and the number of available DHS rounds.

for whether a child k of mother p in region i in year t died before it was one month old (*neonatal*_{kpict}) and whether it died before it was one year old (*infant*_{kpict}). To compare siblings who were born while the (health) minister originated from their region to those born in other years, we only keep mothers with at least two live births. This corresponds to 84 percent of the children in the DHS data. There are 1,273,609 children from 420,408 mothers for which we have information on neonatal mortality and 1,125,295 children from 377,554 mothers for which we have information on infant mortality. For ease of interpretation, we scale the indicator variables for mortality by 1,000. Hence, neonatal_{kpict} (infant_{kpict}) is interpreted as the number of children dying in their first month (year) of life by 1,000 live births. The summary statistics in Table 1 show that average neonatal (infant) mortality in our sample is 32.7 (70.1) deaths per 1,000 live births. Table 1 also shows summary statistics for variables capturing access to health services. Specifically, we construct an indicator variable for whether a child was born in a health facility (public and private hospitals, clinics, or health posts) and and indicator for whether a professional birth attendant (doctor, professional nurse, or professional midwife) was present. These variables are also available from the DHS, however, they are only available for the children born in the last three or five years before the interview (depending on the survey), hence reducing our sample. They are used to study potential mechanisms in Section 5. To make coefficients for different outcomes more easily comparable, we also scale these variables by 1,000 live births. As shown in Table 1, on average, 518.6 out of 1,000 live births occur at a health facility. Similarly, 520.7 out of 1,000 births are accompanied by a professional birth attendant.¹⁴ In the last two rows, Table 1 shows the amount of World Bank health aid allocated to a child's birth region (at the ADM1 level) in their birth year and an indicator for whether any World Bank health aid was allocated to the child's birth region (these variables are also used to investigate the mechanisms in Section 5). On average, 15.4 percent of children are born in a region that receives World Bank health aid in the same year. A child's birth region receives an average of 2 million USD. 15

We include the following control variables in the specifications investigating mortality rates: the gender of the child and indicator variables for whether the birth was the mother's first child, second child, etc. Moreover, we use a multiple birth indicator (twins, triplets, etc.). In case of multiple births, all children are considered unique observations. Table 1 provides summary statistics for these control variables.

Combining the information on the ministers' designations and their birthplaces, we build an indicator

¹⁴The correlation between these two measures is high in our DHS data (84.2%).

¹⁵We obtain geo-referenced data on World Bank health aid projects from AidData. For details on the construction of the aid variables, see Appendix E.2.

variable for whether a health minister in power in year *t* was born in region *i* in country *c*, *healthmin_{ict}*. Additionally, we construct similar indicator variables for the birthplaces of the head of state (*leader_{ict}*), key ministers (*keymin_{ict}*), and any cabinet members (*cabinet_{ict}*). Key ministers refer to top cabinet positions along the lines of Francois et al. (2015) and include the (vice-) president, the (vice-) prime minister, as well as the ministers for economics, finance, development, industry/trade, agriculture, justice, and foreign affairs. The detailed two-dimensional tagging of the cabinet members' designations, as described in Section 2.1, allows us to easily adjust the exact definitions of *healthmin_{ict}*, *keymin_{ict}*, and *cabinet_{ict}*. Several designation indicators are related to health: *health*, *public health*, *HIV*/*AIDS*, *sanitation*, *population*. In most analyses, *healthmin_{ict}* refers only to cabinet members with the cabinet indicator *health*, excluding *public health*, *HIV*/*AIDS*, *sanitation*, also, e.g., vice-ministers or ministers of state. In robustness tests, we only include ministers in the strict sense, excluding vice-ministers, etc. As Table 1 shows, 5.5% of children are born in a current health region.

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
neonatal _{kpict}	1,273,609	32.672	177.776	0	1,000
<i>infant_{kpict}</i>	1,125,295	70.091	255.300	0	1,000
$facility_{kpict}$	407,040	518.561	499.656	0	1,000
birthattendant _{kpict}	415,189	520.654	499.574	0	1,000
gender _{kpict}	1,273,609	0.507	0.500	0	1
birthorder _{kpict}	1,273,609	3.726	2.319	1	18
multiplebirth _{kpict}	1,273,609	0.042	0.200	0	1
healthmin _{ict}	1,273,609	0.055	0.228	0	1
leader _{ict}	1,273,609	0.075	0.264	0	1
keymin _{ict}	1,273,609	0.405	0.491	0	1
<i>cabinet_{ict}</i>	1,273,609	0.657	0.475	0	1
$healthaidD_{ict}$	1,157,120	0.155	0.362	0	1
<i>healthaid_{ict}</i> (in billion USD)	1,157,120	0.002	0.014	0.000	0.420

Table 1: Summary statistics

Notes: This table provides summary statistics on the variables used in the main tables. All variables are explained in the text.

3 Econometric Framework

We use a linear probability model to investigate whether neonates and infants are less likely to die when they are born in the same region as last year's health minister.

For the child k of mother p who is born in region i, country c, and year t, we estimate the following equation:

$$mortality_{kpict} = \phi_p + \beta_{ct} + \gamma \ healthmin_{ict} + \Omega_{control_{kpict}} + \vartheta_{kpict} \tag{1}$$

*mortality*_{kpict} is either *neonatal*_{kpict} or *infant*_{kpict}. General trends affecting a country as a whole will be absorbed by the country-year fixed effects, β_{ct} . *control*_{kpict} is a vector of child-level controls, as discussed in Section 2.2. ϕ_p are mother fixed effects (following Kotsadam et al., 2018, and Bruederle and Hodler, 2019). These fixed effects control for everything that should remain roughly constant over time for the same mother, such as the mother's education and religion, and all time-invariant location- and region-specific characteristics, such as whether they live in a rural or urban area. With this approach, we effectively compare siblings who were born either when a region-born health minister was in power or in a year where the health minister originates from a different region. In our context, families with more than one child are anything but a special case. In the DHS data, 84% of all children have a sibling.

Nevertheless, we address some remaining potential concern regarding the causal interpretation of γ : It could be that subnational development progress leads to a subnational area becoming a ministerial birth region and experiencing lower mortality rates. We address this concern by applying a placebo test: We study whether mortality decreases in the years before the health minister from this region is in power, which is not the case (results shown in the next section). We thus conclude that a causal interpretation of our estimates is warranted.¹⁶

In most specifications, we control for whether the head of state, $leader_{ict}$, a key minister (excluding the head of state), $keymin_{ict}$, or any cabinet member, $cabinet_{ict}$, originates from region *i*.

¹⁶Another concern could be that natural disasters might lead to changes in neonatal and infant mortality and, at the same time, to a health minister from this region being installed (e.g., to gather knowledge that allows the government to restore the region faster or to prevent unrest through representation of the affected population). We find no evidence that natural disasters lead to changes in the health ministry in the subsequent year, nor in the country leader, the key ministers, or any cabinet members. In Table C.1 in Appendix C, we show country-level regressions of an indicator for a change in the health minister (column 1), the country leader (column 2), key ministers (column 3), and any cabinet members (column 4) on an indicator for a natural disaster in the year prior. The coefficients are negative for health ministers and key ministers, and none of them are significant. The data on disasters is taken from the EM-DAT database (Guha-Sapir, n.d.).

 $mortality_{kpict} = \phi_p + \beta_{ct} + \gamma \ healthmin_{ict} + \delta \ leader_{ict} + \eta \ keymin_{ict} + \psi \ cabinet_{ict} + \Omega control_{kpict} + \vartheta_{kpict}$ (2)

A priori, the sign of γ in equations 1 or 2 could go in any direction. A zero effect (conditional on the controls) would occur if the birthplace of the health minister played no role in mortality outcomes. A negative effect could point to ministers discriminating against their birth regions, as it might be easier for them to control their base at home than in other regions, providing an incentive to extract more resources from their home region, whose support they can more easily garner (see, e.g., Kasara, 2007). A positive effect is consistent with political favoritism by ministers towards their birth region.¹⁷

We use robust standard errors clustered at the country-year and ADM1 level. In robustness checks, we also correct the standard errors for spatial correlation.

4 Main Results

Table 2 shows the effect of health ministers on neonatal and infant mortality in their birth region. Columns 1 and 2 provide results for neonatal mortality, and columns 3 and 4 for infant mortality. Columns 1 and 3 show that children born in the same region as the health minister from the previous year are less likely to die before their first month or their first year of life compared to their siblings born in other years: Neonatal and infant mortality decrease by 3.5 and 5.0 deaths per 1,000 live births, respectively. These effects correspond to 11 (7) percent of the average neonatal (infant) mortality in our sample (or 0.02 standard deviations). Hence, being born in the same region as last year's health minister seems to translate into better health outcomes. The effects remain almost unchanged (both in terms of size and significance) when we control for whether the leader, a key minister, or any other cabinet member originates from the same region as the child in columns 2 and 4. We also find that key ministers have a statistically significant lowering effect on infant mortality (see column 4): Infant mortality decreases by 2.6 deaths per 1,000 live births in their birth regions.

¹⁷De Chaisemartin and D'Haultfoeuille (2020) demonstrate that linear regressions with period and group fixed effects estimate weighted sums of the average treatment effects (ATE) in each group and period. Thereby, weights can be negative. Negative weights can lead to a reversal of the coefficient sign (e.g., the linear regression coefficient could be negative while all the ATEs are positive). In our case, the share of negative weights (estimated based on De Chaisemartin et al., 2019) is low: 10% in our main specifications. Accordingly, we find that our estimates signs could only be reversed when we suppose that treatment effects of groups and time periods could be implausibly high (e.g., the health minister coefficient could capture three-quarters of all mortality). We thus proceed with the standard fixed effects estimator.

In Appendix Table D.1, we replicate columns 2 and 4 but look at different key ministers separately. We split the key minister indicator into separate indicators for President/Prime Minister other than the country leader, economics/finance/development, industry/trade, agriculture, justice, and foreign affairs minister. The key ministers' effects seem to concentrate on ministers of agriculture and justice. For leaders, we find negative coefficients for neonatal and infant mortality (see Table 2, columns 2 and 4). They are smaller and with larger standard errors relative to the health ministers' coefficients. For other cabinet members, we do not find statistically significant effects.

	(1)	(2)	(3)	(4)
	$neonatal_{kpict}$	$neonatal_{kpict}$	in f ant _{kpict}	in f ant _{kpict}
healthmin _{ic,t-1}	-3.539***	-3.528***	-5.045***	-5.065***
	(1.105)	(1.113)	(1.863)	(1.852)
$leader_{ic,t-1}$		-1.681		-0.557
		(1.441)		(2.844)
<i>keymin_{ic,t-1}</i>		-0.710		-2.615***
- ,		(0.555)		(0.893)
$cabinet_{ic,t-1}$		0.297		0.670
,		(0.586)		(0.864)
Obs.	1,141,752	1,141,752	1,001,268	1,001,268
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table 2: Health ministers and mortality in the subsequent year

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator of whether a child died before reaching the age of one month (*neonatal_{kpict}*; columns 1 and 2) and one year (*infant_{kpict}*; columns 3 and 4), scaled by 1,000. The independent variables are indicators of whether the child was born in the same region as last year's health minister (*healthmin_{ic,t-1}*), country leader (*leader_{ic,t-1}*), key minister (*keymin_{ic,t-1}*), or any cabinet member (*cabinet_{ic,t-1}*). *healthmin_{ic,t-1}* includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. *leader_{ic,t-1}* includes the effective head of state. *keymin_{ic,t-1}* includes key ministers in the sense of Francois et al. (2015), excluding the head of state. All ministers in the broad sense are included, e.g., also vice ministers and ministers of state. Child-level controls are gender, indicator variables for birth order, and for multiple births. All specifications include mother and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively.

Dynamics and placebo test Figure 2 shows the dynamics of the (health) ministers' effects on mortality (for neonatal mortality in Figure 2a and for infant mortality in Figure 2b). First, as a placebo test, we examine if neonates or infants are less likely to die before a region-born cabinet member is in power. To this end, we replicate columns 2 (Figure 2a) and 4 (Figure 2b) of Table 2, but replace *healthmin*_{*ic,t*-1} by an indicator

for children born *before* a health minister comes from this region. Specifically, we look at *healthmin*_{*ic,t*+2} (indicator for children born four years before a health minister comes from this region) to *healthmin*_{*ic,t*+2} (children born two years before). Note that *healthmin*_{*ic,t*+1} (children born one year before) can only in part be regarded as a placebo due to our data collection procedure (we only code health ministers as such if, for a given year, they are in office for more than six months). Accordingly, the first three points of both subfigures represent the coefficients for a placebo health minister. We find that children are not less likely to die before a region-born health minister is in power, which strengthens our causal interpretation. From the fifth point onward, both subfigures show the dynamics after the appointment of a region-born health minister. Again, we re-run columns 2 and 4 from Table 2 but this time focusing on children born after the region-born health minister is in office. To this end, we replace our main regressor *healthmin*_{*ic,t*-1} by indicators with different lags (we consider lags up to four years, that is, up to *healthmin*_{*ic,t*-4}). The mortality-reducing effect of health ministers fade quickly.¹⁸



Notes: Each point and corresponding 95% confidence interval represent a health minister indicator coefficient from a separate regression. Subfigure a (b) replicates column 2 (4) of Table 2. We replace the health minister indicator by indicators for the years before they are in office and with indicators of whether the previous health minister was born in the region. The indicators for four to two years before they are in office can be interpreted as placebo tests. The year before they are in office can only partly be interpreted as a placebo test due to our data coding procedure: Some health ministers might already be in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.

Figure 2: Health ministers' effects on mortality before and after they enter office.

Robustness In Appendix D, we reproduce Table 2 with various modifications. In Table D.2, we include

¹⁸Appendix Figures D.2 to D.4 show the dynamics for leaders, key ministers, and other cabinet members.

more child-level controls. In Table D.3, we use a broader definition of health ministers, while we only include ministers in the narrow sense in Table D.4 (see Section 2.1 for details on these alternative definitions). Table D.5 additionally includes a control for the population at the regional level (ADM1). In some surveys, the respondents were asked how long they had lived in their current place of residence. We can thus further validate the robustness of our results by restricting our sample to children who were born after their mother moved to a given region. We replicate Table 2 using this smaller sample in Table D.6. We thereby seek to further strengthen the quasi-experimental nature of our analysis (to avoid confounding factors due to mothers entering or leaving a given region in response to political representation). Note that the question since when a mother had lived in their current place of residence is only asked in very few DHS surveys (we thus lose around 70% of our observations). Hence, we use the full sample in our main analysis. With all robustness checks, our results remain qualitatively the same. Tables D.7 and D.8 additionally show that the standard errors are somewhat larger when adjusting them for spatial correlations of up to 100km and 500km, respectively, but the results remain statistically significant at least at the 5% level.¹⁹

Effect heterogeneity In Table D.9, we show that health ministers likely have a stronger influence on mortality rates early on in their term. In columns 1 to 4, we interact the health minister variable with his or her tenure. Columns 1 and 2 show results for neonatal mortality, and columns 3 and 4 for infant mortality. The health minister coefficient is significantly negative. The interaction coefficient is positive (but only statistically significant in columns 1 and 2). Hence, if anything, the health minister's impact on mortality decreases over time. In columns 5 to 8, we conduct a similar exercise. Instead of tenure, we use an indicator for the first year of the (health) minister's term. The coefficients of the interaction between the health minister and the first-year indicators are negative in all four columns and statistically significant for neonatal mortality. While the health minister's main effect remains negative, it is no longer significant for neonatal mortality. Hence, health ministers seem to reduce at least neonatal mortality primarily in their first year. Combined with the analysis of the dynamics (see Figure 2), these findings suggest that not only does a health minister's effect on mortality fade quickly after they are in power, it seems to already erode during their term. For other designations (leaders, key ministers, other cabinet members), we do not find any clear tenure-related patterns.

In Table D.10 in Appendix D, we distinguish between election years (legislative or executive) and other years. We replicate Table 2 but add an interaction between the health minister indicator and an indicator

¹⁹To compute these standard errors, we rely on the Stata package *acreg* introduced by Colella et al. (2019).

for any elections of the legislative (column 1 to 4) or the executive (column 5 to 8) in the following year and interaction terms with the country leader, the key ministers, and any cabinet members and the respective elections in even columns. We do not find election-related patterns for any of the designations.

In Table D.11, we show that the health minister's effect is, if anything, lower in democracies. Specifically, we interact the minister variables with an indicator of whether a country was democratic in a given year based on the Polity2 score provided by the Polity IV Project (Marshall et al., 2019). The Polity2 Score measures regime authority on a spectrum ranging from -10 (corresponding to a hereditary monarchy) to +10 (consolidated democracy). As suggested by the Polity IV Project, we construct an indicator variable for whether a country is democratic in a given year; it is equal to one for scores between +6 and +10.

In Table D.12, we interact the minister variables with an indicator of whether the mother has any education (primary, secondary, or tertiary). The interaction term of education with the health minister is positive in all columns but not significant. Hence, if anything, the health minister's influence on neonatal mortality appears to be lower for children of more educated mothers. Neonatal neonatal and infant mortality are lower among children of mothers with any education (30.2 versus 35.5 per 1,000 live births for neonatal and 63.5 versus 77.3 for infant mortality), suggesting that the health minister provides a catch-up effect.

5 Mechanisms

In the previous section, we presented robust evidence that being born in a health minister's, and to a lower degree, a key minister's birth region is associated with better survival prospects for neonates and infants. In this section, we investigate possible mechanisms.

5.1 Health ministers' effect on access to health services

First, we ask whether lower mortality might be due to increased access to healthcare services at birth, as measured (i) by an indicator for mothers giving birth at a health facility (public or private hospital, clinic, or health post) and (ii) an indicator for the presence of a birth attendant (doctors, nurses, midwives, or trained birth attendants).²⁰ We focus on access to health services at birth due to its definitive timing: These two

 $^{^{20}}$ We use the notion of "increased access" to healthcare services broadly. It could entail lower costs in using existing or new services. It could, however, also mean increased utilization when (monetary) costs remain the same (such as people increasing their use due to having more trust in infrastructure or services).

outcomes reflect health care services provided at the very beginning of a child's life.²¹ In this subsection, we will have fewer observations than in the main analysis because the healthcare access variables are only available for the children born in the last three or five years before the interviews were conducted (depending on the survey).

To understand the role increased access to healthcare might play in lowering mortality, we mirror our main results (see Table 2) in Table 3: Instead of mortality, we use indicators for whether the child was delivered in a health facility and whether the birth was attended by a professional as outcomes. As we rely on the same research design as in our main table, we interpret these estimates causally.

	(1)	(2)	(3)	(4)
	$facility_{kpict}$	$facility_{kpict}$	att end ant _{kpict}	$attendant_{kpict}$
healthmin _{ic,t-1}	10.240**	10.189**	10.457***	10.377***
	(4.042)	(4.031)	(3.889)	(3.874)
<i>leader</i> _{<i>ic</i>,<i>t</i>-1}		-0.214		-3.696
,		(10.004)		(10.418)
<i>keymin_{ic,t-1}</i>		2.432		3.965*
		(2.089)		(2.166)
$cabinet_{ic,t-1}$		-2.678		-3.410
		(2.596)		(2.357)
Obs.	380,188	380,188	387,692	387,692
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table 3: Health ministers and other health outcomes in the subsequent year

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator of whether a child was born in a health facility ($facility_{kpict}$; columns 1 and 2) and in the presence of a professional birth attendant ($attendant_{kpict}$; columns 3 and 4), scaled by 1,000. The independent variables are indicators of whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), country leader ($leader_{ic,t-1}$), key minister ($keymin_{ic,t-1}$), or any cabinet member ($cabinet_{ic,t-1}$). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. $leader_{ic,t-1}$ includes the effective head of state. $keymin_{ic,t-1}$ includes key ministers in the sense of Francois et al. (2015), excluding the head of state. All ministers in the broad sense are included, e.g., also vice ministers and ministers of state. Child-level controls are gender, indicator variables for birth order, and for multiple births. All specifications include mother and country-year level. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively.

 $^{^{21}}$ We also considered vaccinations. However, they suffer from timing problems because different degrees of immunization might occur at various points in time. One vaccination that is available in the DHS comes with unambiguous timing: the Bacillus Calmette–Guerin (BCG) birth-dose against tuberculosis (recommended in places with high tuberculosis incidence and/or high leprosy burden, see World Health Organization, 2020). However, relatively few observations are available. If anything, health ministers are associated with higher vaccination rates (results not shown; available upon request).

Children born in the same region as last year's health minister are more likely to be born in a health facility relative to their siblings (see columns 1 and 2 in Table 3): facility-based births increase by around 10 births per 1,000 live births. Columns 3 and 4 show that the presence of a professional birth attendant is more likely for children born in the health minister's birth region (the size of the effect is similar to facility-based births). For key ministers, we find a positive effect, which is significant for births attended by a professional (and of around half the size of the health minister's effect).

In sum, Table 3 highlights that mothers are more likely to give birth in facilities and with professionals present when they live in the health minister's birth region, providing another piece of evidence for ministers' favoritism. There is evidence (experimental and other) that mortality is lower among children born in a health facility or in the presence of a skilled birth attendant (e.g., Darmstadt et al., 2005; Tura et al., 2013; Bhutta et al., 2014; Tanner et al., 2016). We also uncover this association in our data (see Table E.1 in Appendix E.1): Facility-based births correlate with 6.4 (14.9) fewer neonatal (infant) deaths per 1,000 births. Similarly, births attended by a professional are associated with 7.1 (8.4) fewer neonatal (infant) deaths per 1,000 births.²²

In Appendix Table E.2, we look at antenatal and postnatal care, as there is evidence that antenatal and postnatal care can reduce mortality (e.g., Darmstadt et al., 2005; Bhutta et al., 2014; Tekelab et al., 2019). Unfortunately, these variables are only available for very few observations. We find positive but largely insignificant effects for all minister birthplace indicators. At least in terms of the coefficient sign, these findings are again consistent with our interpretation that access to healthcare matters.

Dynamics and placebo test By replicating Figure 2 with health care access instead of mortality as outcomes, Figure E.1 shows the dynamics of the (health) ministers' effects on facility-based births (Figure E.1a) and births attended by a professional (Figure E.1b). Facility-based births and births with professional attendants are not more likely before a region-born health minister is in power, which supports our causal interpretation.²³

 $^{^{22}}$ To obtain these estimates, we regress mortality on facility-based births and births attended by a professional using our usual child-level specification with mother fixed effects. Note that these estimates reflect correlations and not causal effects because unobserved child-specific variables could influence both mortality and the use of healthcare services. For example, medical complications might lead a mother who would otherwise not go to a facility to do so in this case.

 $^{^{23}}$ As for mortality, the health minister's effect on healthcare access peaks for *healthmin_{ic,t}* and *healthmin_{ic,t-1}*. For facility-based births, the effect remains relatively stable (even for *healthmin_{ic,t-4}*). For births attended by a professional, the effect remains less stable over time (it starts to decline and loses its significance for *healthmin_{ic,t-2}*). In sum, there seems to be some stickiness to health minister's influence on healthcare access. Note that the healthcare access variables are available for fewer children in the DHS data than the mortality variables. When we study the dynamics of health ministers' mortality-lowering effect but only focus on the observations for which the healthcare access variables are also available, we still find that the the effect peaks for *healthmin_{ic,t-1}*, but it fades less quickly in this reduced sample. Appendix Figures E.2 to E.4 in Appendix E.1 show the dynamics for leaders, key ministers, and other cabinet members.

Effect heterogeneity In Table E.3 in Appendix E.1, we show that uneducated mothers, in particular, seem to profit from increased access to healthcare during the term of a region-born health minister. At the same time, in our sample, children of mothers with any education are more likely to be born in a health facility (648.7 versus 374.2 of 1,000 live births) and/or in the presence of a professional birth attendant (639.2 versus 387.3), suggesting a catch-up effect of the health minister. Recall that we found some weak evidence for the same pattern when looking at mortality in our main analysis, see Appendix Table D.12.

5.2 Health ministers' effect on aid allocation

In the previous section, we argued that health ministers' lowering effect on neonatal and infant mortality is – at least in part – driven by increased access to healthcare. Likely, increased access to healthcare goes hand-in-hand with increased funding of some sort.²⁴ To our knowledge, cross-country data on the subnational allocation of public funds in Africa is not available. However, AidData provides geo-referenced data on health-related World Bank aid projects.

First, we analyze whether more World Bank health aid is available in a child's birth region when a regionborn (health) minister is in power. As in our main analysis, the observation units are children. We study the extensive margin as well as health aid amounts. For the extensive margin, we replicate the linear probability model from Equation 2 but with an indicator for whether the child's home region received any health aid in a given year, *healthaidD_{ict}* (instead of neonatal and infant mortality), as the outcome. The construction of the aid variables is described in Appendix E.2. For the amounts, we assume a log-linear relationship between health aid allocated to an ADM1 region, *healthaid_{ict}*, and the ministerial birth region indicators.

$$ln(healthaid_{ict}) = \phi_p + \beta_{ct} + \gamma healthmin_{ict} + \delta \ leader_{ict} + \eta \ keymin_{ict} + \psi \ cabinet_{ict} + \Omega control_{kpict} + \vartheta_{kpict}$$
(3)

As before, ϕ_p represents mother fixed effects and β_{ct} country-year fixed effects. Of primary interest is the coefficient γ , the effect of a child being born when the health minister originates from the same region *i*. Again, we also estimate the same coefficients for heads of state (δ), key ministers (η), or any cabinet member (ψ). Many regions do not receive aid in a given year: *healthaid_{ict}* is often zero. We, therefore, do not estimate the log-linear relationship in Equation 3 directly but rely on its exponential form by running a

 $^{^{24}}$ An exception is if health ministers' effect on healthcare access only operates through signaling, i.e., when mothers trust (existing, under-utilized) healthcare facilities and services more when a region-born health is in power.

Poisson pseudo maximum likelihood (PPML) regression:²⁵

$$healthaid_{ict} = exp[\phi_p + \beta_{ct} + \gamma healthmin_{ict} + \delta leader_{ict} + \eta keymin_{ict} + \psi cabinet_{ict}] + \varepsilon_{kpict}$$
(4)

We interpret the coefficients from Equation 4 as semi-elasticities. We interpret γ , δ , η , and ψ as causal estimates (the same considerations as for the ministers' effect on mortality apply). If health (key) ministers can channel more resources to their birth region, we expect a positive sign for $\gamma(\eta)$. We use robust standard errors clustered at the ADM1 and the country-year level. For data availability reasons, our World Bank health aid analysis covers 2001-2014 (while the main analysis covers 2001-2015).

Figure 3 shows the results. Each point and corresponding 95% confidence interval represent a health minister (Figure 3a) or key minister (Figure 3b) indicator coefficient from a separate regression. In Figure 3a, we run Equation 4 and vary the timing of the health minister indicator from four years before they are in office to lags of up to four years (along the lines of Figure 2 for mortality). In Figure 3b, we also run Equation 4 but vary the timing of the key minister variable.²⁶

For health ministers, we find positive coefficients after they enter office. In the years before they are in office, we do not find consistent patterns and no significant effects, again strengthening our causal interpretation. The coefficients for lags from zero (the minister's effect on allocation in the same year) to two (their effect on allocation in two years) are of similar size. The effect of a lag of two – an increase in health aid of 70% – is close to statistically significant (with a p-value of 0.106).²⁷ We conclude that health ministers may have some leeway to direct health aid resources to their own birth region. If World Bank health aid reduces neonatal and infant mortality (as previous contributions suggest),²⁸ it could be a plausible channel for the mortality-lowering effects of health ministers.

²⁵To estimate equation 4, we use the Stata package *ppmlhdfe*, which implements PPML estimation with multiple high-dimensional fixed effects and allows for multiway-clustering (Correia et al., 2019a). Santos Silva and Tenreyro (2006) show that PPML is superior to simple OLS and Tobit approaches with heteroskedasticity and many zero observations in the data. As Gourieroux et al. (1984) show, for the Poisson regression estimator to be consistent, we only need to assume that the conditional mean of the dependent variable is correctly specified. Under these circumstances, the Poisson regression becomes a Poisson pseudo maximum likelihood (PPML) regression. Given that no distributional assumption is required for the dependent variable, the application of the PPML regression is not restricted to counting data but can be applied to any dependent variable with non-negative values (Santos Silva and Tenreyro, 2006; Correia et al., 2019a). In applying PPML to study aid allocation, we follow Fuchs and Vadlamannati (2013), Acht et al. (2015), Davies and Klasen (2019), and Dreher et al. (2019).

 $^{^{26}}$ We control for population size. Data on population is taken from CIESIN (2018). It is available for the years 2000, 2005, 2010, and 2015. To obtain a yearly population proxy, we interpolate the data after computing the population in the ADM1 regions. The estimates of the minister coefficients remain virtually unchanged with and without the population controls.

²⁷The quantitative interpretation of the coefficients is given by the following formula: $(exp(\gamma) - 1) * 100\%$.

²⁸See Kotsadam et al. (2018) for Nigeria, Wayoro and Ndikumana (2019) for Ivory Coast, Martorano et al., 2020 (2020) for 13 African countries, and Cruzatti et al. (2020) for 53 countries. While Cruzatti et al. (2020) document mortality-lowering effects of World Bank aid, they find that Chinese aid seems to increase mortality.

However, the mortality-lowering effects of the ministers are more immediate (recall that they are most pronounced for *healthmin_{ic,t}* and especially *healthmin_{ic,t-1}*, and that our tenure-related results suggest that health ministers influence mortality early in their term). Likely, at least part of the aid manifests in mortality-lowering effects only with a lag. In Appendix E.3, we provide some descriptive evidence that, indeed, in our sample, health aid is most strongly associated with lower mortality two years after it was committed. Hence, although health ministers might be able to influence the allocation of health aid, it is unclear to what extent World Bank health aid is a relevant mechanism driving our main finding for health ministers.

For key ministers, we find they have an impact on health-related flows in the current and subsequent year. For leaders, we find positive but statistically insignificant coefficients for health aid allocation in the current and subsequent year. We do not detect any influence of other cabinet members (see Appendix Figure 3). At the extensive margin (whether or not any health project is allocated to a region in a given year), we do not find any effects for health or key ministers (nor for leaders and other cabinet members), as Appendix Figures E.10 and E.11 show.

Taken together, we derive two conclusions from our analyses on World Bank health aid. First, this subsection provides causal evidence that health ministers possibly influence health aid allocation, and key ministers likely influence health aid allocation – an additional avenue through which they favor their birth regions.²⁹ Second, in terms of mechanisms explaining our main findings, our evidence on health aid seems to be have limited explanatory power for the mortality-lowering effects of health ministers (mainly because the timing does not match up well). There are most likely additional channels through which health ministers direct resources to their birth region.

Our analysis from this section highlights the ambiguity of the aid-related favoritism exercised by political figures. On the one hand, higher aid flows to ministers' birth regions might be a sign of political capture (where aid allocation is not needs-based) and thus reduce the effectiveness of aid. On the other hand, ministers could have information advantages (they know where, in their birth region, a project might have a high impact, or they feel more confident in monitoring projects in their birth region). Then, political motives need not always translate into lower aid effectiveness. Hence, whether favoritism by ministers results in lower aid effectiveness is ex ante ambiguous.

²⁹Warner (2010) provides some evidence for shortcomings in the World Bank's (pre-approval) evaluation of the projects, which suggests that the allocation of projects could be influenced by political incentives, and the governments of recipient countries might have some scope to influence the allocation decision.



Notes: Each point and corresponding 95% confidence interval represent a health minister (subfigure a) or key minister (subfigure b) indicator coefficient from a separate regression. Subfigure a (b) shows the health (key) minister coefficient from Equation 4, but we replace the health (key) minister indicator by indicators for the years before they enter office (analogous to Figure 2 but focusing on health aid amounts as an outcome). The indicators for four to two years before they take office can be interpreted as placebo tests. The year before they take office can only partly be interpreted as a placebo test due to our data coding procedure: Some ministers might already be in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.

Figure 3: Health and key ministers' effects on the allocation of health aid amounts before and after they enter office.

6 Conclusion

This work is, to our knowledge, the first ever systematic study of favoritism by cabinet members in Africa. In particular, we examine whether health ministers engage in regional favoritism. We combine a novel data set on the birthplaces of cabinet members in 36 African countries from 2001 to 2015 with geocoded data on neonatal and infant mortality (from the DHS) and on the location of World Bank health aid projects (from AidData).

Using a mother fixed effects strategy, we provide causal evidence that neonatal and infant mortality is lower in health (and to a lower extent also in key) ministers' birth regions. Part of this effect seems to run though increased access to healthcare services. Additionally, we provide some evidence that health (and key) ministers might allocate more health-related World Bank aid to their birth regions, hence providing evidence for an additional avenue of favoritism. However, the ministers' effect on mortality rates only seems to be partly (if at all) explained by the additional World Bank health aid.

Our finding that health ministers are able to improve health outcomes, but not necessarily through increased World Bank health aid, points to additional channels through which health ministers allocate resources to their birth region. This work thus implies that ministers' favoritism likely manifests itself in diverse ways.

Taken together, we document health outcome-related favoritism by health (and to a lower degree key) ministers. We conclude that not only can country leaders influence outcomes or the allocation of funds (as shown in previous work) but so can cabinet members. Hence, broad representation within cabinets (see Francois et al., 2015) seems to translate into actual power.

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Appendices

A Countries in our Sample

Our analysis sample consists of 36 countries. For each country, the number of DHS rounds is shown in parentheses. For 11 additional countries, zero DHS rounds are available, but they are included in our novel data set on ministers' birthplaces. They are also listed below. Our replication folder contains the birthplace data for all 47 countries.

1.	Algeria (0)	17.	Gabon (1)
2.	Angola (3)	18.	Gambia (1)
3.	Benin (3)	19.	Ghana (3)
4.	Botswana (0)	20.	Guinea (2)
5.	Burkina Faso (2)	21.	Guinea-Bissau (0)
6.	Burundi (2)	22.	Ivory Coast (2)
7.	Chad (2)	23.	Kenya (3)
8.	Cameroon (2)	24.	Liberia (3)
9.	Central African Republic (0)	25.	Lesotho (3)
10.	Congo, Democratic Republic of (2)	26.	Madagascar (2)
11.	Congo, Republic of (2)	27.	Malawi (3)
12.	Djibouti (0)	28.	Mali (3)
13.	Egypt (3)	29.	Mauritania (0)
14.	Equatorial Guinea (0)	30.	Morocco (1)
15.	Eritrea (0)	31.	Mozambique (3)
16.	Ethiopia (3)	32.	Namibia (2)

- 33. Niger (2)
- 34. Nigeria (4)
- 35. Rwanda (4)
- 36. Senegal (8)
- 37. Sierra Leone (2)
- 38. South Africa (1)
- 39. South Sudan (0)
- 40. Sudan (0)

- 41. Swaziland (1)
- 42. Tanzania (4)
- 43. Togo (1)
- 44. Tunisia (0)
- 45. Uganda (5)
- 46. Zambia (3)
- 47. Zimbabwe (3)

B Data coverage: Birthplaces



Notes: Share of all cabinet members for whom we have information on their birth region (ADM1). Darker shades indicate a higher share. Countries that are not in our sample are left white.

Figure C.1: Share of birthplaces identified, 2001-2015

C Additional Results for Section 3: Econometric Framework

	(1)	(2)	(3)	(4)
	$healthmin_{ct}$	$leader_{ct}$	keymin _{ct}	$cabinet_{ct}$
$disaster_{c,t-1}$	-0.036	0.023	-0.011	-0.018
	(0.047)	(0.027)	(0.061)	(0.049)
Obs.	648	648	648	648
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table C.1: Do natural disasters lead to switches in cabinet positions?

Notes: Panel with countries as the unit of observation. Estimates based on linear regressions. The dependent variables are indicators of whether at least one new health minister (*healthmin_{ct}*; column 1), a country leader (*leader_{ct}*, column 2), key minister (*keymin_{ct}*; column 3), or any cabinet member (*cabinet_{ct}*; column 4) is in power. The independent variable is an indicator variable for any natural disaster in the previous year (*disaster_{c,t}*-1). All specifications include country and year fixed effects. Robust standard errors adjusted for clustering on the country and year level. p-values in parentheses. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively.

D Additional Results for Section 4: Main Resu

	(1)	(2)
	$neonatal_{kpict}$	in f ant _{kpict}
$healthmin_{ic,t-1}$	-3.561***	-5.014***
	(1.118)	(1.824)
$leader_{ic,t-1}$	-1.709	-0.143
	(1.477)	(2.900)
PMother:	-1 281	-1 893
1 monucle, $t-1$	(0.795)	(1.481)
	(0.793)	(1.401)
$econ_{ic,t-1}$	0.257	-0.058
,.	(0.843)	(1.413)
		~ /
indut rade _{ic,t-1}	0.441	-0.737
,	(0.907)	(1.293)
$agr_{ic,t-1}$	-1.022	-2.184*
	(0.806)	(1.170)
ius.	0.865	4 027**
Jus _{ic,t-1}	(0.068)	(1,722)
	(0.908)	(1.722)
foreign _{ic t-1}	-0.260	-1.769
<i>v o io</i> , <i>i i</i>	(0.872)	(1.748)
	(
$cabinet_{ic,t-1}$	0.312	0.721
,	(0.584)	(0.864)
Obs.	1,141,752	1,001,268
Child-level controls	Yes	Yes
Mother FE	Yes	Yes
Country-year FE	Yes	Yes

Table D.1: Which key ministers drive the effect?

Notes: This table replicates columns 1 and 3 of Table 2. The only difference is that several indicators for key ministers are included. The key ministers are: $PMother_{ic,t-1}$: President or Prime Minister other than the effective country leader; $econ_{ic,t-1}$: economics, finance, and/or development minister; $indutrade_{ic,t-1}$: industry and/or trade minister; $agr_{ic,t-1}$: agriculture minister; $jus_{ic,t-1}$: justice minister; $foreign_{ic,t-1}$: foreign affairs minister. $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. See the notes to Table 2 for more details. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively.



Notes: Each point and corresponding 95% confidence interval represent a key minister indicator coefficient from a separate regression. Subfigure a (b) replicates column 2 (4) of Table 2. We replace the key minister indicator by indicators for the years before they are in office and with indicators for whether the previous key minister was born in the region. The indicators for four to two years before they are in office can be interpreted as placebo tests. The year before they are in office can only partly be interpreted as a placebo test due to our data coding procedure: Some key ministers might already be in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.

Figure D.2: Key ministers' effects on mortality before and after they enter office.



Notes: Each point and corresponding 95% confidence interval represent a leader indicator coefficient from a separate regression. Subfigure a (b) replicates column 2 (4) of Table 2. We replace the leader indicator by indicators for the years before they are in office and with indicators for whether the previous leader was born in the region. The indicators for four to two years before they are in office can be interpreted as placebo tests. The year before they are in office can only partly be interpreted as a placebo test due to our data coding procedure: Some leaders might already be in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.





Notes: Each point and corresponding 95% confidence interval represent a cabinet member indicator coefficient from a separate regression. Subfigure a (b) replicates column 2 (4) of Table 2. We replace the minister indicator by indicators for the years before they are in office and with indicators for whether the previous minister was born in the region. The indicators for four to two years before they are in office can be interpreted as placebo tests. The year before they are in office can only partly be interpreted as a placebo test due to our data coding procedure: Some ministers might already be in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.

Figure D.4: Other cabinet members' effects on mortality before and after they enter office.

	(1)	(2)	(3)	(4)
	$neonatal_{kpict}$	$neonatal_{kpict}$	in f ant _{kpict}	in f ant _{kpict}
healthmin _{ic,t-1}	-3.633***	-3.622***	-5.217***	-5.238***
	(1.094)	(1.106)	(1.871)	(1.865)
$leader_{ic,t-1}$		-1.767		-0.829
		(1.398)		(2.829)
<i>keymin_{ic,t-1}</i>		-0.703		-2.592***
. ,		(0.555)		(0.890)
<i>cabinet</i> _{<i>ic</i>,<i>t</i>-1}		0.389		0.809
,		(0.587)		(0.858)
Obs.	1,134,481	1,134,481	994,795	994,795
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table D.2: Health ministers and mortality in the subsequent year - Additional birth controls

Notes: This table replicates Table 2. The only difference is the additional controls: mother's age (squared) and dummy variables indicating whether the previous birth took place in the last 12 months, in the last 13 to 24 months, or in the last 25 to 36 months. See the notes to Table 2 for more details. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively.

Table D.3:	(Public)	health	ministers	and	mortality	in	the	subsequent	year

	(1)	(2)	(3)	(4)
	neonatal _{kpict}	neonatal _{kpict}	in fant _{kpict}	in f ant _{kpict}
<i>healthmin_{ic,t-1}</i>	-3.613***	-3.591***	-4.663***	-4.693***
	(0.935)	(0.939)	(1.672)	(1.652)
$leader_{ic,t-1}$		-1.644		-0.497
,		(1.435)		(2.830)
<i>keymin_{ic,t-1}</i>		-0.731		-2.643***
- ,		(0.555)		(0.893)
<i>cabinet</i> _{<i>ic</i>,<i>t</i>-1}		0.377		0.640
,		(0.584)		(0.850)
Obs.	1,141,752	1,141,752	1,001,268	1,001,268
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Notes: This table replicates Table 2. The only difference is that *healthmin_{ict}* includes all health-related ministries: health, public health, HIV/AIDS, population, sanitation. See the notes to Table 2 for more details. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, ***, and * indicate significance at the 1, 5, and 10%-level, respectively.

	(1)	(2)	(3)	(4)
	$neonatal_{kpict}$	$neonatal_{kpict}$	in f ant _{kpict}	in f ant _{kpict}
<i>healthmin_{ic,t-1}</i>	-3.184***	-3.127***	-4.935**	-4.772**
	(1.055)	(1.056)	(1.944)	(1.911)
$leader_{ic,t-1}$		-1.600		-0.334
,		(1.444)		(2.821)
keymin _{ic.t-1}		-0.507		-2.240**
2, -		(0.575)		(0.903)
$cabinet_{ic,t-1}$		-0.077		0.155
,.		(0.608)		(0.836)
Obs.	1,141,752	1,141,752	1,001,268	1,001,268
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table D.4: Health ministers (narrow) and mortality in the subsequent year

Notes: This table replicates Table 2. The only difference is that only ministers in the narrow sense are included, excluding e.g., vice ministers and ministers of state. See the notes to Table 2 for more details. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively.

Table D.5: Health ministers and mortality in the subsequent year - Controlling for population

	(1)	(2)	(3)	(4)
	$neonatal_{kpict}$	$neonatal_{kpict}$	in f ant _{kpict}	in f ant _{kpict}
healthmin _{ic,t-1}	-3.531***	-3.521***	-5.047***	-5.066***
	(1.107)	(1.117)	(1.860)	(1.851)
<i>leader</i> _{<i>ic</i>,<i>t</i>-1}		-1.656		-0.572
		(1.439)		(2.846)
keymin _{ic,t-1}		-0.712		-2.613***
- ,		(0.554)		(0.892)
<i>cabinet</i> _{ic,t-1}		0.293		0.673
,		(0.583)		(0.861)
Obs.	1,141,752	1,141,752	1,001,268	1,001,268
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Notes: This table replicates Table 2. The only difference is that the regressions control for a standardized population in the region. See the notes to Table 2 for more details. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively.

	(1)	(2)	(3)	(4)
	$neonatal_{kpict}$	$neonatal_{kpict}$	in f ant _{kpict}	in f ant _{kpict}
healthmin _{ic,t-1}	-4.309*	-4.209*	-3.783	-3.551
	(2.221)	(2.215)	(3.597)	(3.463)
$leader_{ic,t-1}$		-0.860		-0.196
,		(3.358)		(7.163)
<i>keymin_{ic,t-1}</i>		-0.334		-3.427*
- ,		(1.266)		(1.975)
$cabinet_{ic,t-1}$		-0.924		0.554
· -).		(1.387)		(1.931)
Obs.	330,853	330,853	275,062	275,062
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table D.6: Health ministers and mortality in the subsequent year - Mother moved here before birth

Notes: This table replicates Table 2. The only difference is that only children born after their mother moved to the current place of residence are included. See the notes to Table 2 for more details. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, ***, and * indicate significance at the 1, 5, and 10%-level, respectively.

Table D.7: Health ministers and mortality	v in the subsequent v	vear – Spatial correlati	on up to 100km
fuore Diff. freuten ministers und mortant	j m m o subbequent j	our opullur correlati	on up to roomin

	(1)	(2)	(3)	(4)
	$neonatal_{kpict}$	$neonatal_{kpict}$	in f ant _{kpict}	in f ant _{kpict}
healthmin _{ic,t-1}	-3.539***	-3.528***	-5.045**	-5.065**
	(1.123)	(1.137)	(2.141)	(2.130)
$leader_{ic,t-1}$		-1.681		-0.557
,		(1.397)		(2.647)
<i>keymin_{ic,t-1}</i>		-0.710		-2.615***
		(0.531)		(0.904)
$cabinet_{ic,t-1}$		0.297		0.670
,		(0.624)		(0.925)
Obs.	1,141,752	1,141,752	1,001,268	1,001,268
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Notes: This table replicates Table 2. The only difference is that the standard errors (in parentheses) are adjusted for spatial correlation up to 100km. See the notes to Table 2 for more details. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively.

	(1)	(2)	(3)	(4)
	$neonatal_{kpict}$	$neonatal_{kpict}$	in f ant _{kpict}	in f ant _{kpict}
healthmin _{ic,t-1}	-3.539**	-3.528**	-5.045**	-5.065**
	(1.528)	(1.566)	(2.151)	(2.173)
$leader_{ic,t-1}$		-1.681		-0.557
		(1.518)		(2.207)
<i>keymin_{ic,t-1}</i>		-0.710		-2.615***
		(0.573)		(0.804)
cabinat.		0 297		0.670
cubiner _{ic,t-1}		0.297		0.070
		(0.622)		(0.713)
Obs.	1,141,752	1,141,752	1,001,268	1,001,268
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table D.8: Health ministers and mortality in the subsequent year – Spatial correlation up to 500 km

Notes: This table replicates Table 2. The only difference is that the standard errors (in parentheses) are adjusted for spatial correlation up to 500km. See the notes to Table 2 for more details. ****, ***, and * indicate significance at the 1, 5, and 10%-level, respectively.

	(1)	(2)	(3)	(4)	(2)	(9)	(2)	(8)
Interaction with:	neonatal _{kpict} tenure _{ic,t–1}	$neonatal_{kpict}$ tenure $_{ic,t-1}$	infant _{kpict} tenure _{ic,t-1}	infant _{kpict} tenure _{ic,t–1}	$neonatal_{kpict}$ firstyear $_{ic,t-1}$	neonatal _{kpict} firstyear _{ic,t-1}	infant _{k pict} firstyear _{ic,t-1}	infant _{kpict} firstyear _{ic,t-1}
$healthmin_{ic,t-1}$	-4.681*** (1.278)	-4.685*** (1.281)	-6.432*** (2.206)	-6.650*** (2.162)	-1.893 (1.246)	-1.881 (1.258)	-3.935** (1.987)	-4.006** (1.968)
$healthmin_{ic,l-1} imes var$	1.077^{**} (0.509)	1.093^{**} (0.509)	1.284 (0.957)	1.333 (0.952)	-3.686** (1.724)	-3.760** (1.710)	-2.571 (2.677)	-2.711 (2.661)
leader _{ic,t-1}		-1.981 (1.657)		-2.226 (3.134)		-1.419 (1.455)		1.212 (2.834)
$leader_{ic,t-1} imes var$		0.090 (0.250)		0.775^{*} (0.464)		-1.401 (1.928)		-6.891^{**} (3.439)
keymin _{ic,t-1}		-0.498 (0.605)		-2.176** (1.000)		-0.606 (0.623)		-2.535*** (0.938)
keymin _{ic,t-1} × var		-0.146 (0.157)		-0.304 (0.278)		-0.338 (0.824)		-0.299 (1.279)
$cabinet_{ic,t-1}$		0.269 (0.655)		0.932 (0.969)		0.273 (0.624)		0.207 (0.918)
$cabinet_{ic,t-1} \times var$		0.002 (0.147)		-0.165 (0.270)		0.072 (0.861)		1.627 (1.376)
Obs. Child-level controls Mother FE	1,141,752 Yes Yes	1,141,752 Yes Yes	1,001,268 Yes Yes	1,001,268 Yes Yes	1,141,752 Yes Yes	1,141,752 Yes Yes	1,001,268 Yes Yes	1,001,268 Yes Yes
Country-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Notes: This table replicate with the first year being eq to 8). See the notes to Tabl significance at the 1, 5, and	s Table 2, adding ial to zero (colurr e 2 for more detai 10%-level, respe	interactions between a line 1 to 4), and <i>fii</i> ils. Robust standar ctively.	sen the minister set $t_{styearic,t} - 1$, and the set of the set	indicators and <i>t</i> , an indicator vari entheses) adjust	<i>snureic</i> , $t - 1$, the n able for the first ye.	umber of years the ar in which the resp the ADM1 and cou	respective minister ective minister is i ntry-year level. ***	is already in office n office (columns 5 , **, and * indicate

Table D.9: Health ministers and mortality - Tenure

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	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)
Interaction with:	neonatal _{kpict} leg _{ict}	neonatal _{kpict} leg _{ict}	inf ant _{kpict} leg _{ict}	inf ant _{kpict} leg _{ict}	neonatal _{kpict} exec _{ict}	neonatal _{kpict} exec _{ict}	infant _{kpict} exec _{ict}	inf ant _{kpict} exec _{ict}
$healthmin_{ic,t-1}$	-3.930***	-3.921***	-5.203**	-5.178**	-3.813***	-3.767***	-5.228**	-5.187**
	(1.266)	(1.268)	(2.154)	(2.132)	(1.300)	(1.311)	(2.223)	(2.220)
<i>healthmin</i> _{<i>ic,t-1</i>} × <i>var</i>	2.002	1.733	0.796	0.320	1.206	0.805	0.778	0.287
	(1.795)	(1.848)	(3.114)	(3.244)	(1.923)	(1.994)	(3.314)	(3.480)
$leader_{ic,t-1}$		-2.333		-1.343		-2.505*		-1.603
		(1.428)		(2.859)		(1.456)		(2.896)
$leader_{ic,t-1} \times var$		2.986		3.461		3.140^{*}		4.244
~		(1.961)		(3.646)		(1.782)		(3.298)
$keymin_{ic.t-1}$		-0.557		-2.725***		-0.731		-2.771***
х 		(0.601)		(0.968)		(0.613)		(0.969)
keymin $_{ic.t-1} imes var$		-0.854		0.429		0.074		0.627
~		(1.047)		(1.517)		(0.937)		(1.583)
$cabinet_{ic,t-1}$		0.348		0.780		0.401		0.968
		(0.619)		(0.946)		(0.607)		(0.939)
$cabinet_{ic,t-1} imes var$		-0.327		-0.587		-0.423		-1.410
		(1.104)		(1.694)		(1.055)		(1.673)
Obs.	1,141,752	1,141,752	1,001,268	1,001,268	1, 141, 752	1,141,752	1,001,268	1,001,268
Child-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Notes: This table replicate variables for whether there	s Table 2, adding is an election of t	interactions betw he legislative or th	een the ministe he executive, re	r indicators an spectively, in th	d legict (columns he current year. S	1–4) and with ex ee the notes to Ta	<i>tec_{ict}</i> (columns) the 2 for more	5–8), indicator letails. Robust
standard errors (1n parenth respectively.	eses) adjusted for	clustering at the	ADM1 and cou	ıntry-year level	. **, **, and * 1	ndicate significan	ice at the 1, 5, 5	ind 10%-level,

Table D.10: Health ministers and mortality – Elections

	(1)	(2)	(3)	(4)
	neonatal _{kpict}	neonatal _{kpict}	in f ant _{kpict}	in fant _{kpict}
<i>healthmin_{ic,t-1}</i>	-3.977***	-4.110***	-5.994**	-6.416**
,	(1.513)	(1.498)	(2.890)	(2.824)
$healthmin_{ic,t-1} \times democracy_{c,t-1}$	1.016	1.321	2.246	3.024
	(2.026)	(2.047)	(3.358)	(3.316)
<i>leader</i> _{<i>ic</i>,<i>t</i>-1}		0.813		4.445
		(2.971)		(3.925)
$leader_{ic,t-1} \times democracy_{c,t-1}$		-4.507		-8.783
		(3.929)		(5.377)
<i>keymin_{ic,t-1}</i>		-1.107		-3.776***
. ,		(0.748)		(1.181)
$keymin_{ic,t-1} \times democracy_{c,t-1}$		1.067		3.152*
		(1.061)		(1.814)
<i>cabinet</i> _{<i>ic</i>,<i>t</i>-1}		0.247		1.135
,		(0.774)		(1.218)
$cabinet_{ic,t-1} \times democracy_{c,t-1}$		0.071		-1.381
		(1.110)		(1.622)
Obs.	1,141,752	1,141,752	1,001,268	1,001,268
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table D.11: Health ministers and mortality in the subsequent year - Democracy

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator of whether a child died before reaching the age of one month (*neonatal_{kpict}*; columns 1 and 2) and one year (*infant_{kpict}*; columns 3 and 4), scaled by 1,000. The independent variables are indicators of whether the child was born in the same region as last year's health minister (*healthmin_{ic,t-1}*), country leader (*leader_{ic,t-1}*), key ministers (*keymin_{ic,t-1}*), or any cabinet member (*cabinet_{ic,t-1}*). *healthmin_{ic,t-1}* includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. *leader_{ic,t-1}* includes the effective head of state. *keymin_{ic,t-1}* includes key ministers in the sense of Francois et al. (2015), excluding the head of state. All ministers in the broad sense are included, e.g., also vice ministers and ministers of state. Child-level controls are gender, indicator variables for birth order, and for multiple births. All specifications include mother and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively.

	(1)	(2)	(2)	(4)
	(1)	(2)	(3)	(4)
1	<i>neonalalkpict</i>	5 29 4***	<i>injuni_{kpict}</i>	<i>inj uni_{kpict}</i>
nealthmin _{ic,t-1}	-5.517	-5.284	-5.0/5	-5.755
	(1.587)	(1.598)	(2.242)	(2.231)
$healthmin_{ic,t-1} \times educ_{pc}$	3.256	3.235	1.173	1.441
,	(1.996)	(2.004)	(3.444)	(3.421)
leader _{ic t-1}		-2.915*		2.589
,		(1.624)		(3.396)
$leader_{ic,t-1} \times educ_{nc}$		2.621		-6.853
		(2.733)		(4.608)
keymin _{ic t-1}		0.140		-2.555*
		(0.897)		(1.457)
$keymin_{ic\ t-1} \times educ_{nc}$		-1.660		-0.170
		(1.209)		(1.780)
cabinet _{ic.t-1}		0.122		1.021
,		(0.833)		(1.297)
$cabinet_{ic,t-1} \times educ_{pc}$		0.308		-0.736
,. r.		(1.118)		(1.689)
Obs.	1,141,675	1,141,675	1,001,202	1,001,202
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table D.12: Health ministers and mortality – Mother's education

Notes: This table replicates Table 2, adding interactions between the minister indicators and an indicator of whether the child's mother has any education (primary, secondary, tertiary $-educ_{pict}$). See the notes to Table 2 for more details. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, ***, and * indicate significance at the 1, 5, and 10%-level, respectively.

E Additional Results for Section 5: Mechanisms

E.1 Access to healthcare and mortality

In this section, we show the association between facility-based births and births attended by a professional, on the one hand, and mortality, on the other hand. We estimate the following equation:

$$mortality_{kpict} = \phi_p + \beta_{ct} + \kappa \ healthcare_{kpict} + \Omega \ control_{kpict} + \vartheta_{kpict}$$
(5)

The variable *healthcare*_{kpict} will capture either facility-based births or births with a professional attendant. The other variables are defined as per equation 1. The results are summarized in Table E.1: We find that being born in a health facility and/or in the presence of a professional birth attendant is related to lower neonatal and infant mortality.

	(1)	(2)
	$neonatal_{kpict}$	in f ant _{kpict}
facility _{kpict}	-6.375**	-14.948***
	(2.663)	(4.863)
attendant _{kpict}	-7.092***	-8.415*
1	(2.687)	(4.943)
Obs.	398,393	251,822
Child-level controls	Yes	Yes
Mother FE	Yes	Yes
Country-year FE	Yes	Yes

Table E.1: Mechanisms: Health facilities and professional birth attendant

Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator of whether a child died before reaching the age of one month (*neonatal_{kpict}*; column 1) and one year (*infant_{kpict}*; column 2), scaled by 1,000. The independent variables are indicators of whether a child was born in a health facility (*facility_{kpict}*) and in the presence of a professional birth attendant (*attendant_{kpict}*). Childlevel controls are gender, indicator variables for birth order, and for multiple births. All specifications include mother and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, **, and * indicate significance at the 1, 5, and 10%level, respectively.

	(1)	(2)	(3)	(4)
	<i>post_{kpict}</i>	<i>post_{kpict}</i>	ant e _{kpict}	ant e _{kpict}
healthmin _{ic,t-1}	1.081	5.073	15.594	15.233
	(17.888)	(14.989)	(15.240)	(13.416)
$leader_{ic,t-1}$				9.219
,				(19.843)
<i>keymin_{ic,t-1}</i>		30.810**		9.433*
- ,		(10.207)		(4.673)
$cabinet_{ic,t-1}$		-6.384		8.639
,		(13.078)		(5.172)
Obs.	5,542	5,542	16,709	16,709
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table E.2: Ante- and postnatal care

Notes: This table replicates Table 2. The only difference is the dependent variables: They are a dummy for whether the mother attended a postnatal care visit within two months of the birth (*post_{kpict}* in columns 1 and 2) and whether the mother attended at least one prenatal care visit before the birth (*ante_{kpict}* in columns 3 and 4). See the notes to Table 2 for more details. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, **, and * indicate significance at the 1, 5, and 10%-level, respectively. The leader indicator is dropped in column 2 due to collinearity.



Notes: Each point and corresponding 95% confidence interval represent a health minister indicator coefficient from a separate regression. Subfigure a (b) replicates column 2 (4) of Table 3. We replace the health minister indicator by indicators for the years before they are in office and with indicators for whether the previous health minister was born in the region. The indicators for four to two years before they are in office can be interpreted as placebo tests. The year before they are in office can only partly be interpreted as a placebo test due to our data coding procedure: Some health ministers might already be in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.

Figure E.1: Health ministers' effects on healthcare access before and after they enter office.



Notes: Each point and corresponding 95% confidence interval represent a key minister indicator coefficient from a separate regression. Subfigure a (b) replicates column 2 (4) of Table 3. We replace the key minister indicator by indicators for the years before they are in office and with indicators for whether the previous key minister was born in the region. The indicators for four to two years before they are in office can be interpreted as placebo tests. The year before they are in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.





(a) Health facility

(Lag 1)

(Lag 2) (Lag 3) (Lag 4)

(Before 4) (Before 3) (Before 2) (Before 1) (Now)

40



fore 4) (Before 3) (Before 2) (Before 1) (Now) (Lag 1) (Lag 2) (Lag 3) (Lag 4)

Notes: Each point and corresponding 95% confidence interval represent a leader indicator coefficient from a separate regression. Subfigure a (b) replicates column 2 (4) of Table 3. We replace the leader indicator by indicators for the years before they are in office and with indicators for whether the previous leader was born in the region. The indicators for four to two years before they are in office can be interpreted as placebo tests. The year before they are in office can only partly be interpreted as a placebo test due to our data coding procedure: some leaders might already be in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.

Figure E.3: Leaders' effects on healthcare access before and after they enter office.



Notes: Each point and corresponding 95% confidence interval represent a cabinet member indicator coefficient from a separate regression. Subfigure a (b) replicates column 2 (4) of Table 3. We replace the minister indicator by indicators for the years before they are in office and with indicators for whether the previous minister was born in the region. The indicators for four to two years before they are in office can be interpreted as placebo tests. The year before they are in office can only partly be interpreted as a placebo test due to our data coding procedure: Some ministers might already be in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.

Figure E.4: Other cabinet members' effects on healthcare access before and after they enter office.

	(1)	(2)	(3)	(4)
	$facility_{kpict}$	facility _{kpict}	attendant _{kpict}	$attendant_{kpict}$
healthmin _{ic,t-1}	18.614***	18.420***	16.933***	16.838***
	(5.631)	(5.604)	(6.082)	(6.066)
$healthmin_{ic,t-1} \times educ_{pc}$	-16.209**	-16.178**	-12.433*	-12.440*
	(7.152)	(7.154)	(7.381)	(7.372)
		2 005		6.50
<i>leader</i> _{ic,t-1}		2.805		-6.569
		(12.782)		(13.008)
leadering 1 × educing		-8 176		7 678
reture pc		(13,888)		(11.306)
		(15.000)		(11.500)
<i>keymin_{ic,t-1}</i>		-0.639		-0.907
		(2.886)		(2.911)
keymin: 1 × educ.		6 044		9 664**
$keymin_{lc,l=1} \wedge euwepc$		(3.770)		(3.906)
		(3.770)		(3.900)
<i>cabinet</i> _{<i>ic</i>,<i>t</i>-1}		-2.125		-1.345
		(3.684)		(3.505)
$cabinet_{ic,t-1} \times educ_{pc}$		-0.631		-3.481
		(4.626)		(4.314)
Obs.	380,165	380,165	387,668	387,668
Child-level controls	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes
Country-year FE	Yes	Yes	Yes	Yes

Table E.3: Health ministers and alternative health outcomes - Mother's education

Notes: This table replicates Table 3, adding interactions between the minister indicators and an indicator of whether the child's mother has any education (primary, secondary, tertiary $-educ_{pict}$). See the notes to Table 3 for more details. Robust standard errors (in parentheses) adjusted for clustering at the ADM1 and country-year level. ***, ***, and * indicate significance at the 1, 5, and 10%-level, respectively.

E.2 Health allocation at the ADM1 level

To construct variables capturing the allocation of World Bank health aid on the ADM1 level, we use georeferenced data on World Bank projects from AidData.

The data from AidData includes projects from the International Development Association (IDA) and International Bank for Reconstruction and Development (IBRD) product lines. Each World Bank aid project belongs to one or several sectors, such as health, education, agriculture, etc., which allows for the identification of health projects.³⁰ For the period spanning 2001 to 2014, 307 health projects are allocated to the 47 countries we study. 231 (or 75%) of these projects come with geoinformation that is precise enough to match them to ADM1 regions. The 231 matched projects are dispersed over 2,496 project locations. Apart from the geoinformation, AidData also provides the committed amount in current USD, the targeted sector, and the year the project was approved. For all 231 matched projects, information on the amount committed to the project is available. As we do not know the amount committed to each project location, we assume that the amount is evenly distributed across project locations, and we divide the project amount by the number of project locations. Some projects span several years. We consider the year in which the project is approved. On average, a country in our sample receives USD 25 million of health-related flows per year.³¹ We construct two variables: an indicator variable of whether a region *i* receives any health aid in a given year t (healthaid D_{ict} , with D for dummy), and the aggregated amount for all projects assigned to i in t (healthaid_{ict}). For our analysis at the child-level, we connect these health aid variables to the children in our panel based on their ADM1 region and their year of birth.

³⁰AidData also provides geocoded data on Chinese aid. However, as there are too few health projects to conduct meaningful analyses, we restrict our analyses to World Bank aid.

³¹Considering all types of flows, the figure amounts to USD 143 million per year.

E.3 Health aid and mortality

In this section, we seek to uncover any negative associations between health aid and mortality in our data. We run the following equation:

$$mortality_{kpict} = \phi_p + \beta_{ct} + \kappa \ healthaid_{ict} + \Omega \ control_{kpict} + \vartheta_{kpict} \tag{6}$$

If health aid reduces mortality rates, we expect a negative sign for κ . Health aid and mortality are likely co-determined. With the mother fixed effects, we compare siblings born at different levels of regional aid, lending some credibility to the interpretation of κ that goes beyond a descriptive analysis. Nevertheless, to err on the side of caution, we recommend a descriptive interpretation of the estimates from Equation 6.

In Figure E.5, we plot the coefficient of *healthaid_{ict}* for lags zero to four. The Figure shows that health aid reduces neonatal and infant mortality in two years. This pattern remains similar when controlling for population in Figure E.6, when using log amounts in Figure E.7 (note that the coefficients are noisier than in the other graphs) and when using disbursed instead of committed amounts in Figure E.8.



Notes: Each point and corresponding 95% confidence interval represent a health aid coefficient from a separate regression based on Equation 6, where each point represents a different lag structure (ranging from health aid allocated this year to four years ago). The outcome is neonatal (infant) mortality in subfigure a (b).

Figure E.5: Effect of health aid on mortality over time



Notes: This figure is a robustness test of Figure E.5. The only difference is that the regressions include controls for population. See the notes to Figure E.5 for more information.

Figure E.6: Effect of health aid on mortality over time- - Controlling for population



(a) Neonatal mortality

(b) Infant mortality

Notes: This figure is a robustness test of Figure E.5. The only difference is that the log amount of health aid is used. See the notes to Figure E.5 for more information.

Figure E.7: Effect of health aid on mortality over time - Log amounts



Notes: This figure is a robustness test of Figure E.5. The only difference is that the disbursed instead of the committed amounts are used. See the notes to Figure E.5 for more information.

Figure E.8: Effect of health aid on mortality over time - Disbursed amounts

E.4 Health aid allocation





(b) Other cabinet members

Notes: Each point and corresponding 95% confidence interval represent a leader (subfigure a) or other cabinet member (subfigure b) indicator coefficient from a separate regression. Subfigure a (b) shows the leader (other cabinet member) coefficient from Equation 4, but we replace the leader (other cabinet member) indicator by indicators for the years before they are in office (analogous to Figure 2 but focusing on health aid amounts as an outcome). The indicators for four to two years before they are in office can be interpreted as placebo tests. The year before they are in office can only partly be interpreted as a placebo test due to our data coding procedure: Some ministers might already be in office for a few months, but we do not code them as such unless they are in office for more than six months in a given year.

Figure E.9: Leaders' and other cabinet members' effects on the allocation of health aid amounts before and after they enter office.





(b) Key ministers

Notes: This figure is a robustness test of Figure 3. The only difference is that the outcome is an indicator of whether there was any health aid (instead of health aid amounts), and estimates are based on OLS (instead of PPML). See the notes to Figure 3 for more information.

Figure E.10: Health and key ministers' effects on the allocation of health aid (extensive margin) before and after they enter office.



(a) Leaders

(b) Other cabinet members

Notes: This figure is a robustness test of Figure E.9. The only difference is that the outcome is an indicator of whether there was any health aid (instead of health aid amounts), and estimates are based on OLS (instead of PPML). See the notes to Figure E.9 for more information.

Figure E.11: Leaders' and other cabinet members' effects on the allocation of health aid (extensive margin) before and after they enter office.



(a) Health ministers

(b) Key ministers

Notes: This figure replicates Figure 3. The only difference is that the outcome is the amount of non-health aid (instead of health aid amounts). See the notes to Figure 3 for more information.

Figure E.12: Health and key ministers' effects on the allocation of non-health aid amounts before and after they enter office.



(a) Leaders

(b) Other cabinet members

Notes: This figure replicates Figure E.9. The only difference is that the outcome is the amount of non-health aid (instead of health aid amounts). See the notes to Figure E.9 for more information.

Figure E.13: Leaders' and other cabinet members' effects on the allocation of non-health aid amounts before and after they enter office.