Essays in Development Economics

DISSERTATION
of the University of St.Gallen,
School of Management, Economics,
Law, Social Sciences, International Affairs
and Computer Science,
to obtain the title of
Doctor of Philosophy in Economics and Finance

submitted by

Noémie Zurlinden

from

Attiswil (Bern)

Approved on the application of

Prof. Dr. Roland Hodler

and

Prof. Dr. Andreas Fuchs

Dissertation no. 5085

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The University of St.Gallen, School of Management, Economics, Law, Social Sciences, International Affairs and Computer Science, hereby consents to the printing of the present dissertation, without hereby expressing any opinion on the views herein expressed.

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The President:

Prof. Dr. Bernhard Ehrenzeller

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Abstract

This dissertation comprises three contributions in development economics. Using geocoded data, ethnic stratification, social conflict, favoritism and local aid effectiveness in Africa are studied.

Chapter 1 conjectures that mistrust and social conflict in a society may depend on ethnic stratification, i.e., the extent to which the hierarchy in socio-economic positions across individuals follows ethnolinguistic lines. This chapter defines and axiomatically characterizes an index of ethnic stratification that generalizes the idea of between-group inequality to situations where data on economic and ethnolinguistic distances between pairs of individuals is available. It uses Afrobarometer survey data to measure ethnic stratification at the level of towns and villages in 26 ethnically diverse African countries. It shows that ethnic stratification is negatively related to trust in relatives, neighbors and other acquaintances, and positively related to nearby conflicts. These findings shed new light on the debate about the merits of conflict and contact theory.

Chapter 2 conducts a systematic study of favoritism by cabinet members in Africa with continent-wide coverage (47 countries). For this purpose, birthplace information for all cabinet members between 2001 and 2014 was hand-collected. This chapter provides causal evidence of favoritism by health ministers. First, administrative regions receive more World Bank health aid when a region-born health minister is in office. Second, neonates and infants are less likely to die when the current health minister originates from their region. However, the reduction in mortality is not associated with increased health aid, implying that health ministers' favoritism also occurs through other channels.

Chapter 3 studies whether World Bank health aid can improve health outcomes at the local level. Exploiting geocoded data on the location of projects and on individual-level health outcomes, it investigates whether health-related World Bank projects reduce infant mortality at the local level in 25 African countries. This chapter shows that children born close to health-related World Bank projects are less likely to die before their first birthday than their siblings born before any health projects are implemented in their vicinity. It studies whether this effect is higher where the need for aid is higher and institutions stronger. The results suggest that health aid is more effective in more disadvantaged subnational areas but not in poorer countries. No evidence is found that the effect of health projects is higher in subnational regions or countries with stronger institutions.

Zusammenfassung

Die vorliegende Dissertation umfasst drei Beiträge zur Entwicklungsökonomie. Ethnische Stratifikation, soziale Konflikte, Vetternwirtschaft und die lokale Effektivität von Entwicklungshilfe in Afrika werden mithilfe von geocodierten Daten untersucht.

Kapitel 1 befasst sich mit der Frage, ob Misstrauen und soziale Konflikte von ethnischer Stratifikation – dem Ausmass, in welchem die Hierarchie in sozio-ökonomischen Positionen zwischen Individuen ethnolinguistischen Trennlinien folgt – abhängen. Das Kapitel definiert und charakterisiert axiomatisch einen Index für ethnische Stratifikation. Dieser Index verallgemeinert das Konzept von Ungleichheit zwischen Gruppen zu Situationen, in welchen Daten zu ökonomischen und ethnolinguistischen Distanzen zwischen Individuen vorhanden sind. Umfragedaten von Afrobarometer werden verwendet, um ethnische Stratifikation in Städten und Dörfern in 26 afrikanischen Ländern zu messen. Es wird gezeigt, dass ethnische Stratifikation negativ mit Vertrauen in Verwandte, Nachbarn, und andere Bekannte zusammen hängt, und positiv mit gewaltsamen Konflikten. Diese Resultate werfen neues Licht auf die Konflikt- und Kontakttheorien.

Kapitel 2 beleuchtet das Ausmass von Vetternwirtschaft in 47 afrikanischen Ländern und untersucht, ob Minister ihre Herkunftsregionen bevorzugen. Zu diesem Zweck wurden Informationen zu den Geburtsregionen der Minister zwischen 2001 und 2014 gesammelt. Dieses Kapitel zeigt kausale Evidenz für Vetternwirtschaft. Erstens erhalten administrative Regionen mehr gesundheitsbezogene Hilfsgelder von der Weltbank, wenn der aktuelle Gesundheistminister von dieser Region stammt. Zweitens sterben Neugeborene und Kleinkinder, welche in derselben Region wie der Gesundheitsminister geboren wurden, mit einer geringeren Wahrscheinlichkeit. Die Mortalitätsreduktion kann nicht durch Gesundheitsgelder der Weltbank erklärt werden, was darauf hindeutet, dass Gesundheitsminister auch andere Ressourcen in ihre Geburtsregion lenken können.

Kapitel 3 untersucht, ob gesundheitsbezogene Weltbankprojekte Säuglingssterblichkeit reduzieren. Dazu werden geocodierte Daten zur Position von Projekten und Umfragedaten zum Gesundheitszustand von Individuen kombiniert. Das Kapitel zeigt, dass Kinder, welche in der Nähe von aktiven Gesundheitsprojekten geboren wurden, mit einer geringeren Wahrscheinlichkeit vor dem ersten Geburtstag sterben als ihre Geschwister, welche vor dem Start des ersten Projekts geboren wurden. Das Kapitel untersucht zusätzlich, ob dieser Effekt in subnationalen Regionen und Ländern grösser ist, in denen ein grösserer Bedarf für Hilfsgelder besteht und Institutionen stärker sind. Die Resultate deuten darauf hin, dass Gesundheitsgelder in benachteiligten Regionen effektiver sind, aber nicht in ärmeren Ländern. Es wird keine Evidenz dafür gefunden, dass Gesundheitsprojekte in Regionen und Ländern mit stärkeren Institutionen einen grösseren Effekt haben.

Contents

Chapter 1

Measuring ethnic stratification and its effect on trust in Africa

Joint with Roland Hodler, Sorawoot Srisuma and Alberto Vesperoni
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1.1 Introduction

A main goal of the social sciences is to understand and predict cooperation and social conflict, whereby the latter is a broad concept ranging from mistrust to outbreaks of organized violence. Ethnic diversity and economic inequality are prominent candidates for predicting trust and conflict. The effects of diversity and inequality, however, may be intertwined. Conflict and contact theory, for instance, shed light on this interrelation. Conflict theory predicts that diversity and interactions among members of distant ethnolinguistic groups often lead to perceived threats, greater antagonism, mistrust and conflict. In contrast, contact theory, which goes back to Allport (1954), states that intergroup contact can reduce prejudice towards other groups and, thereby, increase trust and reduce conflict. However, Allport (1954) did not expect intergroup contact to have such desirable effects in all circumstances. Among others, he emphasized that the members of the different ethnic groups require "equal status" for interactions to reduce prejudice. This condition makes clear that conflict and contact theory are – from a theoretical perspective – not mutually exclusive.

The empirical literature on the effects of ethnic diversity on social conflict typically employs indices of ethnic fractionalization and polarization. There is considerable evidence that these indices are related to mistrust and a low sense of community (e.g., Alesina and La Ferrara, 2000, 2002; Algan et al., 2016) as well as civil conflict (e.g., Montalvo and Reynal-Querol, 2005; Esteban et al., 2012). These results are consistent with conflict theory, but it is unclear how they relate to contact theory, as these indices are silent on the status of the members of the different ethnic groups. Partly aimed at addressing this shortcoming, some recent studies show that high levels of between-group inequality (sometimes called ethnic or horizontal inequality) are related to low trust (Tesei, 2017) and civil conflict (e.g., Østby, 2008; Cederman et al., 2011; Gubler and Selway, 2012; Guariso and Rogall, 2017).[1]

In this paper, we propose a new predictor of social conflict that builds on the idea of between-group inequality, but offers a more sophisticated account of the interaction between ethnic diversity and economic inequality, and allows us to shed new light on the debate about the merits of conflict and contact theory. We call this predictor an index of *ethnic stratification.* Social stratification refers to the hierarchy of socio-economic positions in a population, and ethnic stratification, therefore, to the extent to which the hierarchy in socio-economic positions follows ethnolinguistic lines. Our index is based on interactions between pairs of individuals. A key assumption is that the degree of alienation (or mistrust) between each pair depends on their economic and ethnolinguistic distances, i.e., the difference in their economic resources and the dissimilarity of the languages

[1] See Kanbur (2006) for an early contribution highlighting the importance of between-group inequality in social conflict.

they speak. Our index of ethnic stratification extends the index of ethnic fractionalization by weighting ethnolinguistic differences between pairs of individuals by their economic distances. Symmetrically, it extends the Gini coefficient of economic inequality by weighting economic inequalities between pairs of individuals by their ethnolinguistic distance. In this sense, our index generalizes the idea of between-group inequality.

Our reliance on ethnolinguistic distances rather than purely categorical data on group affiliation is a major difference to measures of between-group inequality. We thereby follow earlier contributions to the literature on ethnic diversity that proxy for the degree of "alienation" between two members of different ethnolinguistic groups using the dissimilarity of their languages (e.g., Fearon, 2003; Desmet et al., 2009). As an example, consider the three most common languages in Nigeria: Hausa, Igbo and Yoruba. The latter two are closer to one another than to Hausa, as they both belong to the Niger-Congo language family, while Hausa is an Afroasiatic language. To us, it seems desirable to take this information into account when measuring how economic resources are distributed across "alienated" individuals with the aim of predicting social tensions.[2]

The experience of the Igbos in Nigeria illustrates this point. During and after colonization, the Igbos were – in the terminology of Horowitz (1985) – an advanced group in a backward region. Many Igbos were well-educated thanks to Christian mission schools, but there were few economic opportunities in their native Eastern Region. As a consequence, Igbos migrated to other regions of Nigeria "to get jobs in the civil service, trading companies, utilities. Nigeria became, in effect, an Igbo diaspora" (Diamond, 1967, p. 43). The resulting ethnic stratification was higher in towns in the Hausa-dominated Northern Region than in towns in the Yoruba-dominated Western Region for two reasons: First, the ethnolinguistic distance between Igbo and Hausa is larger than that between Igbo and Yoruba.

[2] There is a second advantage of relying on ethnolinguistic distance. The arbitrary decision of whether to treat two closely related ethnicities as a single group or as two distinct groups may have a large effect on between-group inequality when using ethnicity as a categorical variable. In contrast, ethnolinguistic distances allow for "smoothing" this problem by giving a small, but non-zero, weight to economic differences between members of closely related ethnicities. In this sense, our approach can be interpreted as measuring between-group inequality based on a "fuzzy partition" of the population into (unobserved) groups, where the probability that two individuals belong to the same group depends on their (observed) ethnolinguistic distance.

Second, the economic distance, in particular the educational distance, also tended to be greater between Igbo migrants and natives in the Northern Region, where the local Muslim elite opposed Christian missions, than between Igbo migrants and natives in the Western Region. While mistrust and intergroup violence were common in Nigeria in the years after independence, they increased dramatically after two military coups in 1966. “All the envy, resentment and mistrust that Northerners felt for the minority Eastern community living in their midst burst out with explosive force into a pogrom. [...] In the savage onslaught that followed thousands of Easterners died or were maimed, and as others sought to escape the violence, a mass exodus to the East began” (Meredith, 2005, p. 202). While far from peaceful, violence against Igbos was less extreme in the Western Region, where Igbo in-migration had not raised ethnic stratification to the same extent as in the Northern Region.

This paper is divided into a theoretical and an empirical part. As mentioned above, in our theoretical framework we assume that the degree of alienation (or mistrust) between each pair of individuals is determined by the distances between their economic and ethnolinguistic (or social) traits, and we restrict our attention to a class of bivariate measures that are expressions of the *expected alienation* between a randomly selected pair. Having introduced this general class of measures, we focus on a particular index from this class where the alienation of each pair is defined by the product of their economic and ethnolinguistic distances. This strong degree of complementarity across dimensions ensures that economic and ethnolinguistic distances between two individuals are counted only if these individuals are diverse in both dimensions, which is an essential feature of ethnic stratification as a generalization of between-group inequality.

Ethnic stratification, as we define it, depends on two crucial properties of economic and ethnolinguistic distances: their overall magnitude and their co-directionality, i.e., the extent to which these distances correlate across pairs of individuals. To better understand these forces, we show that our index simplifies to the product of the index of generalized ethnic fractionalization, the Gini coefficient of the wealth distribution and the average wealth in the population if we abstract from the co-directionality of economic and ethnolinguistic distances. Hence, ethnic stratification depends on these three components plus co-directionality. To further clarify the properties of our index, we present an axiomatic characterization that uniquely identifies our index from the class of measures of expected alienation via a set of axioms that we motivate as desirable properties of a measure that generalizes between-group inequality. We consider three axioms: *co-directionality by wealth creation or transfer*, *bi-polarization by wealth transfer*, and *co-directionality by linguistic change.* Each of these axioms focuses on a particular trade-off between increasing (decreasing) the magnitude of economic or ethnolinguistic distances and decreasing (increasing) their co-directionality to maintain or increase a given level of ethnic stratification.

Our index can be readily applied to data. There are at least two ways in which it can be used. First, as a summary statistic for directly comparing ethnic stratification across populations. We show that it is easy to perform inference with our index. Second, our index can be used as a key variable to explain and predict social conflict in regression analyses. We illustrate the use of our index in both cases. The main application focuses on the latter.

We use data from geocoded Afrobarometer surveys (BenYishay et al., 2017) for 26 ethnically diverse African countries. We match the respondents' ethnic groups and native languages to the languages listed in the Ethnologue (Gordon Jr., 2005), which allows us to use language trees to calculate ethnolinguistic distances between pairs of individuals. We further construct a wealth index, which allows us to calculate economic distances between pairs of individuals. These distances enable us to compute our index of ethnic stratification and its components at the level of, e.g., countries, or towns and villages. To investigate whether ethnic stratification is a predictor of social conflict at the level of towns and villages, we focus on its relation to *trust* in relatives, neighbors and other people the respondent knows, who are likely to live close by. We thereby focus on the Afrobarometer surveys of round 5 (conducted in 2011–2013), because much less information about the respondents' wealth is available in earlier rounds, and because round 6 does not include the relevant trust questions.

Our main specifications include interacted country-ethnolinguistic group fixed effects and many individual and geographical control variables. We find that respondents are less trusting when local ethnic stratification is high. As one may expect, this negative association is stronger for trust in neighbors and other acquaintances than for trust in relatives. Importantly, it is robust to controlling for ethnic fractionalization and the Gini coefficient. We further document that local ethnic stratification is positively related to the respondents' fear of crime (but not necessarily crime itself) and nearby violent conflict events.

Focusing on ethnic fractionalization, we find a negative relation to trust in neighbors and other acquaintances in specifications that omit ethnic stratification, but not in specifications that include ethnic stratification. These results have important implications for the debate about the merits of conflict and contact theory. The finding that high local ethnic fractionalization tends to go hand-in-hand with low trust towards neighbors and other acquaintances supports conflict theory. The finding that this negative association is strong when the ethnolinguistic distances are complemented by differences in economic resources but disappears when the members of the different groups that interact at the local level have similar socio-economic status supports contact theory.

Our paper develops as follows: Section 1.2 reviews the related theoretical and empirical literature. Section 1.3 introduces our index and presents its decomposition and an axiomatic characterization. Section 1.4 shows how to compute our index using Afrobarometer survey data and illustrates a cross-country comparison with African countries. Section 1.5 presents our main empirical application studying the effect of local ethnic stratification on trust in African towns and villages. Section 1.6 concludes.

1.2 Related literature

1.2.1 Theoretical literature on inequality and diversity measurement

The theoretical literature related to our contribution is vast, partly because we stand at the intersection between inequality and diversity measurement. Chakravarty (2015) presents a comprehensive review of both fields. In this section we focus on contributions that are particularly close to our framework and methodology. To start with, we see ethnic stratification as an extension of the concept of between-group inequality and our measure is evidently related to the group decomposition of the Gini coefficient. The axiomatic approach to between-group inequality started with a series of seminal contributions characterizing indices decomposable into between-group and within-group components (e.g., Bourguignon, 1979; Cowell, 1980; Shorrocks, 1980). In particular, Shorrocks (1980) shows that, to be decomposable in such a fashion, an index must belong to the class of Generalized Entropy measures. As the Gini coefficient does not belong to this class, its decomposition presents a residual term which has been the subject of various studies and interpretations.[3] Roughly speaking, our index of ethnic stratification can be seen as a generalization of the complement of the within-group component of the Gini coefficient, i.e., the sum of the between-group component and the residual. We choose to build on the Gini coefficient rather than a measure of Generalized Entropy as the latter does not lend itself naturally to the introduction of ethnolinguistic distances, which we believe are crucial for the measurement of ethnic stratification.

In a broader perspective, our index can be seen as a special type of multivariate inequality measure with two dimensions: economic and social traits. All multivariate inequality measures present a certain degree of complementarity across dimensions, which is typically moderate. In this work we deliberately focus on a particularly high degree of complementarity to capture the essential feature of ethnic stratification as a generalization of between-group inequality, i.e., that economic and social distances between individuals are counted only if they differ in both dimensions. Within the context of multivariate inequality, our index can be seen as a bivariate extension of the Gini coefficient.[4]

[3] Gini himself denoted it as the "transvariation" coefficient. Within the literature, Ebert (1988) interprets the residual as a measure of the overlap of groups' distributions, while Lambert and Aronson (1993) explore its geometrical properties and link it to the Lorenz curve. There are many ways of decomposing the Gini coefficient, which lead to alternative formulations of the residual. See Deutsch and Silber (1999) for a review.

[4] Other multivariate Gini indices are defined in Koshevoy and Mosler (1997), Gajdos and Weymark (2005) and Banerjee (2010). In particular, the distance-Gini mean difference of Koshevoy and Mosler (1997) and symmetric indices of the class of measures characterized in Theorem 4 of Gajdos and Weymark (2005) belong to our class of measures of expected alienation for

Our index can also be interpreted as a diversity measure in the form of a bivariate extension of the generalized fractionalization index formulated in Greenberg (1956), which extends the well-known ethnolinguistic fractionalization (ELF) index by introducing continuous distances between languages. Among other diversity measures, the univariate polarization indices in Esteban and Ray (1994), Duclos et al. (2004), and Reynal-Querol and Montalvo (2005) are related to our model but contain a crucial difference: while we assume that the alienation between two individuals is only determined by the distance of their attributes, they additionally consider how many individuals share an attribute with them (i.e., the group effect).[5] To the best of our knowledge there are no multivariate fractionalization measures in the literature, while there are some multivariate polarization indices that use categorical attributes to define groups.[6] Permanyer (2012) and Permanyer and D'Ambrosio (2015) axiomatically characterize bivariate measures where groups are defined via a categorical attribute and the polarization between groups is quantified via a cardinal or ordinal attribute. Other non-axiomatic contributions present similar features (e.g., Gigliarano and Mosler, 2009).

the bivariate case. However, as the economic distances between socially identical individuals are counted in the measurement of inequality in their models, their indices do not satisfy the aforementioned essential feature of ethnic stratification as a generalization of between-group inequality. Other multivariate inequality measures with a high level of complementarity belong to the Generalized Entropy family and are characterized in Tsui (1995, 1999). The Entropy indices are not measures of expected alienation and, more generally, are not based on distances between individuals but on individual traits that should be meaningfully ordered from high to low (e.g., income or years of education). As already mentioned, this is a crucial limitation in the context of diversity measurement as it precludes taking ethnolinguistic distances into account.

[5]When comparing the performance of various diversity measures in predicting outcomes related to social conflict, Desmet et al. (2012) rarely find significant differences between polarization and fractionalization measures, indicating that omitting the group effect may be of secondary importance. Indeed, there can be ambiguity on the degree (and even direction) of the effect of group size on the ability of a group to mobilize for collective action due to the opposing forces of economies of scale and free-riding (e.g., Olson, 1965; Isaac and Walker, 1988; Feddersen and Pesendorfer, 1998; Guarnaschelli et al., 2000; Esteban and Ray, 2001).

[6]In their axiomatic analysis of the Greenberg (1956) index, Bossert et al. (2011) interpret it as multivariate and postulate that the average fractionalization across attributes should be equal to the fractionalization of the average attributes, implying that it is additively separable in each dimension. Hodler et al. (2017) propose a framework analogous to ours for the measurement of ethnic segregation based on spatial and ethnolinguistic distances. Their axiomatic characterization treats the spatial and ethnolinguistic dimensions symmetrically, while in this paper we consider specific axioms for the economic dimension based on progressive/regressive transfers of wealth in the tradition of inequality measurement.

1.2.2 Empirical literature on inequality, diversity, and trust

Going back to Putnam (2000) and Uslaner (2002), the literature on trust typically distinguishes between particularized and generalized (or social) trust. Particularized trust refers to trust towards people within a small radius, whom the trusting individual knows well and who typically (but not necessarily) belong to the same socio-economic or ethnolinguistic group. Generalized trust refers to trust towards socially distant people about whom the trusting individual has little or no information and who typically belong to different groups. The importance of this distinction is supported by evidence that the two types of trust can even be negatively correlated (e.g., Alesina and Giuliano, 2011).

Many cross-country studies focus on generalized trust and provide evidence for a negative relation between economic inequality and ethnic diversity, on the one hand, and generalized trust, on the other (e.g., Knack and Keefer, 1997; Delhey and Newton, 2005; Bjørnskov, 2007; Nannestad, 2008). These findings are often (implicitly) interpreted as evidence for a causal effect of inequality and diversity on trust, but the direction of causality is difficult to disentangle. Bergh and Bjørnskov (2014) indeed provide evidence for reverse causality running from generalized trust to economic inequality. In line with the aforementioned cross-country evidence, several studies find that within a single (Western) country, individuals living in ethnically mixed or economically unequal localities have lower generalized trust (e.g., Alesina and La Ferrara, 2002; Leigh, 2006; Gustavsson and Jordahl, 2008).

Tesei (2017) is the first to investigate the effect of economic inequality between ethnic groups on trust. He finds that higher between-group inequality lowers generalized trust across US municipalities. Moreover, he documents that the negative effects of economic inequality and ethnic fractionalization on generalized trust turn insignificant once he accounts for between-group inequality. His contribution is probably the closest to our empirical part. There are, however, three important differences: First, we focus on towns and villages in Africa, while Tesei (2017) focuses on US municipalities, making these papers complementary. Second, we apply our index of ethnic stratification, which is based on ethnolinguistic distances between pairs of individuals, while he uses a measure of between-group inequality that treats ethnicity as a categorical variable. Third, our dependent variables measure trust in relatives, neighbors and other acquaintances, while he focuses on generalized trust.

Other contributions study how trust in specific groups of people is related to economic inequality and ethnic diversity. These trust measures are sometimes difficult to classify into generalized and particularized trust.[7] Putnam (2007) shows that individuals in ethnically heterogeneous neighborhoods in the United States have lower levels of trust in neighbors as well as in members of their own and other ethnic groups. Koopmans and Veit (2014) find that ethnic diversity in German neighborhoods made salient by experimental stimuli reduces trust in neighbors. Robinson (2020) focuses on the relation between ethnic diversity and trust in African countries. She finds that the gap between trust in co-ethnics and trust in members of other ethnic groups is greater in more diverse countries, but lower in more diverse regions within countries.

At a more general level, our empirical part contributes to the growing literature on the determinants of trust in Africa. Like Robinson (2020) and ourselves, most contributions to this literature rely on trust questions asked in Afrobarometer surveys. One strand documents the importance of the traditional structure of society and historical experiences for current trust. Nunn and Wantchekon (2011) study the effect of the trans-Atlantic slave trade on trust in neighbors and others. Gershman (2016) focuses on the effect of witchcraft on generalized trust and trust in members of other religions. Moscona et al. (2017) investigate the effect of an ethnic group's traditional reliance on segmentary lineage organization on the difference in trust towards relatives and non-family members. Another strand of the literature studies how current events shape trust. For example, Rohner et al. (2013) find that generalized trust deteriorates with conflict intensity in Uganda; and Sangnier and Zylberberg (2017) provide evidence that trust in political institutions decreases after protests.

[7] In our case, for example, trust in relatives is clearly particularized trust. The same holds true for trust in neighbors whom a respondent knows well and who belong to the same socio-economic and ethnolinguistic group. However, it is less obvious how to categorize trust in neighbors or other acquaintances who belong to different socio-economic and ethnolinguistic groups.

1.2.3 Literature on contact and conflict theory

Various scholars have postulated that diversity can lead to prejudice towards out-group members; a view that is often referred to as conflict theory. An often assumed mechanism is that with increasing diversity, individuals perceive out-groups as a greater threat to their own interests and as competitors over limited resources (e.g., Blumer, 1958; Blalock, 1967; Putnam, 2007). While earlier works mostly focus on the negative effects of diversity on prejudice towards out-groups, Putnam (2007) emphasizes that diversity can also lead to mistrust towards in-group members: He expects diversity to lead to a general decrease in trust (and even "social isolation"). In contrast, contact theory, founded by Allport (1954), states that intergroup contact can reduce prejudice towards other groups. Allport (1954) did not expect all intergroup contact to reduce prejduce, but only in situations in which certain conditions are met: namely, if the groups are of equal status, have common goals and cooperate, and their contact is supported by an authority, law or customs. Pettigrew and Tropp (2006), Pettigrew et al. (2011) and Paluck et al. (2019) review the literature in social psychology and social sciences that provides ample evidence in support of this hypothesis. However, as Pettigrew and Tropp (2006) and Pettigrew (2008) point out, most of this literature has focused on positive contact between groups, thereby ignoring the potential effects of negative encounters, such as reduced trust and increased conflict. Paolini et al. (2010) and Barlow et al. (2012) show that negative contact indeed makes the in-group/out-group distinction more salient and can increase intergroup conflict more than positive contact can reduce it, thereby providing evidence for conflict theory. Importantly, contact and conflict theory are not mutually exclusive: Given that Allport (1954) predicts a positive outcome from intergroup contact only if certain conditions are met, negative outcomes of intergroup contact are consistent with contact theory. As discussed in the introduction, our empirical findings indeed suggest that both theories have their merits.

1.3 Model

In this section, we first introduce our index of ethnic stratification. We then present a decomposition and an axiomatic characterization.

1.3.1 Definition of ethnic stratification

Consider a population constituted by a large set of individuals $P \subseteq \mathbb{R}_+$. Each individual in this population is associated with an ethnicity and a wealth level. We denote by $f(e, w)$ the density of individuals in the population that are associated with ethnicity $e \in E \subseteq \mathbb{R}_+$ and wealth level $w \in W \subseteq \mathbb{R}_+$, referring to $f : E \times W \to \mathbb{R}_+$ as the density function of the joint distribution of ethnicity and wealth.[8] We measure the distance between each pair of wealth levels $w, w' \in W$ by the absolute value of their difference, $|w - w'|$. Unlike wealth levels, ethnicities are not necessarily ordered in a meaningful way in E. To measure the distance between ethnicities, we assume that each ethnicity is associated with a language and denote by $\lambda(e, e') \in \mathbb{R}_+$ the distance between the languages of each pair of ethnicities $e, e' \in E$. We refer to $\lambda : E^2 \to \mathbb{R}_+$ as the linguistic function. In line with the idea of distance, we assume $\lambda(e, e) = 0$ and $\lambda(e, e') = \lambda(e', e)$ for each $e, e' \in E$.[9] In what follows, we assume that the relevant characteristics of the population are summarized by a pair of density and linguistic functions. Hence, each pair (f, λ) can be interpreted as a different population (or society).

Denoting by $e_i \in E$ and $w_i \in W$ the ethnicity and wealth level of each individual $i \in P$ and by $m > 0$ the size (or mass) of the population, our starting

[8]For a finite population, $f(e, w)$ represents the fraction of individuals associated with ethnicity $e \in E$ and wealth level $w \in W$.

[9]Another standard property of a distance is the triangle inequality, i.e., $\lambda(e, e') \leq \lambda(e, e'') + \lambda(e'', e')$ for each $e, e', e'' \in E$. We do not impose this property on λ as it is not necessarily satisfied by linguistic or, more generally, social distances. However, as the triangle inequality can be desirable in many contexts, our characterization is based on axioms in which the linguistic function satisfies the triangle inequality.

point is the general class of measures

$$\begin{aligned}\mathcal{M}(f,\lambda) :=& \frac{1}{m^2}\int_{i\in P}\int_{j\in P}\pi\left(\lambda(e_i,e_j),|w_i-w_j|\right)djdi\\ =& \int_{e\in E}\int_{e'\in E}\int_{w\in W}\int_{w'\in W} f(e,w)f(e',w')\pi\left(\lambda(e,e'),|w-w'|\right)dw'dwde'de,\end{aligned} \tag{1.1}$$

where $\pi:\mathbb{R}^2_+\to\mathbb{R}_+$ can be any function that is continuous and non-decreasing in each dimension satisfying $\pi(0,0)=0$ and $\pi(a,b)\neq 0$ for some $a,b>0$. We interpret π as a quantification of the degree of alienation (e.g., mistrust or lack of common interest) between two individuals as a function of their distances. Hence, any measure from class (1.1) can be interpreted as the *expected alienation* between a randomly selected pair of individuals. We refer to Rao (1982) and Bossert et al. (2011) for axiomatic characterizations of this broad class of measures where the alienation between two individuals is interpreted as a generic aggregation of their distances in multiple dimensions.

We are now ready to define our index of ethnic stratification. For each pair (f,λ), we measure the degree of *ethnic stratification* in the population by the index

$$\begin{aligned}S(f,\lambda) :=& \frac{1}{m^2}\int_{i\in P}\int_{j\in P}\lambda(e_i,e_j)|w_i-w_j|djdi\\ =& \int_{e\in E}\int_{e'\in E}\int_{w\in W}\int_{w'\in W} f(e,w)f(e',w')\lambda(e,e')|w-w'|dw'dwde'de.\end{aligned} \tag{1.2}$$

This index belongs to class (1.1) and coincides with the multiplicative form $\pi(a,b)=ab$ for each $a,b\geq 0$. If the wealth and ethnolinguistic distances between any two individuals take values in the unit interval,[10] then they can be interpreted as the probabilities that the poorer individual feels economically deprived vis-a-vis the richer one and that the two individuals do not share a common ethno-cultural background, respectively. Then, the index (1.2) can be interpreted as the probability that, for a randomly selected pair of individuals, *both* these events occur so that the poorer individual may feel unjustly deprived due to different economic

[10] This can be readily achieved by dividing these distances by the maximal distances within a superset of the population, e.g., the population of a single country as in our regression analysis.

opportunities across ethno-cultural backgrounds. Note that the interpretation of this joint event as a proxy of alienation is in line with conflict and contact theory at the same time.

1.3.2 Decomposition of ethnic stratification

We next show that our index of ethnic stratification depends on four different components. The first three components are common measures of average wealth, wealth inequality and ethnic diversity. These components capture the overall magnitude (or scale) of the wealth and ethnolinguistic distances across pairs of individuals. The fourth component instead captures the role played by the co-directionality of these distances.

We start by showing that our index of ethnic stratification nests common measures of wealth level, inequality and ethnic diversity. Letting f be any density function, the densities of the marginal distributions of ethnicity and wealth are $\varphi_f(e) := \int_{w \in W} f(e,w)dw$ and $\gamma_f(w) := \int_{e \in E} f(e,w)de$, respectively.[11] We consider three indices that summarize properties of the marginal distributions that are relevant for the decomposition of ethnic stratification: the *average wealth*

$$\mu(\gamma_f) := \frac{1}{m} \int_{i \in P} w_i di = \int_{w \in W} \gamma_f(w) w dw,$$

the *Gini coefficient* of inequality (in relative form) of the marginal distribution of wealth

$$\begin{aligned} G(\gamma_f) :=& \frac{1}{2\mu(\gamma_f) m^2} \int_{i \in P} \int_{j \in P} |w_i - w_j| dj di \\ =& \frac{1}{2\mu(\gamma_f)} \int_{w \in W} \int_{w' \in W} \gamma_f(w) \gamma_f(w') |w - w'| dw' dw, \end{aligned}$$

[11] For a finite population, $\varphi_f(e)$ represents the fraction of individuals associated with ethnicity $e \in E$ independently of their wealth level, while $\gamma_f(w)$ represents the fraction of individuals associated with wealth level $w \in W$ independently of their ethnicity.

and the coefficient of *fractionalization* of the marginal distribution of ethnicity[12]

$$F(\varphi_f, \lambda) := \frac{1}{m^2} \int_{i \in P} \int_{j \in P} \lambda(e_i, e_j) dj di = \int_{e \in} \int_{e' \in E} \varphi_f(e) \varphi_f(e') \lambda(e, e') de' de.$$

To see that these indices are nested in our model, note that if ethnolinguistic distances "didn't matter" in the sense that $\lambda(e, e') = 1$ for all $e, e' \in E$ (including $e = e'$), then the ethnic stratification of (f, λ) would be equal to (twice) the Gini coefficient in absolute form, i.e., $S(f, \lambda) = 2\mu(\gamma_f)G(\gamma_f)$. This identifies two scale effects related to the marginal distribution of wealth, as $\mu(\gamma_f)$ measures the average scale of wealth and $G(\gamma_f)$ the average scale of the wealth distances between all pairs of individuals relative to the average scale of wealth. Similarly, if wealth "didn't matter" in an analogous way, the ethnic stratification of (f, λ) would be equal to $F(\varphi_f, \lambda)$, which represents the average scale of the ethnolinguistic distances between all pairs of individuals, identifying the scale effect related to the marginal distribution of ethnicity.

In what follows, we show that ethnic stratification would be equal to (twice) the product of the three indices above, $2\mu(f)G(f)F(f, \lambda)$, if wealth and ethnicities were independently distributed across all individuals of society (f, λ). We then argue that the deviation of the "true" ethnic stratification $S(f, \lambda)$ from $2\mu(f)G(f)F(f, \lambda)$ captures the role played by the co-directionality of ethnolinguistic and wealth distances in shaping ethnic stratification in a society. To develop the argument formally, define the *benchmark* of density f as the density function $b_f : E \times W \to \mathbb{R}_+$ such that the marginal densities are identical to those of f and that ethnicity and wealth are independently distributed across individuals, i.e., $\varphi_{b_f}(e) = \varphi_f(e)$, $\gamma_{b_f}(w) = \gamma_f(w)$ and $b_f(e, w) = \varphi_{b_f}(e)\gamma_{b_f}(w)$. By extension, the benchmark of a pair (f, λ) is the pair (b_f, λ) constituted by the benchmark density of f and the same linguistic function λ. Figure 1.1 provides an example: The population (f, λ) is partitioned into three equally sized wealth classes that also correspond to three different ethnicities, while each ethnicity is proportionally represented within each wealth class in the corresponding benchmark (b_f, λ).

[12] This coefficient is the generalized fractionalization index proposed by Greenberg (1956) and axiomatically characterized by Bossert et al. (2011).

— Figure 1.1 about here —

Proposition 1. *For each pair* (f, λ), *the ethnic stratification of the benchmark* (b_f, λ) *is*

$$S(b_f, \lambda) = 2\mu(\gamma_f)G(\gamma_f)F(\varphi_f, \lambda). \tag{1.3}$$

Proposition 1 offers several insights for the decomposition of ethnic stratification in terms of scale and co-directionality effects. First, it shows that the ethnic stratification of the benchmark can be written as a function of the marginal densities γ_f and φ_f instead of f, providing a formal link between ethnic stratification and the three scale effects corresponding to the indices $\mu(\gamma_f)$, $G(\gamma_f)$ and $F(\varphi_f, \lambda)$. Second, in comparison to the ethnic stratification of the benchmark, the "true" ethnic stratification of (f, λ) additionally depends on a fourth component that captures the co-directionality of ethnolinguistic and wealth distances across pairs of individuals. Roughly speaking, we should expect

$$S(f, \lambda) > S(b_f, \lambda),$$

i.e., a positive residual indicating co-directionality, whenever high (low) wealth distances tend to go hand in hand with high (low) ethnolinguistic distances across pairs of individuals, but $S(f, \lambda) < S(b_f, \lambda)$, i.e., a negative residual indicating "reverse" co-directionality, whenever high (low) wealth distances tend to be associated with low (high) ethnolinguistic distances. To conclude, it follows from Proposition 1 that each of the three components $\mu(\gamma_f)$, $G(\gamma_f)$ and $F(\varphi_f, \lambda)$ captures a different scale effect, while the co-directionality effect is quantified by the deviation of the ethnic stratification of (f, λ) from the ethnic stratification of the benchmark (b_f, λ).[13]

[13]This deviation could be formalized in many alternative ways, e.g., the subtractive form $S(f, \lambda) - S(b_f, \lambda) \lesseqgtr 0$, the ratio form $S(f, \lambda)/S(b_f, \lambda) \lesseqgtr 1$, the logarithmic form $\ln[S(f, \lambda)/S(b_f, \lambda)] \lesseqgtr 0$, or the exponential form $\exp[S(f, \lambda) - S(b_f, \lambda)] \lesseqgtr 1$. Each of these formalizations would lead to a different decomposition of ethnic stratification into the three parts $\mu(\gamma_f)$, $G(\gamma_f)$ and $F(\varphi_f, \lambda)$ plus the corresponding version of co-directionality.

Remark 1. *For each pair (f, λ), the three components of*

$$S(b_f, \lambda) = 2\mu(\gamma_f)G(\gamma_f)F(\varphi_f, \lambda)$$

indicate different scale effects, while the comparison $S(f, \lambda) \lesseqgtr S(b_f, \lambda)$ indicates the co-directionality effect.

1.3.3 Axiomatic characterization of ethnic stratification

In what follows we show that a measure $M \in \mathcal{M}$ from class (1.1) satisfies a set of desirable properties (or axioms) if and only if it coincides with our index of ethnic stratification (1.2) up to positive scalar multiplication, i.e., $M = kS$ for some constant $k > 0$. We motivate our axioms as natural properties of an index of ethnic stratification that generalizes the idea of between-group inequality to situations where additional data on ethnolinguistic differences is available. Moreover, we believe these to be appealing properties of an index that aims to predict social tensions in line with the intuitions of conflict and contact theory. For simplicity, we state our axioms by means of examples based on degenerate joint distributions consisting of two or three mass points. These examples consist of comparative static exercises (or shifts) in the functions f and λ, and our axioms impose ethnic stratification to increase (or at least not to decrease) as a consequence of these shifts, all else equal. We consider three axioms in total, each focusing on a different trade-off between increasing (decreasing) the overall magnitude of wealth or ethnolinguistic distances and decreasing (increasing) their co-directionality.

Our first axiom, *co-directionality by wealth creation or transfer (CDbW)*, considers shifts in the density function induced by wealth creation or progressive (i.e., inequality-decreasing) wealth transfers within ethnicities such that the population becomes clustered into two groups whose members are perfectly homogeneous both in terms of ethnicity and wealth level. The intuition can be immediately grasped from Figures 1.2(a) and 1.2(b). In each of them the distribution on the right is obtained from the distribution on the left by a shift in the density function. In Figure 1.2(a) the poor of the dark group become rich by wealth creation, while in Figure 1.2(b) they become wealthier by a transfer of wealth from their rich co-ethnics. In both cases the population becomes partitioned into two perfectly homogeneous groups so that ethnic and economic divisions perfectly coincide. The axiom, which we are about to state formally, requires ethnic stratification to weakly increase as a consequence of such shifts due to the perfect ethnic homogeneity of the new economic classes. The idea that the population should become more conflictual as individuals of different ethnicities no longer share the same socio-economic status is supported by contact theory (and consistent with conflict theory).

— Figure 1.2 about here —

Axiom CDbW (co-directionality by wealth creation or transfer). *Data:* Let the pair (f, λ) be such that the population is partitioned into three equally sized sets $P_1, P_2, P_3 \subset P$. Suppose that all individuals in P_2 hold wealth level w_1 while all individuals in $P_1 \cup P_3$ hold wealth level w_2, where $w_1 > w_2$ so that $P_1 \cup P_3$ are the poor and P_2 are the rich. Moreover, assume that the population is partitioned into two ethnicities, labelled 1 and 2, speaking different languages, and that all individuals in P_1 belong to ethnicity 1 while all individuals in $P_2 \cup P_3$ belong to ethnicity 2. This description implies $f(1, w_2) = f(2, w_1) = f(2, w_2) = 1/3$ and $\lambda(1, 2) > 0$. *Statement:* For any $\epsilon \in [0, 1]$, we require $M(\tilde{f}_\epsilon, \lambda) \geq M(f, \lambda)$ for the density function $\tilde{f}_\epsilon$ that satisfies $\tilde{f}_\epsilon(1, w_2) = 1/3$ and $\tilde{f}_\epsilon(2, (1-\epsilon)w_1 + \epsilon(w_1 + w_2)/2) = 2/3$.

Let us discuss the shift from density f to density $\tilde{f}_\epsilon$ in more detail. Axiom CDbW requires that ethnic stratification should not decrease when wealth is created by P_3 (i.e., the poor of ethnicity 2) or transferred from P_2 to P_3 (i.e., from the rich to the poor of ethnicity 2) to the point that all individuals in $P_2 \cup P_3$ (i.e., all members of ethnicity 2) come to hold the new wealth level $(1-\epsilon)w_1+\epsilon(w_1+w_2)/2$, which is above w_1 (i.e., the wealth level of all members of ethnicity 1), where $\epsilon \in [0, 1]$ denotes the fraction of wealth that is transferred from P_2 to P_3 while the remaining fraction $(1-\epsilon)$ is created by P_3.[14] As a result, for each ϵ the population becomes clustered in two ethnically homogeneous economic classes, P_1 and

[14]Let us discuss Axiom CDbW in relation to standard principles in inequality measurement (see, e.g., Cowell, 2011 for a review). Let us start with the Principle of Transfers (stating that

$P_2 \cup P_3$, leading to a weakly higher average wealth differential between the two groups. Axiom CDbW implicitly assumes that these changes fuel ethnic tensions and requires that they weakly increase ethnic stratification.

Our second axiom, *bi-polarization by wealth transfer (BPbW)*, considers shifts in the density function induced by regressive (i.e., inequality-increasing) transfers of wealth such that the wealth distribution becomes polarized into two opposite economic classes while (possibly) altering the co-directionality of wealth and ethnolinguistic distances. The message can be easily discerned from Figures 1.3(a) and 1.3(b), where in each of them the distribution on the right is obtained from the distribution on the left by a shift in the density function. In both, the disappearance of the middle class follows from the same regressive transfer within itself but has different consequences on within-group homogeneity. In Figure 1.3(a), the half of the middle class that gets richer belongs to the dark gray group (which is already richer on average) while the half that gets poorer belongs to the light gray group (which is already poorer on average), so that the rise in inequality strengthens economic homogeneity within ethnic groups. In Figure 1.3(b) on the other hand, the middle class is constituted by a third ethnic group in mid gray (thus linguistically in between the other two), so that the rise in inequality partially blurs the ethnic homogeneity within economic classes. In both cases, the average wealth differential between ethnically diverse individuals is weakly higher than before. We think of ethnic stratification as closely related to such wealth differ-

inequality should decrease when wealth is transferred from the rich to the poor). Axiom CDbW relaxes this principle when such transfers occur within an ethnic group. This violation of the Anonymity Axiom (stating that the identity of individuals should not affect the measurement of inequality) is standard in between-group inequality measurement as only wealth differentials between members of different groups should be taken into account. Notice further that the kind of wealth creation considered in Axiom CDbW (which weakly increases ethnic stratification) can lead to decreases in standard inequality measures. For instance, note that average wealth appears in the denominator of the Gini coefficient in relative form, as shown in Section 1.3.2. For this reason we believe that ethnic stratification should be seen as an inequality measure in absolute form, as substantiated by our decomposition in Section 1.3.2, which accounts both for the Gini coefficient in relative form and the average wealth level as separate components (which jointly account for the Gini coefficient in absolute form). Moreover, note that our index increases linearly in the scale of wealth (i.e., by multiplying all wealth levels by the same positive constant). It is therefore scale invariant only in the sense of not altering the ranking of populations and not altering their ethnic stratification levels up to a common positive scalar multiplication, as standard inequality measures in absolute form.

entials, and contact theory also suggests that such wealth differentials are a core determinant of ethnic tensions. Therefore, the axiom requires ethnic stratification to weakly increase as a consequence of such shifts in the density function.

— Figure 1.3 about here —

Axiom BPbW (bi-polarization by wealth transfer). *Data:* Let the pair (f_ϵ, λ) be such that the population is partitioned into three equally sized sets $P_1, P_2, P_3 \subset P$ divided into three ethnicities, labelled 1, 2 and 3, such that all individuals in P_1 belong to ethnicity 1, all individuals in P_3 belong to ethnicity 3, and a fraction $\epsilon \in [0,1]$ of the individuals in P_2 belong to ethnicity 2 while the remaining fraction $(1-\epsilon)$ is equally split between ethnicity 1 and ethnicity 3. Assume that the wealth level is homogeneous within each set P_1, P_2, P_3, and denote by $w_1 > w_2 > w_3$ the respective wealth levels. Hence, we can refer to P_1, P_2 and P_3 as the rich, the middle class and the poor. This description implies $f_\epsilon(1, w_1) = f_\epsilon(3, w_3) = 1/3$, $f_\epsilon(2, w_2) = \epsilon/3$ and $f_\epsilon(1, w_2) = f_\epsilon(3, w_2) = (1-\epsilon)/6$ for each $\epsilon \in [0,1]$. In addition, suppose that the wealth of the middle class is the average of those of the rich and the poor and that the language of ethnicity 2 is a balanced mixture of the languages of ethnicities 1 and 3, implying $w_2 = (w_1 + w_3)/2$ and $\lambda(1,2) = \lambda(2,3) = \lambda(1,3)/2 > 0$. *Statement:* For any $\epsilon \in [0,1]$, we require $M(\tilde{f}_\epsilon, \lambda) \geq M(f_\epsilon, \lambda)$ for the density function $\tilde{f}_\epsilon$ that satisfies $\tilde{f}_\epsilon(1, w_1) = \tilde{f}_\epsilon(3, w_3) = (3-\epsilon)/6$ and $\tilde{f}_\epsilon(2, w_1) = \tilde{f}_\epsilon(2, w_3) = \epsilon/6$.

Let us discuss the shift from density f_ϵ to density $\tilde{f}_\epsilon$ in more detail. Axiom BPbW requires that ethnic stratification should not decrease when half of the middle class becomes poor (with their wealth level going from w_2 down to w_3) while the other half becomes rich (with their wealth level going from w_2 up to w_1) due to regressive transfers of wealth from the former to the latter. As a result, the middle class disappears and economic inequality unquestionably surges, leading to economic bi-polarization. However, for each ϵ this surge in inequality weakly blurs the pattern of ethno-economic clustering, as there is now a mass $(3-\epsilon)/6$ of members of ethnicity 2 within the poor and the rich economic classes (which were originally constituted only by members of ethnicity 1 and ethnicity

3, respectively). Despite this, note that for each ϵ the average wealth differential between ethnic groups weakly rises due to the growth in wealth distances between the ex-middle class members of ethnicities 1 and 3. Axiom BPbW requires that ethnic stratification weakly increases in response to this weak increase in the average wealth differential. This requirement can be interpreted as a counter-force to Axiom CDbW, which considers the same trade-off in the opposite direction, guaranteeing that our framework is sensitive to within-group homogeneity but still (and mainly) about the measurement of economic inequality.

Our third axiom, *co-directionality by linguistic change (CDbL)*, considers shifts in the linguistic function that increase the co-directionality of wealth and ethnolinguistic distances by altering the relative ethnolinguistic distances between ethnic groups while leaving their overall magnitude unchanged. Figure 1.4, where the distribution on the right is obtained from the distribution on the left by a shift in the linguistic function, easily conveys the intuition. There are three economic classes that coincide with three different ethnic groups. The middle class (in mid gray) speaks a language exactly in between those of the rich (in dark gray) and the poor (in light gray) and holds a wealth level that is closer to that of the rich than to that of the poor. The axiom, which we are about to state formally, requires ethnic stratification to weakly increase when the language of the middle class shifts closer to that of the rich and farther from that of the poor, thus strengthening the co-directionality of economic and linguistic distances. In line with the insights of contact theory, this should weakly increase ethnic tensions, as individuals with higher wealth differentials diverge further along the ethnolinguistic dimension.

— Figure 1.4 about here —

Axiom CDbL (co-directionality by linguistic change). *Data:* Let the pair (f, λ) be such that the population is partitioned into three equally sized sets $P_1, P_2, P_3 \subset P$ with homogeneous wealth level and ethnicity within each set. Denote by $w_1 > w_2 > w_3$ the respective wealth levels, so that we can again refer to P_1, P_2 and P_3 as the rich, the middle class, and the poor. Then, letting 1, 2 and 3 indicate the ethnicities of the sets P_1, P_2 and P_3, we can write $f(1, w_1) = f(2, w_2) = f(3, w_3) = 1/3$. In addition, suppose that the language of ethnicity 2 is a mix of the languages of ethnicities 1 and 3, implying $\lambda(1,3) = \lambda(1,2) + \lambda(2,3) > 0$. Importantly, let the wealth level of the middle class be closer to that of the rich than to that of the poor, so that $w_2 > (w_1 + w_3)/2$. *Statement:* For each $\epsilon \in (0, \lambda(1,2)]$, we require $M(f, \tilde{\lambda}_\epsilon) \geq M(f, \lambda)$ for the linguistic function $\tilde{\lambda}_\epsilon$, which is identical to λ except that $\tilde{\lambda}_\epsilon(1,2) = \lambda(1,2) - \epsilon$ and $\tilde{\lambda}_\epsilon(2,3) = \lambda(2,3) + \epsilon$.

Let us discuss the shift in the linguistic function from λ to $\tilde{\lambda}_\epsilon$ in more detail. Axiom CDbL requires that a change in the language of the middle class (ethnicity 2) which brings it ϵ-closer to that of the rich (ethnicity 1) and ϵ-farther from that of the poor (ethnicity 3), should not lead to a decrease in ethnic stratification. This should hold for any ϵ such that the language of the middle class remains in between the other two languages, i.e., any $\epsilon \in (0, \lambda(1,2)]$. As by assumption the wealth level of the middle class (w_2) is closer to that of the rich (w_1) than to that of the poor (w_3), this linguistic change increases the co-directionality between the wealth distances $|w_i - w_j|$ and the ethnolinguistic distances $\lambda(e_i, e_j)$ across all pairs of individuals i, j on average. Then, while the overall magnitude of such distances is unchanged (as wealth distances are fixed, while for each ϵ-increase there is a corresponding ϵ-decrease in linguistic distances), the "effective" wealth differential between ethnic groups increases due to the higher alignment of distances across the economic and ethnolinguistic dimensions. As previously mentioned, we believe such a wealth differential to be at the core of the idea of ethnic stratification and a significant predictor of ethnic tensions.

We are now ready to state our characterization result:

Theorem 1. *A measure $M \in \mathcal{M}$ satisfies axioms CDbW, BPbW, CDbL if and only if it coincides with (1.2) up to positive scalar multiplication.*

To further clarify the role of each axiom, we now sketch the "only if" part of the proof of Theorem 1.[15] As our premise is any measure from class $\mathcal{M}$, the proof focuses on showing that the three axioms jointly imply $\pi(a,b) = kab$ for some $k > 0$. To start with, we show that the combination of CDbW and BPbW implies

[15]The "if" part is trivial as it is straightforward that $M = kS$ satisfies the axioms for each $k > 0$.

$\pi(a, 0) = \pi(0, b) = 0$ for each $a, b > 0$, meaning that the wealth and ethnolinguistic distances between two individuals are counted only if the individuals differ in both dimensions. As argued earlier, we believe this high degree of complementarity is a defining feature of ethnic stratification as a generalization of between-group inequality, and a core insight of conflict and contact theory to predict ethnic tensions. Given this, we proceed by showing that CDbW additionally requires $\pi(a, b)$ to be weakly concave in b, while BPbW additionally demands it to be weakly convex in b, so that $\pi(a, b)$ must be linear in b by the combination of these two axioms.[16] Together with our finding $\pi(a, 0) = \pi(0, b) = 0$ for each $a, b > 0$, this implies that $\pi(a, b) = \rho(a)b$ for some non-decreasing function $\rho : \mathbb{R}_+ \to \mathbb{R}_+$ that satisfies $\rho(0) = 0$ and $\rho(a) > 0$ for some $a > 0$. Finally, our proof concludes by showing that by CDbL the function ρ is linear, so that $\rho(a) = ka$ for some $k > 0$.

1.4 Computing ethnic stratification

In this section, we first describe how to construct the economic and ethnolinguistic distances between pairs of individuals using data from Afrobarometer surveys. We then show that our index of ethnic stratification can be interpreted as a parameter that can be estimated from data, which we illustrate with a cross-country comparison.

1.4.1 Constructing economic and ethnolinguistic distances

The main data we use comes from Afrobarometer surveys. These surveys aim to be representative of all citizens of voting age in a given country and year. For that purpose, the samples are stratified according to the main subnational administrative units and by urban or rural locations. Importantly, the samples within the primary sampling units, which we later call towns and villages, are randomly drawn from the local population (Afrobarometer, 2018).

[16]These conclusions are drawn by considering the implications of CDbW and BPbW at the extreme cases with $\epsilon = 1$, as depicted in Figure 1.2(b) and Figure 1.3(b).

The use of Afrobarometer data has several advantages. First, these surveys contain information on the respondents' wealth. This information, which is considerably richer from survey round 5 onward, allows us to create measures of individual economic wealth and, consequently, economic distances between pairs of respondents. Second, the Afrobarometer surveys contain information on the respondents' native language and their ethnic affiliation. After matching this information to the languages in the Ethnologue (Gordon Jr., 2005), we can use the Ethnologue's language trees to compute ethnolinguistic distances between pairs of individuals. Third, the Afrobarometer data has recently been geocoded by AidData (BenYishay et al., 2017). Given the geo-coordinates of the survey locations, plus the economic and ethnolinguistic distances between pairs of individuals, we can compute ethnic stratification at the level of various spatial units, including countries or towns and villages. Fourth, the Afrobarometer surveys, in particular round 5, include questions on trust in relatives, neighbors and other acquaintances, which allows us to study the relation between local ethnic stratification and the respondents' trust in people they know.

To compute economic distances, we first construct a wealth index similar to the one constructed in the Demographic and Health Surveys (DHS). We thereby use the information on the assets that the respondents and their families possess and the quality of the house in which they live. Afrobarometer surveys of round 5 contain information on the possession of a radio, television, mobile phone, and a motorcycle or car, and on the types of water source, toilet, house and roof (see Appendix 1.C.1 for details). We then follow DHS in creating a wealth index consisting of the first principal component of the asset and housing variables, and we construct the wealth index for each country separately and later normalize them to take a value between zero and one.[17] We use the resulting wealth indices as a measure of individual economic wealth and the absolute difference between

[17] The first principal component is calculated by conducting a principal component analysis. This method is used to reduce the large number of asset and housing variables to linear combinations, which can be interpreted more easily. The first principal component has the largest variance of all linear combinations and thus accounts for the largest part of the variation in the data (Jolliffe, 2002). For robustness tests that include multiple survey rounds, we construct the wealth index separately for each survey, i.e., each country and survey round. We do not construct separate wealth indices for urban and rural areas, because the asset and housing variables on which our index is based appear relevant in both urban and rural settings.

the index values of a pair of individuals as their economic distance.

In addition, Afrobarometer surveys provide information on the respondents' native language and their ethnic affiliation.[18] To compute the ethnolinguistic distances between respondents, we match the language and ethnicity information from the Afrobarometer to the languages in the Ethnologue. The Ethnologue provides the most complete classification of world languages. It lists 7,097 known living languages, of which 2,143 are in Africa. The Ethnologue's data are modelled as trees that show the historical relation between all languages.

Many languages and ethnic groups used in the Afrobarometer surveys do not match the names of the languages in the Ethnologue. We match them manually using information from the Ethnologue website,[19] which contains information on alternative names and dialects, and sometimes also on the ethnic groups that speak a certain language. In cases in which a language or ethnic group from Afrobarometer was not found on the Ethnologue website, we use the following sources to match the information from Afrobarometer to the Ethnologue: Eldredge (2015), Falola and Jean-Jacques (2015), Futhwa (2012), Hall (1999), Olson (1996), Otlogetswe (2011), and the Joshua Project.[20] For Afrobarometer surveys of round 5, we successfully match 727 Afrobarometer languages to 560 Ethnologue languages, and 677 Afrobarometer ethnicities to 502 Ethnologue languages. We are unable to match 14 (19) languages (ethnicities) of the possible answer categories in the Afrobarometer surveys of round 5 to any Ethnologue language. For some respondents, we cannot match the language or ethnicity information from Afrobarometer to the Ethnologue because the corresponding question was not asked or not answered.[21] In addition, we treat European languages (i.e., English, French, German and Portuguese) as missing. For all these reasons and in order

[18] The specific questions are: "Which language is your home language?" and "What is your ethnic community, cultural group or tribe?"

[19] https://www.ethnologue.com

[20] https://www.joshuaproject.net

[21] The Afrobarometer data also contain responses that do not fit to any of the Afrobarometer's answer categories. Of these additional responses, we successfully match 296 (213) languages (ethnicities), which contribute to the total of 727 (677) languages (ethnicities) matched. We cannot match 86 (226) languages (ethnicities) of these additional responses to any Ethnologue language listed in the respondent's country. We additionally use Afrobarometer surveys of rounds 3, 4 and 6 in robustness tests. Aggregated over rounds 3–6, we are able to match 1,211 (1,167) Afrobarometer languages (ethnicities) to 714 (661) Ethnologue languages.

to base our indices on as many respondents as possible, we combine the respondents' information on language and ethnicity, and compute two different measures of ethnolinguistic distance between pairs of individuals: one based primarily on the Afrobarometer language (with the Afrobarometer ethnicity used only if the Afrobarometer language could not be matched to any Ethnologue language), and one based primarily on the Afrobarometer ethnicity (with the Afrobarometer language used only if the Afrobarometer ethnicity could not be matched to any Ethnologue language). We get 578 distinct ethnolinguistic groups according to the Ethnologue's classification when relying primarily on Afrobarometer languages, and 545 groups when relying primarily on Afrobarometer ethnicities.[22] For our main analysis, we use the coding that relies primarily on Afrobarometer languages, as all surveys ask about the respondents' native language whereas some do not ask about their ethnic affiliation.

Language trees depict the historical relations between languages, as languages that share more branches have a longer common history. Therefore, many scholars have used language trees to calculate the distance between any two ethnolinguistic groups (e.g., Desmet et al., 2009; Fearon, 2003). Here, we use the formula proposed by Putterman and Weil (2010) to compute ethnolinguistic distances:

$$\lambda(e_i, e_j) = 1 - \sqrt{\frac{2t_{ij}}{T_i + T_j}},$$

where T_i and T_j are the number of nodes in the branch of the languages of the ethnolinguistic groups e_i and e_j, respectively, and t_{ij} the number of common nodes.

1.4.2 Ethnic stratification as a parameter

Suppose our dataset consists of an i.i.d. sample of size $n \geq 2$, $\{(E_i, W_i)\}_{i=1}^n$, where (E_i, W_i) are ethnicity and wealth levels of individual i that have the same distribution as (E, W). For a given λ, we can equivalently re-write ethnic stratification

[22] Aggregated over rounds 3–6, the number of distinct ethnolinguistic groups is even 731 (702) when relying primarily on Afrobarometer languages (ethnicities).

from equation (1.2) as a population parameter:

$$\theta := \mathbb{E}\left[\lambda\left(E, E'\right)\left|W - W'\right|\right].$$

where (E, W) and (E', W') denote two generic independent pairs of random variables that are drawn from some density underlying f.

The most natural way to estimate θ is to use its sample counterpart, where an expectation is replaced by a sample average. In particular, using the Law of Iterated Expectation, we see that $\theta = \mathbb{E}\left[\mathbb{E}\left[\lambda\left(E, E'\right)\left|W - W'\right| \mid E, W\right]\right]$. We thus propose the following estimator for θ:

$$\theta_n := \frac{1}{n^2}\sum_{i=1}^{n}\sum_{j=1}^{n}\lambda\left(E_i, E_j\right)\left|W_i - W_j\right|. \tag{1.4}$$

A measure of ethnic stratification can therefore be computed in a similar way to other related indices in the literature (e.g., Gini, fractionalization, polarization).

One use of the above index is to compare ethnic stratification across samples. We show in Appendix 1.B that θ_n is a consistent estimator of θ and has an asymptotic normal distribution. Furthermore, the asymptotic variance of θ_n can be easily estimated so that one can construct confidence intervals and perform inference. As an illustration, we plot the ethnic stratification estimates for African countries and their corresponding confidence intervals in Figure 1.5.

To generate Figure 1.5, we use all respondents from Afrobarometer surveys of round 5 who answered the questions used to construct the wealth index and whom we could assign to an Ethnologue language group. We exclude countries where more than 95 percent of the respondents belong to the largest ethnolinguistic group.[23] We are left with 26 ethnolinguistically diverse African countries. We present the estimates of these countries' ethnic stratification, arranged from highest to lowest.

— Figure 1.5 about here —

[23] We discuss our sample selection in more detail in Section 1.5. Note that the respondents that we exclude here are a subset of the respondents that we exclude in Section 1.5, where we focus on the local level and need the respondents' answers on the trust questions of interest.

1.5 Estimating the effect of ethnic stratification on trust

In this section, we compute ethnic stratification at the level of African towns and villages according to (1.4) and use it as an explanatory variable and estimate its relation to trust in relatives, neighbors and other people the respondent knows. We first discuss the construction of the dataset and our empirical specification. We then present our main results, many robustness tests, a comparison of our index with other indices of diversity and inequality, as well as results for crime and violent conflict.

1.5.1 Our dataset

We base our main analysis on Afrobarometer surveys of round 5. There are two main reasons for this. First, none of the other rounds contains all of the three trust questions we are interested in. Questions on trust in relatives are asked in rounds 3–5, questions on trust in neighbors in rounds 3 and 5, and questions on trust in other people the respondent knows in rounds 4 and 5. Round 6 contains none of these questions. Second, the definition of our index of ethnic stratification in equation (1.2) makes clear that good proxies for economic/wealth distances between pairs of individuals are crucial for the computation of this index; and round 5 (and round 6) contain considerably richer information on individual wealth, in particular on the quality of housing, than rounds 3 and 4 (see Appendix 1.C.1 for details).

For our main analysis, we compute our index of ethnic stratification at the level of survey areas such as town or villages. We focus on ethnic stratification at such a granular level, because we are interested in its relation to trust in people the respondents know. We use the wealth and ethnolinguistic distances between pairs of individuals (see Section 1.4.1) to compute our index of ethnic stratification S_{vc} at the level of each town or village v of country c. In addition, we can also compute the average wealth (μ_{vc}), the Gini coefficient (G_{vc}) and the index of ethnic fractionalization (F_{vc}). They are all nested in the ethnic stratification index (see Section 1.3.2), such that their computation requires no additional information.

To construct our main dependent variables, we focus on the following three trust questions in the Afrobarometer surveys: "How much do you trust each of the following types of people: Your relatives? Your neighbors? Other people you know?" Following Rohner et al. (2013), we build indicator variables that equal one if the respondent answers "a lot" or "somewhat," and zero if she answers "just a little" or "not at all."

We also use alternative dependent variables measuring crime and conflict. For crime, we use two binary measures based on questions in the Afrobarometer surveys. The first measure, which we call "fear of crime," indicates whether the respondent (or a family member) had feared crime within their own home during the past year. The second, which we call "actual crime," indicates whether the respondent (or a family member) had been physically attacked or something had been stolen from their home during the past year. For measuring conflict at the local level, we use data from the Armed Conflict Location and Event Dataset (ACLED) introduced by Raleigh et al. (2010). We proxy for conflict with a binary variable that indicates whether there would be at least one violent conflict event within 10 km of the center of the town or village within three years from the date of the interviews.[24] This conflict variable is available at the level of towns and villages rather than individual respondents. (We postpone the discussion of the implications that this change in the units of observation has for our empirical strategy to Section 1.5.6.)

For our analysis, we restrict the sample along three dimensions. First, as in Section 1.4, we exclude countries where more than 95 percent of the respondents belong to the same ethnolinguistic group according to our data. These countries are Burundi, Cape Verde, Egypt, Lesotho, Mauritius, Sudan, Swaziland and Tunisia.[25] Second, we restrict our attention to interviews conducted in locations

[24] ACLED records reported events of political violence across Africa (and other regions of the world). Among other information, ACLED provides the date, the location and the type of each event. We classify the following types as violent events: "battles," "violence against civilians," and "explosions/remote violence."

[25] Respondents in Burundi indicate different ethnicities (Hutu, Tutswi, Twa), which are however not distinct languages in the Ethnologue; and more than 99 percent indicate Rundi as their

that AidData (BenYishay et al., 2017) classified as “populated places” such as towns or villages (and whose coordinates correspond to an exact location), as we want to be sure that the various respondents from a cluster were living in close proximity.[26] Third, we exclude respondents who answered none of the trust questions we are using, respondents who did not answer some of the questions used to construct the wealth index, respondents to whom we could not assign an Ethnologue language, and respondents for whom information on age, education or religion (which we use as control variables) is missing. In addition, we exclude the few towns and villages where the wealth index and the Ethnologue language are available for fewer than three respondents, because our index of ethnic stratification is not defined in case of a single respondent and would depend on just one pair of individuals in case of two respondents.

Our final sample consists of 21,379 respondents from 2,558 towns and villages across 1,147 districts (ADM2 regions) in 371 provinces (ADM1 regions) of 26 ethnolinguistically diverse African countries. 54% of these towns and villages have exactly eight respondents, and the average town or village has 8.50 respondents. Figure 1.6 depicts the towns and villages in our final sample. In addition, it provides some information on local ethnic stratification, with darker dots indicating higher values.

— Figure 1.6 about here —

native language. Respondents in Cape Verde indicate ethnicities that cannot be matched to the Ethnologue (as they are related to, e.g., age or class), and 100 percent indicate Kabuverdianu as their native language. Respondents in Egypt, Sudan and Tunisia were not asked about their ethnicity, and more than 99 percent indicate Arabic as their native language. Most respondents in Mauritius indicate ethnicities that cannot be matched to the Ethnologue (e.g., their religion), and more than 96 percent indicate Creole as their native language. More than 97 percent of the respondents in Lesotho indicate Southern Sotho as both their native language and their ethnicity. Respondents in Swaziland were not asked about their ethnicity, and more than 98 percent indicate Swati as their native language.

[26]There are three other categories: “Administrative regions,” “structures” (e.g., schools or health clinics), and “other topographical features” (e.g., mountains, rivers or forests). We exclude clusters coded as “administrative regions” or “other topographical features,” because they are less geographically precise, as confirmed by the precision code in the data. We exclude locations coded as “structures,” because schools or health clinics might serve as central meeting points to conduct interviews with people from different villages. Results remain qualitatively unchanged and quantitatively very similar if we include the relatively few respondents from “structures.”

Table 1.1 reports summary statistics. Out of every ten respondents, around eight trust their relatives, around six their neighbors, and around four other acquaintances. Similarly, seven out of every ten respondents were afraid of crime in the previous year, and three experienced crime. The correlation coefficients between any two of the three trust measures range from 0.30 to 0.54, and the correlation coefficient between the two crime measures is 0.28.

— Table 1.1 about here —

Figure 1.7 presents scatter plots illustrating how ethnic stratification relates to the Gini coefficient and ethnic fractionalization at the local level. The top row does so for all the towns and villages in our sample, and the bottom row for the towns and villages in Nigeria only. These scatter plots show that the towns and villages differ considerably in terms of ethnic stratification even after accounting for the Gini coefficient or ethnic fractionalization; and that ethnic stratification is more strongly related to ethnic fractionalization than the Gini coefficient.[27]

— Figure 1.7 about here —

The Nigerian village with the highest level of ethnic stratification (0.19) in our sample is Tsokundi in Taraba state. Ethnic fractionalization is high (0.62) as the 13 respondents from this village belong to seven different ethnolinguistic groups, with eight respondents speaking languages of the Niger-Congo language family and five respondents languages of the Afroasiatic language family. The Gini coefficient is high (0.24) as five respondents belong to the bottom-10% of the Nigerian wealth distribution (according to our wealth index), while two respondents belong to the top-10%. Ethnic stratification is high mainly because ethnic fractionalization and wealth inequality are high. Moreover, economic and

[27]These observations are consistent with the results of the Shapley-Owen decomposition in Appendix 1.C.2. When we estimate a linear model using ethnic stratification as the dependent variable, this decomposition of our index gives the contribution that each regressor has on the R^2. It shows that the country fixed effects contribute 5.8%, ethnic fractionalization 50.5%, the Gini coefficient 1.5%, average wealth 1.9%, and the co-directionality of economic and ethnolinguistic distances across pairs of individuals the remaining 40.3%. This substantial contribution by the co-directionality lends further support to our index of ethnic stratification, which differs from other measures of diversity and inequality by explicitly taking co-directionality into account.

ethnolinguistic distances are relatively co-directional, with the five respondents from the bottom-10% of the wealth distribution belonging to different ethnolinguistic groups than the two respondents from the top-10%. As we would expect, the respondents from Tsokundi are not very trusting: six of the 13 respondents trust their relatives, one trusts their neighbors and not a single one trusts other people they know.

The bottom left scatter plot of Figure 1.7 shows that there are two villages with zero ethnic stratification despite having a slightly larger Gini coefficient than Tsokundi. These two villages are ethnolinguistically homogenous. Similarly, the bottom right scatter plot shows that there are two villages with much lower ethnic stratification despite having slightly higher ethnic fractionalization. These two villages have considerably lower wealth inequality than Tsokundi. Interestingly, the respondents from each of these four villages are on average more trusting towards relatives, neighbors and others than the respondents from Tsokundi (except one village where again no respondent trusts others). We next investigate more systematically whether such a negative relation between ethnic stratification and trust holds at a more general level.

1.5.2 Empirical specification

We use the following two specifications to investigate the relation between local ethnic stratification and trust:

$$Trust_{ivce} = \alpha_{ce} + \beta S_{vc} + \theta X_{ivce} + \lambda Q_{vc} + \epsilon_{ivce} \tag{1.5}$$

$$Trust_{ivce} = \alpha_{ce} + \beta S_{vc} + \gamma F_{vc} + \delta G_{vc} + \psi \mu_{vc} + \theta X_{ivce} + \lambda Q_{vc} + \epsilon_{ivce} \tag{1.6}$$

where $Trust_{ivce}$ is one of our three trust indicators for respondent i living in town or village v of country c and belonging to ethnolinguistic group e. The interacted country-ethnolinguistic group fixed effects α_{ce} (henceforth simply country-group fixed effects) control for all country-specific determinants and experiences that may affect trust as well as any group-specific characteristics or experiences. In addition, they allow for the fact that some ethnolinguistic groups are present in multiple countries and play different roles in different countries.[28] To address potential omitted variable bias, we further include individual and geographical control variables. The vector of individual control variables X_{ivce} contains respondent i's economic wealth (measured by our wealth index), her age and age squared as well as indicator variables for her gender, her religion (Christian/ Muslim/ other), her education (none/ primary/ secondary/ tertiary) and whether she lives in an urban or rural area. The vector of geographical control variables Q_{vc} includes soil suitability for agriculture, malaria suitability, average precipitation, altitude, terrain ruggedness, distance to the coast, population, and a measure of past conflict events.[29]

The main coefficient of interest is β, which measures the effect of local ethnic stratification (S_{vc}) on the respondents' trust. We expect β to be negative in both specifications. In specification (1.5), $\beta < 0$ implies a negative relation between ethnic stratification and trust (conditional on all the fixed effects and control variables). This specification, however, provides no information about whether the negative relation is driven by ethnic diversity, economic inequality or, indeed, the interaction of ethnolinguistic and economic distances at the level of pairs of individuals. In specification (1.6), we therefore control for ethnic fractionalization (F_{vc}), the Gini coefficient (G_{vc}) and average wealth (μ_{vc}). Hence, in this specification, $\beta < 0$ implies that the negative relation between ethnic stratification and trust is driven by the interaction of ethnolinguistic and economic distances (as a negative relation can no longer result from a direct effect of F_{vc}, G_{vc} or μ_{vc}).[30]

Furthermore, specification (1.6) is helpful to shed light on the merits of conflict and contact theory. In particular, it allows us to investigate whether ethnolinguistic distances between pairs of individuals are predictive for mistrust in general (as captured by F_{vc}) or only if they go hand-in-hand with economic distances (as captured by S_{vc}).

[28]Country-group fixed effects or, at least, country fixed effects are also important because we calculate our wealth index and the economic distances for each country/survey separately.

[29]The geographical control variables are computed for circles with a radius of 10 km around the locations' geo-coordinates provided by BenYishay et al. (2017). Appendix 1.C.3 provides more information.

[30]In support of this interpretation, we further control for benchmark stratification (as defined in Proposition 1) in some robustness tests.

We estimate all specifications using linear probability models. We use multiway clustering and cluster the standard errors ϵ_{ivce} at the level of country-ethnolinguistic group interactions and provinces (ADM1 regions).

1.5.3 Main results

Table 1.2 presents our main results. The outcome variables are our indicators for trust in relatives in columns (1)–(2), trust in neighbors in columns (3)–(4), and trust in others in columns (5)–(6). The odd columns of Panels A and B present the results for specifications (1.5) and (1.6), respectively. Panel C will be helpful to discuss the merits of conflict and contact theory. The even columns include province fixed effects and therefore serve as the first robustness test.

— Table 1.2 about here —

Starting with Panel A, we see that the estimated coefficient on ethnic stratification is negative and statistically significant at the 5%-level in all six regressions. Hence, the general pattern is clear: high local ethnic stratification coincides with low trust in people that a respondent knows. The estimates in the odd columns imply that an increase in ethnic stratification by one standard deviation coincides with a reduction in the probability that a respondent trusts her relatives, neighbors and others by 1.4, 2.5 and to 2.1 percentage points, respectively. It is worth noticing that the reduction in trust in relatives is smaller than the reduction in trust in neighbors and others. Possible reasons are that relatives may be more likely than neighbors and other acquaintances to belong to the same ethnolinguistic group as the respondent; and that respondents may have more private information about relatives, such that trust in relatives depends to a lesser degree on town- and village-level characteristics such as ethnic stratification.

Panel B shows that ethnic stratification remains negatively related to trust after we control for ethnic fractionalization, the Gini coefficient, and average wealth. The estimated coefficients become even larger in absolute values for all trust variables and specifications. The relative statistical significance of ethnic stratification compared to ethnic fractionalization and the Gini coefficient highlights the prominent role that ethnic stratification plays in explaining mistrust at the local level.

In order to understand the role of ethnic stratification better, we re-estimate the regressions used to generate Panel B after removing ethnic stratification. These results are reported in Panel C. We still find almost no relation between the Gini coefficient and trust. The estimated coefficient on average wealth is again negative and statistically significant. This negative relation suggests that individuals are more trusting in poorer towns or villages.[31] More interestingly, the estimated coefficients on ethnic fractionalization have all turned negative compared to the all positive estimates found in Panel B. Even though these effects tend to be weak in terms of statistical significance, the emerging pattern lends itself to the following interpretation: The negative coefficients on ethnic fractionalization in Panel C show that ethnic diversity does typically coincide with low trust. The results in Panel B however show that ethnolinguistic distances between individuals are only predictive of mistrust if the ethnolinguistic distances go hand-in-hand with economic distances. These results have important implications for the debate on the merits of conflict and contact theory. They confirm the prediction of conflict theory that ethnic diversity is on average related to mistrust, and the prediction of contact theory that intergroup interactions contribute to mistrust only if the ethnically diverse individuals also differ in their socio-economic status.

1.5.4 Robustness tests

Appendix 1.D presents many robustness checks. Tables 1.D.1–1.D.6 test the internal validity of our main results. In Table 1.D.1, we base our indices primarily on the Afrobarometer's ethnicity information rather than its language information. In Table 1.D.2, we base our indices on ethnolinguistic distances computed by the

[31] One possible reason for this negative relation could be that there are more resources available to be seized in richer communities, providing incentives for conflict and appropriation (Garfinkel and Skaperdas, 2007) and thereby deteriorating trust (Uslaner, 2002; Rohner et al., 2013). Table 1.4 (columns 5 and 6) indeed shows that average wealth is positively related to violent conflict. Another possible reason for this negative relation is that richer communities tend to be more populous and more likely to be located in urban areas. As a result, social interactions in richer communities may be more anonymous, which could deteriorate trust.

formula of Fearon (2003) rather than Putterman and Weil (2010). In Table 1.D.3, we base our indices on economic distances measured by absolute differences in the lived poverty index, which is an experiential measure of poverty.[32] In Table 1.D.4, we drop towns and villages where the computation of our indices is based on fewer than eight individuals, as our indices may be biased when computed with a very small number of observations (Deltas, 2003). In Table 1.D.5, we follow Nunn and Wantchekon (2011) and use the respondents' categorical answers to the trust questions of interest to build variables that can take integer values from 0–3. In Table 1.D.6, we follow Rohner et al. (2013) in estimating Probit maximum likelihood models instead of linear probability models. These robustness tests support the general pattern of a negative relation between local ethnic stratification and trust in relatives, neighbors and others. The only exception is that the negative coefficients on ethnic stratification become statistically insignificant in at least half of the regressions when using the lived poverty index to compute economic distances.

In Table 1.D.7, we provide results for regressions including benchmark stratification, which is defined in Proposition 1 as $2\mu_{vc}G_{vc}F_{vc}$, as an additional regressor. By construction, benchmark stratification corresponds to our index of ethnic stratification if and only if ethnolinguistic and economic distances are unrelated to one another. This exercise therefore allows us to take a closer look at the role that the co-directionality of ethnolinguistic and economic distances plays in predicting mistrust. We find that the estimated coefficients on our index remain negative in all twelve regressions. While only four of these are statistically significant, most have similar magnitude as those in Table 1.2. The differences are in the standard errors, as they have increased by more than three-folds in the non-significant cases. This is due to the multicollinearity issues arising from the high correlation between our index and benchmark stratification. On the other hand, the estimated coefficients on benchmark stratification vary in sign across columns. They also

[32] The lived poverty index is based on questions about how often respondents and their family members had gone without food, water, medical care, cooking fuel, and cash income over the past year. It corresponds to the average of the ordinal answers to these five questions. We reverse its scale as higher values of the original index imply that people had to go without basic necessities more often.

tend to generate low t-ratios when compared to ethnic stratification (lower by at least an order of magnitude in all but two cases), and they are never statistically significant. These findings suggest that our index, which captures the interaction of ethnolinguistic and economic differences at the level of pairs of individuals, is a better predictor of mistrust than alternatives like benchmark stratification, which simply capture the interaction of ethnic diversity and economic inequality at the level of towns and villages. Hence, trust is low in ethnically stratified towns and villages, not just because they are ethnically diverse and economically unequal, but also because ethnic and economic differences between pairs of individuals go hand-in-hand.

We next test whether our results carry over to ethnic stratification in alternative geographical units, to alternative Afrobarometer survey rounds, and to alternative trust measures. In Table 1.D.8, we restrict our attention to respondents living within the boundaries of a city with more than 50,000 inhabitants, and compute the index of ethnic stratification and its components at the level of these cities.[33] In Tables 1.D.9 and 1.D.10, we again include the respondents from all our locations, but compute the indices at the level of districts (ADM2 regions) and provinces (ADM1 regions), respectively. The coefficients of interest remain negative and sizeable in most instances, with those in specification (1.5) typically being statistically significant when the outcome variable is trust in neighbors or others. We conjecture that our trust measures are more closely related to ethnic stratification at the local level than at the district or province level, because most respondents may primarily interact with people at the local level and because the corresponding trust questions in the Afrobarometer surveys explicitly ask about people whom the respondent knows.[34]

We include additional Afrobarometer survey rounds in Tables 1.D.11 and

[33]To identify city boundaries, we rely on the city polygons provided by the Africapolis database (OECD/SWAC, 2018), and consider Afrobarometer locations that lie within these polygons. We use the population count in 2010, which is also provided by Africapolis.

[34]To further explore the role of cities, we test whether the relation between ethnic stratification and trust is stronger in cities than in rural areas. In Table 1.D.14, we rely on the Afrobarometer's information on whether a location is urban or rural. In Table 1.D.15, we use information on whether or not a cluster lies within the boundaries of a city with more than 50,000 inhabitants. The relation between ethnic stratification and trust in neighbors and others tends to be stronger in cities, but the differences are typically not statistically significant.

1.D.12. First, we include round 4 and compute our indices for rounds 4 and 5 based on those wealth variables that are available in both rounds. The inclusion of round 4 implies that we can no longer use information about the type of shelter where the respondent lives and the material of its roof. Table 1.D.11 presents our estimates based on rounds 4 and 5. We again see the same pattern as in Table 1.2, but the coefficients become smaller in absolute values. A plausible reason is attenuation bias due to a coarsened measure of ethnic stratification resulting from the reduction of wealth information.[35] In Table 1.D.12, we additionally include round 3, which provides less information on assets and no information on the quality of housing. We therefore compute the indices for all rounds using the lived poverty index as the measure of individual wealth (as in Table 1.D.3). Perhaps unsurprisingly, results become weaker, but the coefficient of interest remains negative in all instances and statistically significant in specification (1.5).

We consider two alternative trust variables in Table 1.D.13: trust in the municipal assembly and generalized trust. The latter is based on the question: "Generally speaking, would you say that most people can be trusted or that you must be very careful in dealing with people?" As expected, local ethnic stratification tends to be negatively related to trust in the municipal assembly. Local ethnic stratification tends to be negatively associated with generalized trust as well, but the corresponding coefficients are relatively small in absolute values and not statistically significant. They become somewhat larger when computing our indices at the level of provinces instead of towns and villages. This pattern is consistent with the idea that economic and ethnolinguistic differences at the local level matter for how respondents answer questions on trust in people they know, but less so for how they answer the generalized trust question.

[35] The results for trust in neighbors supports this interpretation. The corresponding question was not asked in Afrobarometer surveys of round 4. Hence, the only difference between columns (3)–(4) of Tables 1.2 and 1.D.11 is that the indices are based on a noisier measure of the respondents' wealth in Table 1.D.11; and we indeed find that the coefficients are lower in columns (3)–(4) of Table 1.D.11.

1.5.5 Comparison of our index with other indices of diversity and inequality

We next investigate how our index of ethnic stratification compares to some other indices of ethnic diversity and economic inequality when it comes to predicting mistrust at the local level in Africa. We focus on the following well-known indices: The between-group Gini coefficient, the between-group polarization index by Gigliarano and Mosler (2009), the distance-Gini mean difference of Koshevoy and Mosler (1997), the between-group and within-group Theil indices, and the index of ethnolinguistic fractionalization (ELF) for categorical ethnicity data computed at different levels of the Ethnologue's language tree following Desmet et al. (2009). We compute all these indices at the local level using the same data as for the computation of our own index.[36] The estimates in the odd columns of Table 1.3 are based on regressions analogous to equation (1.5) where we replace our index of ethnic stratification by these alternative indices. These alternative indices (apart from the within-group Theil index) are negatively related to trust in relatives, neighbors and others, and most of the corresponding coefficients are statistically significant.

The even columns of Table 1.3 present regressions analogous to the odd columns but include our index of ethnic stratification as an additional regressor. They confirm that these alternative indices (apart from the within-group Theil index and ELF at level 1) and our index of ethnic stratification are negatively related to all three measures of trust. The pattern of statistical significance suggests that ethnic stratification is a more important factor than the other indices for describing trust in neighbors and others, while the between-group Gini and between-group polarization are the leading factors in describing trust in relatives. We interpret these findings as evidence that our index of ethnic stratification captures characteristics of a community that (i) are important for predicting mistrust (in particular mistrust towards people outside the family) and (ii) are not captured by established indices of ethnic diversity and economic inequality.

— Table 1.3 about here —

[36]Summary statistics for these indices are presented in Appendix 1.C.4.

1.5.6 Results for crime and conflict

In this section, we look at alternative measures of social conflict, namely crime and violent conflict. We use the two Afrobarometer-based binary variables indicating fear of crime and actual crime as dependent variables in columns (1)–(4) of Table 1.4. We see that local ethnic stratification is positively related to fear of crime.[37] The relation to actual crime is ambiguous, with different signs across specifications.

For conflict we use our binary variable indicating whether there would be a violent conflict event within 10 km of the center of the town or village within three years from the date of the interviews. The fact that this information is available only at the level of towns and villages (rather than individual respondents) has several implications for our empirical specifications. First, the sample size decreases from the number of respondents to the number of towns and villages. To keep the results as comparable as possible to previous results, we run weighted linear regressions with weights equal to the number of respondents per town or village. Second, we average the individual control variables at the level of towns and villages. Third, we can no longer include country-group fixed effects. We replace them with simple country fixed effects. Columns (5) and (6) of Table 1.4 show that local ethnic stratification is a strong predictor of future violent conflict events, in particular in specifications without province fixed effects.

— Table 1.4 about here —

Unlike the questions on trust in relatives, neighbors and others, the questions on crime were asked in Afrobarometer surveys of round 6 as well. In addition, this round also includes all the relevant questions on assets and the quality of housing used to construct an informative wealth index. The conflict data, too, is available for the three years following the interviews of round 6. We present results using the index of ethnic stratification based on Afrobarometer surveys of rounds 5 and 6 in Table 1.D.16. The pattern remains similar as in Table 1.4. The positive relation between local ethnic stratification and future violent conflict events even

[37]The positive relation between ethnic stratification and (fear of) crime could be indirect, as lower trust may increase (fear of) crime (Buonanno et al., 2009).

extends to specifications that include province fixed effects. These results suggest that the patterns presented throughout our paper should generalize beyond the respondents of Afrobarometer surveys of round 5.

1.6 Conclusions

In this paper, we have defined an index of ethnic stratification that generalizes the idea of between-group inequality and measures the extent to which the hierarchy in socio-economic positions follows ethnolinguistic lines. A defining feature of this index is a strong complementarity between economic and ethnolinguistic distances, which is essential to the notion of ethnic stratification as a generalization of between-group inequality. We have provided an axiomatic characterization of our index and discussed how it depends on average wealth, the Gini coefficient of wealth inequality, the index of generalized ethnic fractionalization, and the co-directionality of economic and ethnolinguistic distances across pairs of individuals.

We have computed our index at the level of towns and villages in 26 diverse African countries, and documented a robust negative relation between local ethnic stratification and trust in relatives, neighbors and other acquaintances. Future violent conflict events are more likely around ethnically stratified towns and villages too. These findings suggest that our proposed index is indeed a good predictor of social conflict, and we have shown that it tends to be more successful in predicting mistrust towards neighbors and others than many established measures of diversity and inequality.

We have further employed the index of generalized ethnic fractionalization and our index of ethnic stratification, which would reduce to the former in the absence of any economic inequality, to shed new light on the debate about the merits of conflict and contact theory. Our findings suggest that, on average, ethnic diversity tends to be associated with mistrust, as predicted by conflict theory; but that this negative relation is driven by towns and villages where ethnolinguistic distances between individuals are complemented by differences in economic resources, as predicted by contact theory.

We are hopeful that our index will prove useful in studying many more interesting and relevant questions on potential determinants and consequences of ethnic stratification. Furthermore, a casual glance at the world today reveals that there are socio-economic hierarchies along many social dimensions other than ethnicity. Prominent examples include caste and religion, the skill-level of occupations, and even cultural values. We are confident that our index can be fruitfully applied to studying stratification along such alternative social dimensions.

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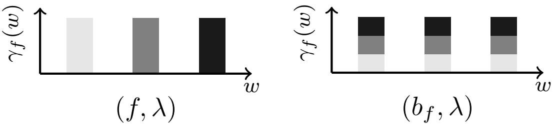
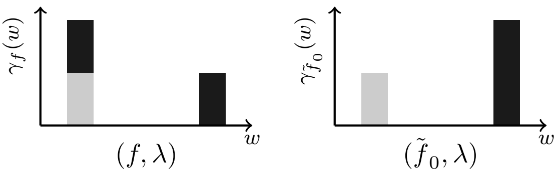
Figures

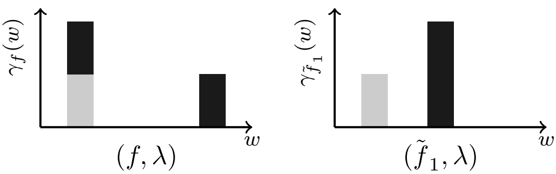


Notes: Each tone of gray indicates a different ethnicity, and ethnolinguistic distances between ethnicities are given by differences in tones of gray.

Figure 1.1: Illustration of a pair of density and linguistic functions and the corresponding benchmark



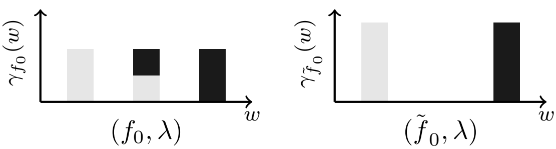
(a) Axiom CDbW with $\epsilon = 0$.



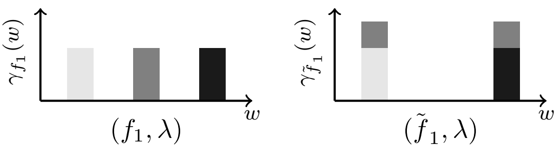
(b) Axiom CDbW with $\epsilon = 1$.

Notes: The two graphs of each sub-figure illustrate examples of the two pairs of density and linguistic functions corresponding to this axiom. Each tone of gray indicates a different ethnicity.

Figure 1.2: Illustration of Axiom CDbW



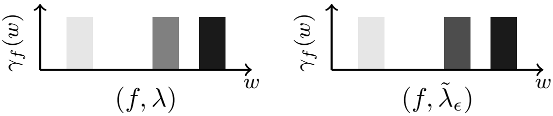
(a) Axiom BPbW with $\epsilon = 0$.



(b) Axiom BPbW with $\epsilon = 1$.

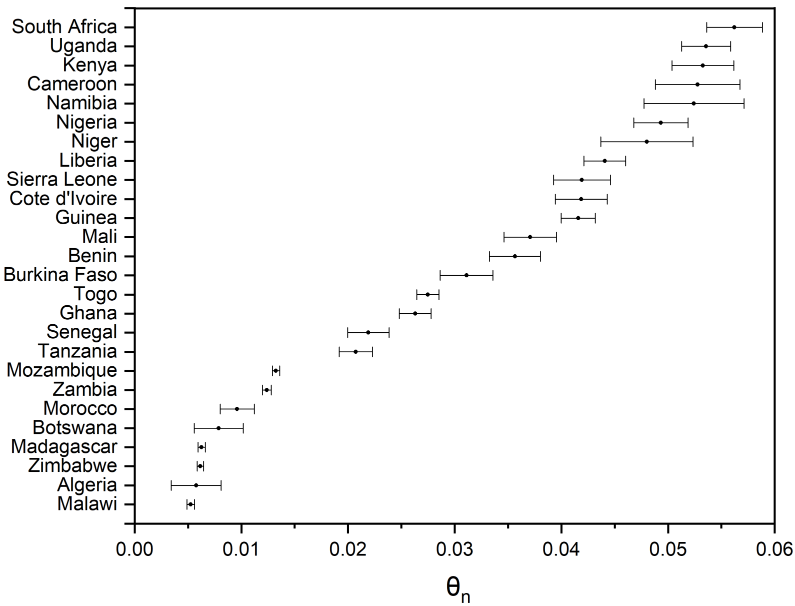
Notes: The two graphs of each sub-figure illustrate examples of the two pairs of density and linguistic functions corresponding to this axiom. Each tone of gray indicates a different ethnicity, and ethnolinguistic distances between ethnicities are given by differences in tones of gray.

Figure 1.3: Illustration of Axiom BPbW



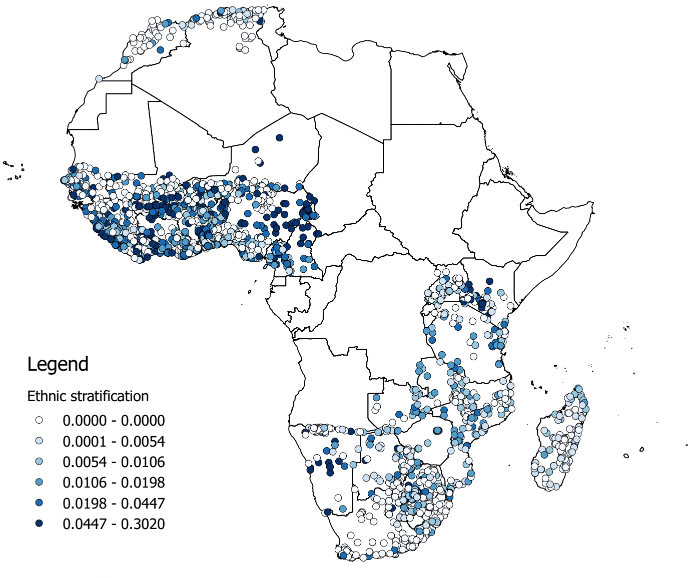
Notes: The two graphs illustrate examples of the two pairs of density and linguistic functions corresponding to this axiom. Each tone of gray indicates a different ethnicity, and ethnolinguistic distances between ethnicities are given by differences in tones of gray.

Figure 1.4: Illustration of Axiom CDbL with generic $\epsilon \in (0, \lambda(1, 2)]$



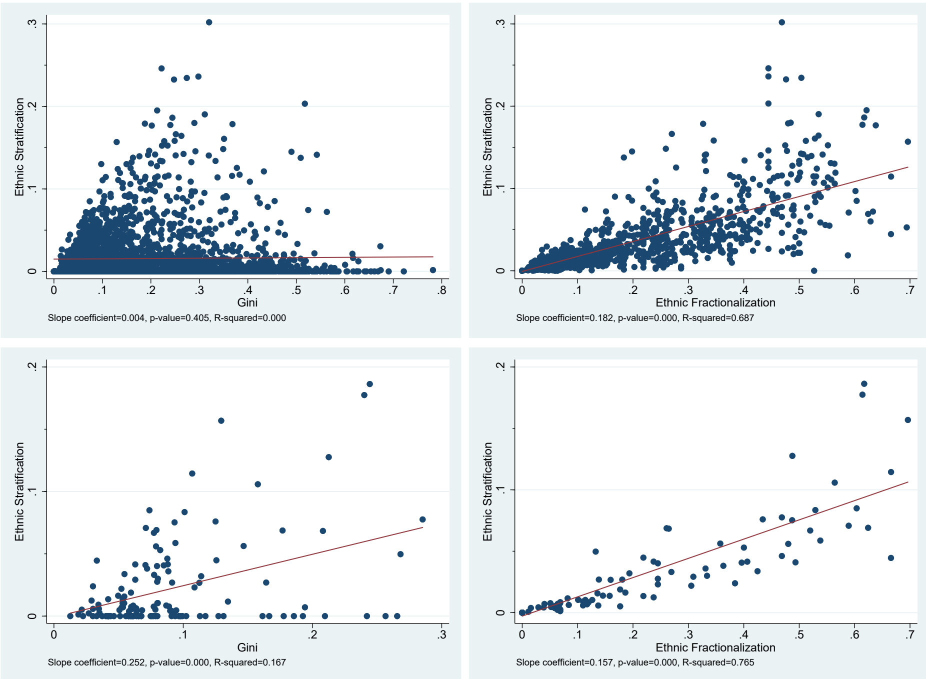
Notes: Dots indicate point estimates of the countries' ethnic stratification index and lines the 95% confidence intervals based on equations (1.4) and (1.10). Data are from Afrobarometer surveys of round 5. Economic distances are based on our wealth index. Ethnolinguistic groups are primarily based on the respondents' language, and ethnolinguistic distances are computed using the Putternam and Weil (2010) formula.

Figure 1.5: Country-level inference



Notes: Dots indicate the towns and villages in the final sample from Afrobarometer round 5. Darker shades indicate higher local ethnic stratification.

Figure 1.6: Towns and villages in Afrobarometer round 5



Notes: The two scatter plots on the left show the association between the index of ethnic stratification and the Gini coefficient; and the two on the right the association between the index of ethnic stratification and ethnic fractionalization. The two scatter plots in the top row include all towns and villages from our sample, and the two in the bottom row only towns and villages from Nigeria.

Figure 1.7: Scatter plots illustrating the index of ethnic stratification and its main components

Tables

Table 1.1: Summary statistics

Variable	Obs.	Mean	Std.Dev.	Min.	Max.
Stratification	21,379	0.018	0.033	0	0.302
Fractionalization	21,379	0.103	0.147	0	0.696
Gini	21,379	0.203	0.140	0	0.782
Average wealth	21,379	0.462	0.243	0.012	1
Trust in relatives	21,318	0.826	0.379	0	1
Trust in neighbors	21,295	0.607	0.488	0	1
Trust in others	21,249	0.416	0.493	0	1
Fear of crime	21,330	0.700	1.172	0	1
Actual crime	21,372	0.296	0.457	0	1
Violent conflict	2,558	0.276	0.447	0	1

Notes: All variables are described in the text. They are all based on Afrobarometer surveys of round 5, except that violent conflict events is based on ACLED. The units of observation are individual respondents for all variables based on Afrobarometer, but towns and villages for conflict events. Summary statistics for violent conflict events are weighted by the number of respondents per town or village.

Table 1.2: Main results for trust

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Panel A:						
Stratification	-0.43**	-0.36**	-0.77***	-0.59***	-0.63***	-0.38**
	(0.17)	(0.15)	(0.18)	(0.20)	(0.13)	(0.16)
R^2	0.16	0.21	0.19	0.23	0.12	0.17
Panel B:						
Stratification	-0.63**	-0.42*	-0.88***	-0.63**	-0.83***	-0.73***
	(0.29)	(0.22)	(0.30)	(0.29)	(0.30)	(0.28)
Fractionalization	0.08	0.04	0.06	0.04	0.07	0.12*
	(0.06)	(0.05)	(0.07)	(0.06)	(0.07)	(0.07)
Gini	-0.02	-0.06	-0.04	-0.08	0.02	0.01
	(0.05)	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)
Average wealth	-0.08**	-0.10**	-0.16**	-0.14**	-0.13**	-0.11**
	(0.04)	(0.05)	(0.06)	(0.07)	(0.06)	(0.05)
R^2	0.16	0.21	0.19	0.23	0.13	0.17
Panel C:						
Fractionalization	-0.04	-0.04	-0.10**	-0.08*	-0.08***	-0.02
	(0.03)	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)
Gini	-0.06	-0.09	-0.10	-0.11*	-0.04	-0.04
	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)
Average wealth	-0.10**	-0.11**	-0.18***	-0.15**	-0.15***	-0.13**
	(0.04)	(0.04)	(0.07)	(0.07)	(0.06)	(0.05)
R^2	0.16	0.21	0.19	0.23	0.12	0.17
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,318	21,318	21,295	21,295	21,249	21,249

Notes: Linear probability estimates. Units of observation are respondents to Afrobarometer surveys of round 5. Dependent variables indicate whether respondents trust the respective group/institution “a lot” or “somewhat” as opposed to “not at all” or “just a little”. The index of ethnic stratification, the index of ethnic fractionalization, the Gini coefficient and average wealth are introduced in Section 1.3. These indices are computed relying on (i) economic wealth and economic distances that are based on the wealth index, and (ii) ethnolinguistic distances that are primarily based on the respondents’ language and the Putternam and Weil (2010) formula. All regressions include individual and geographical control variables. The individual control variables are the respondents’ economic wealth, age and age squared, and indicator variables for gender, religion, education and urban. The geographical control variables are malaria suitability, soil suitability for agriculture, distance to coast, altitude, terrain ruggedness, average precipitation, population and past conflict events. All columns include interacted country-ethnolinguistic group fixed effects. Even columns further include province (ADM1 region) fixed effects. All data (except the geographical controls) are based on Afrobarometer surveys of round 5. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.3: Comparison of our index with other indices of diversity and inequality

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Panel A:						
Between-group Gini	-0.34***	-0.30**	-0.44***	-0.16	-0.29***	0.05
	(0.12)	(0.14)	(0.10)	(0.14)	(0.09)	(0.12)
Stratification		-0.08		-0.58**		-0.69***
		(0.19)		(0.26)		(0.19)
R^2	0.16	0.16	0.19	0.19	0.12	0.12
Panel B:						
Between-group Pol.	-0.88***	-0.70*	-1.04***	-0.28	-0.75***	-0.04
	(0.34)	(0.40)	(0.27)	(0.34)	(0.24)	(0.30)
Stratification		-0.16		-0.66***		-0.62***
		(0.18)		(0.24)		(0.17)
R^2	0.16	0.16	0.19	0.19	0.12	0.12
Panel C:						
Distance-Gini	-0.37**	-0.13	-0.63***	-0.15	-0.48***	-0.01
	(0.14)	(0.19)	(0.16)	(0.20)	(0.13)	(0.22)
Stratification		-0.32		-0.64**		-0.63***
		(0.23)		(0.25)		(0.21)
R^2	0.16	0.16	0.19	0.19	0.12	0.12
Panel D:						
Between-group Theil	-0.26	-0.14	-0.46***	-0.24*	-0.30**	-0.11
	(0.18)	(0.17)	(0.14)	(0.14)	(0.12)	(0.13)
Within-group Theil	0.01	0.01	0.03	0.03	0.06	0.06
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Stratification		-0.35**		-0.65***		-0.58***
		(0.15)		(0.19)		(0.14)
R^2	0.16	0.16	0.19	0.19	0.12	0.12
Panel E:						
ELF (level 1)	-0.07	0.03	-0.15***	-0.01	-0.15***	-0.05
	(0.04)	(0.07)	(0.05)	(0.07)	(0.03)	(0.06)
Stratification		-0.50*		-0.75***		-0.53**
		(0.25)		(0.25)		(0.23)
R^2	0.16	0.16	0.19	0.19	0.12	0.12
Panel F:						
ELF (level 6)	-0.01	0.06**	-0.05	0.07**	-0.05*	0.05
	(0.02)	(0.03)	(0.03)	(0.04)	(0.02)	(0.04)
Stratification		-0.67***		-1.05***		-0.82***
		(0.22)		(0.24)		(0.21)
R^2	0.16	0.16	0.19	0.19	0.12	0.12
Panel G:						
ELF (level 15)	-0.05***	-0.02	-0.09***	-0.04*	-0.05***	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Stratification		-0.34		-0.58**		-0.63***
		(0.22)		(0.23)		(0.18)
R^2	0.16	0.16	0.19	0.19	0.12	0.12
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,318	21,318	21,295	21,295	21,249	21,249

Notes: See Section 1.5.5 and Appendix 1.C.4 for information on all the other indices of diversity and inequality, and the notes to Table 1.2 for details on all other aspects of the regressions presented in this table. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.4: Crime and conflict

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Fear of crime		Actual crime		Violent conflict	
Units of obs.:	Ind. respondents		Ind. respondents		Towns/villages	
Stratification	0.45***	0.27*	0.27*	0.11	1.38***	0.49
	(0.16)	(0.15)	(0.14)	(0.17)	(0.48)	(0.32)
R^2	0.11	0.15	0.09	0.12	0.52	0.69
Stratification	0.46*	0.41*	-0.35	-0.46*	1.67**	0.34
	(0.25)	(0.25)	(0.24)	(0.24)	(0.77)	(0.50)
Fractionalization	0.01	-0.04	0.19***	0.17***	-0.07	0.02
	(0.07)	(0.07)	(0.06)	(0.05)	(0.14)	(0.10)
Gini	-0.03	-0.03	0.03	0.05	-0.07	0.09
	(0.07)	(0.08)	(0.07)	(0.06)	(0.09)	(0.07)
Average wealth	-0.02	-0.03	-0.03	-0.06	0.15*	0.17***
	(0.05)	(0.06)	(0.05)	(0.05)	(0.08)	(0.06)
R^2	0.11	0.15	0.09	0.12	0.52	0.69
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	No	No	No	No	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	No	No
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,330	21,330	21,372	21,372	2,558	2,558

Notes: Dependent variables are an indicator of whether the respondent (or a family member) had feared crime within their home over the past year in columns (1)–(2); an indicator of whether the respondent (or a family member) had been physically attacked or something had been stolen from their home over the past year in columns (3)–(4), and an indicator of whether a violent conflict event would occur nearby within three years after the interviews in columns (5)–(6). See the notes to Table 1.2 for further details relevant for columns (1)–(4). Columns (5)–(6) use towns and villages rather than individual respondents as units of observation and present weighted least squares using the number of respondents per town or village as weights. Individual controls are averaged at the level of towns and villages in columns (5)–(6). Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions in columns (1)–(4), and for clustering at the level of provinces in columns (5)–(6). ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Appendix 1.A: Proofs of results in Section 1.3

Proof of Proposition 1 Let (f, λ) be any pair of density and linguistic functions. The ethnic stratification of the corresponding benchmark (b_f, λ) is

$$S(b_f, \lambda) = \int_{e \in E} \int_{e' \in E} \int_{w \in W} \int_{w' \in W} b_f(e, w) b_f(e', w') \lambda(e, e') |w - w'| dw' dw de' de,$$

where by definition of the benchmark

$$b_f(e, w) = \gamma_{b_f}(w) \varphi_{b_f}(e) = \gamma_f(w) \varphi_f(e) \text{ for each } e \in E \text{ and } w \in W.$$

Then, combining these equations we obtain that $S(b_f, \lambda)$ is equal to

$$\left(\int_{w \in W} \int_{w' \in W} \gamma_f(w) \gamma_f(w') |w - w'| dw' dw \right) \left(\int_{e \in E} \int_{e' \in E} \varphi_f(e) \varphi_f(e') \lambda(e, e') de' de \right),$$

which leads to $S(b_f, \lambda) = 2\mu(\gamma_f) G(\gamma_f) F(\varphi_f, \lambda)$ and concludes our proof. $\square$

Proof of Theorem 1 It is straightforward that any positive scalar multiplication of index (1.2) fulfills axioms CDbW, BPbW, CDbL. Then, it remains to be shown that an index from class (1.1) satisfies these axioms only if it takes the form (1.2) up to positive scalar multiplication, i.e., only if $\pi(a, b) = kab$ for some constant $k > 0$. By axiom CDbW, focusing on the extreme case $\epsilon = 1$,

$$\begin{aligned} M(\tilde{f}_1, \lambda) &= (4/9) \pi \left(\lambda(1, 2), |w_1 - w_2|/2 \right) \\ &\geq M(f, \lambda) = (2/9) \left[\pi \left(\lambda(1, 2), |w_1 - w_2| \right) + \pi(0, |w_1 - w_2|) + \pi(\lambda(1, 2), 0) \right], \end{aligned}$$

which can be rewritten as

$$2\pi \left(\lambda(1, 2), |w_1 - w_2|/2 \right) - \pi \left(\lambda(1, 2), |w_1 - w_2| \right) \geq \pi(0, |w_1 - w_2|) + \pi(\lambda(1, 2), 0). \quad (1.7)$$

By axiom BPbW, focusing on the extreme case $\epsilon = 1$,

$$\begin{aligned} M(\tilde{f}_1, \lambda) = (2/9) &\left[\pi \left(\lambda(1, 3)/2, |w_1 - w_3| \right) \right. \\ &+ \pi \left(\lambda(1, 3)/2, 0 \right) + \pi \left(0, |w_1 - w_3| \right)/4 + \pi \left(\lambda(1, 3), |w_1 - w_3| \right) \left. \right] \\ &\geq M(f_1, \lambda) = (2/9) \left[2\pi \left(\lambda(1, 3)/2, |w_1 - w_3|/2 \right) + \pi \left(\lambda(1, 3), |w_1 - w_3| \right) \right], \end{aligned}$$

which implies

$$\pi\left(0,|w_1-w_3|\right)/4 \geq 2\pi\left(\lambda(1,3)/2,|w_1-w_3|/2\right)-\pi\left(\lambda(1,3)/2,|w_1-w_3|\right). \tag{1.8}$$

Combining (1.8) with (1.7) and letting $a, b \geq 0$ denote a generic pair of wealth and ethnolinguistic distances, we obtain

$$\pi(0,b)/4 \geq 2\pi(a,b/2)-\pi(a,b) \geq \pi(0,b)+\pi(a,0).$$

By the non-negativity of π this implies $\pi(0,b)=\pi(a,0)=0$ and $2\pi(a,b/2)=\pi(a,b)$, so that there is a non-decreasing function $\rho:\mathbb{R}_+\rightarrow\mathbb{R}_+$ such that $\pi(a,b)=\rho(a)b$ and $\rho(0)=0$. By axiom CDbL, for each $\epsilon\in(0,\lambda(1,2)]$,

$$\begin{aligned}M(f,\tilde{\lambda}_\epsilon) = {} & (2/9)\left[\pi\left(\lambda(1,2)-\epsilon,|w_1-w_2|\right)+\pi\left(\lambda(2,3)+\epsilon,|w_2-w_3|\right)\right.\\ & \left.+\pi\left(\lambda(1,2)+\lambda(2,3),|w_1-w_2|+|w_2-w_3|\right)\right]\\ & \geq M(f,\lambda)=(2/9)\left[\pi\left(\lambda(1,2),|w_1-w_2|\right)+\pi\left(\lambda(2,3),|w_2-w_3|\right)\right.\\ & \left.+\pi\left(\lambda(1,2)+\lambda(2,3),|w_1-w_2|+|w_2-w_3|\right)\right],\end{aligned}$$

which by our previous finding $\pi(a,b)=\rho(a)b$ can be rewritten as

$$\rho(\lambda(1,2)-\epsilon)|w_1-w_2|+\rho(\lambda(2,3)+\epsilon)|w_2-w_3|\geq\rho(\lambda(1,2))|w_1-w_2|+\rho(\lambda(2,3))|w_2-w_3|. \tag{1.9}$$

Note that the axiom's restrictions $w_1>w_2>w_3$ and $w_2>(w_1+w_3)/2$ imply $|w_1-w_2|<|w_2-w_3|$. Then, by (1.9) the function ρ is linear and, given our previous findings, we must have $\rho(a)=ka$ for some constant $k\geq 0$. As $\pi(a,b)>0$ for some $a,b>0$ by assumption, it follows that $k\neq 0$ which concludes our proof. $\square$

Appendix 1.B: Statistical properties of the ethnic stratification index

Here we provide the statistical properties of θ_n, as defined in (1.4), and explain how they can be used to perform inference on θ. We list them in Propositions 2 to 4. Their proofs are collected at the end of the appendix.

Proposition 2. *Suppose* $\mathbb{E}[|\lambda(E, E')|W - W'||] < \infty$, *then* $\theta_n \xrightarrow{p} \theta$ *as* $n \to \infty$.

Proposition 3. *Suppose* $\mathbb{E}[|\lambda(E, E')|W - W'||^2] < \infty$, *then* $\sqrt{n}(\theta_n - \theta) \xrightarrow{d} \mathcal{N}(0, \sigma^2)$ *as* $n \to \infty$, *where* $\sigma^2 = 4Var(\mathbb{E}[\lambda(E, E')|W - W'||E, W])$.

Propositions 2 and 3 respectively assume that $\lambda(E, E')|W - W'|$ has finite first and second moments. These conditions are expected to be satisfied in most applications. For instance, a sufficient condition for $\mathbb{E}[|\lambda(E, E')|W - W'||^k] < \infty$ is when λ is a bounded function and $\mathbb{E}|W|^k < \infty$ for $k = 1, 2$. Then, Proposition 2 says that θ_n is a consistent estimator for θ, and Proposition 3 says that θ_n has a limiting normal distribution. Furthermore, the asymptotic variance of θ_n has a simple form that can be estimated by using its sample counterpart. This can be seen from re-writing σ^2 as

$$\sigma^2 = 4\mathbb{E}[\mathbb{E}[\lambda(E, E')|W - W'||E, W]^2] - 4(\mathbb{E}[\lambda(E, E')|W - W'|])^2.$$

One natural candidate for an estimator of σ^2 is σ_n^2, where we again replace expectations in the display above by sample averages:

$$\sigma_n^2 := \frac{4}{n}\sum_{i=1}^{n}\left(\frac{1}{n}\sum_{j=1}^{n}\lambda(E_i, E_j)|W_i - W_j|\right)^2 - 4\theta_n^2. \tag{1.10}$$

Proposition 4 says that σ_n^2 is a consistent estimator for σ^2.

Proposition 4. *Suppose* $\mathbb{E}[|\lambda(E, E')|W - W'||^2] < \infty$, *then* $\sigma_n^2 \xrightarrow{p} \sigma^2$ *as* $n \to \infty$.

Propositions 2 to 4 ensure that we can construct valid confidence intervals and perform hypothesis tests on ethnic stratification based on normal approximation under weak conditions. Alternatively, inference can also be performed using a standard bootstrap (i.e. random resampling with replacement). We will show below that θ_n is essentially a U-statistic that can be consistently bootstrapped (see Arcones and Giné (1992)). Subsequently, we can easily perform inference on differences between ethnic stratifications across independent samples.

Proofs of Propositions 2 and 3 Our Propositions can be easily proven once we recognize that θ_n is asymptotically equivalent to the following object:

$$\theta'_n = \frac{2}{n(n-1)} \sum_{i=1}^{n-1} \sum_{j=i+1}^{n} \lambda(E_i, E_j) |W_i - W_j| .$$

First note that we can re-write θ_n as:

$$\theta_n = \frac{2}{n^2} \sum_{i=1}^{n-1} \sum_{j=i+1}^{n} \lambda(E_i, E_j) |W_i - W_j| .$$

This follows from the fact that $\lambda(E_i, E_j) = \lambda(E_j, E_i)$ for all i, j. Since $\theta_n = \frac{n-1}{n}\theta'_n$ it is clear that θ'_n and θ_n are asymptotically equivalent.

In what follows we shall focus on the asymptotic properties of θ'_n which takes the form of a standard second order U-statistic. We refer the reader to Chapter 5.3 in Serfling (1980) for background materials on this subject. A crucial element in deriving statistical properties of a U-statistic is its *projection*. In particular, for $i \neq j$, let

$$r(E_i, W_i) = \mathbb{E}[\lambda(E_i, E_j) |W_i - W_j| | E_i, W_i] ,$$

then we can denote the projection of θ'_n by:

$$\begin{aligned} \widehat{\theta'_n} &= \sum_{i=1}^{n} \mathbb{E}[\theta'_n | E_i, W_i] - (n-1)\mathbb{E}[r(E_i, W_i)] \\ &= \mathbb{E}[r(E_i, W_i)] + \frac{2}{n} \sum_{i=1}^{n} (r(E_i, W_i) - \mathbb{E}[r(E_i, W_i)]) . \end{aligned}$$

The projection is well-defined since $\mathbb{E}[|\lambda(E, E')|W - W'||] < \infty$. By the Law of Iterated Expectation we have $\theta = \mathbb{E}[r(E_i, W_i)]$. Then we can write,

$$\widehat{\theta}'_n - \theta = \frac{2}{n} \sum_{i=1}^{n} \left(r(E_i, W_i) - \mathbb{E}[r(E_i, W_i)]\right).$$

Furthermore, it can be shown that $\theta'_n - \theta$ and $\widehat{\theta}'_n - \theta$ have the same asymptotic distribution when $\mathbb{E}[|\lambda(E, E')|W - W'||^2] < \infty$. The square integrability condition holds by assumption. Therefore $\theta'_n = \widehat{\theta}'_n + o_p\left(n^{-1/2}\right)$. Thus $\theta_n - \theta$ can be approximated by a sum of i.i.d. zero mean variables as shown in the display above. Propositions 2 and 3 then follow immediately from a standard Law of Large Numbers and Central Limit Theorem for i.i.d. variables respectively. □

Proof of Proposition 4 Let r_n denote the sample counterpart of r, which is defined in the previous proof, so that

$$r_n(E_i, W_i) = \frac{1}{n-1} \sum_{j \neq i}^{n} \lambda(E_i, E_j)|W_i - W_j|.$$

We can then write (1.10) as,

$$\sigma_n^2 = \frac{4}{n} \left(\frac{n-1}{n}\right)^2 \sum_{i=1}^{n} r_n(E_i, W_i)^2 - 4\theta_n^2,$$

and σ^2, defined in Proposition 3, can be written as

$$\sigma^2 = 4\mathbb{E}[r(E_i, W_i)^2] - 4\theta^2.$$

Since θ_n is consistent, it suffices to show

$$\frac{1}{n} \sum_{i=1}^{n} r_n(E_i, W_i)^2 = \mathbb{E}[r(E_i, W_i)^2] + o_p(1).$$

To this end,

$$\begin{aligned}\mathbb{E}[|r_n(E_i,W_i)-r(E_i,W_i)|^2] &= \mathbb{E}\left[Var\left(r_n(E_i,W_i)\,|E_i,W_i\right)\right]\\ &= \frac{1}{n-1}\mathbb{E}\left[Var\left(\lambda\left(E_i,E_j\right)|W_i-W_j||\,X_i\right)\right]\\ &\leq \frac{1}{n-1}\mathbb{E}[|\lambda\left(E_i,E_j\right)|W_i-W_j||^2]\\ &= O\left(n^{-1}\right).\end{aligned}$$

Therefore $\mathbb{E}[|r_n(E_i,W_i)-r(E_i,W_i)|^2] = o(1)$, which implies $\mathbb{E}[|r_n(E_i,W_i)^2 - r(E_i,W_i)^2|] = o(1)$, and the required result follows from Markov's inequality. The proof then follows from applications of the Continuous Mapping Theorem. $\square$

Appendix 1.C: Additional information on data

This appendix was published as part of the online appendix accompanying this paper.

1.C.1 Description of Afrobarometer data used to construct our wealth index

To construct our wealth index, we use the answers to the following questions from the Afrobarometer surveys of round 5:

(a) "Which of these things do you personally own?" "Radio", "Television" and "Motor vehicle, car or motorcycle." Answer categories are "No (Don't own)" and "Yes (Do own)." We construct an indicator variable for each of these three types/groups of assets.

(b) "How often do you use a computer?" Answer categories are "Every day," "A few times a week," "A few times a month," "Less than once a month," and "Never." We create an indicator variable for whether the household owns a computer, assigning the value one if the answer is "Every day", and the value zero for all other answer categories.

(c) "How many mobile phones are owned in total by members of your household, including yourself?" We construct an indicator variable equal to one if the household owns one or more mobile phones.

(d) "Please tell me whether each of the following are available inside your house, inside your compound, or outside your compound:" "Your main source of water for household use" and "A toilet or latrine." Answer categories are "Inside the house," "Inside the compound," "Outside the compound," and "None, no latrine available" (only for the second question). Following the approach by DHS (Rutstein, 2015), we construct an indicator variable for each answer category. Hence, we construct three indicator variables for the question on the main source of water, and four for the question on whether there is a toilet or latrine.

(e) “In what type of shelter does the respondent live?” Answer categories are “Non-traditional/formal house,” “Traditional house/hut,” “Temporary structure/shack,” “Flat in a block of flats,” “Single room in a larger dwelling structure or backyard,” “Hostel in an industrial compound or farming compound,” and “Other.” Analogous to (d), we construct seven indicator variables from the seven answer categories.

(f) “What was the roof of the respondent’s home or shelter made of?” Answer categories are “Metal, tin or zinc,” “Tiles,” “Shingles,” “Thatch or grass,” “Plastic sheets,” “Asbestos,” “Multiple materials,” “Some other material,” and “Concrete.” Analogous to (d) and (e), we construct nine indicator variables out of the nine answer categories.

To construct the wealth index for Afrobarometer surveys of rounds 5 and 6, we can use the same information. Note that in round 6 the question on mobile phones is: “Which of these things do you personally own: Mobile phone?”

To construct the wealth index for Afrobarometer surveys of rounds 4 and 5, we can only use the information in (a)–(d). Note that in round 4 the question on mobile phones is: “How often do you use a mobile phone?” We assume that a household owns a mobile phone if the respondent answers “Every day.”

To construct the wealth index for Afrobarometer surveys of rounds 3–5 (which we do not), we could only use the information in (a).

1.C.2 Shapley-Owen decomposition of our index of ethnic stratification

Table 1.C.1: Shapley-Owen decomposition of our index of ethnic stratification

	(1)		(2)		(3)	
	Coef.	$\%R^2$	Coef.	$\%R^2$	Coef.	$\%R^2$
Fractionalization	0.13***	54.55	0.13***	50.51	0.13***	42.39
	(0.01)		(0.01)		(0.01)	
Gini	0.03***	1.28	0.04***	1.48	0.04***	1.24
	(0.00)		(0.00)		(0.00)	
Average wealth	0.01***	2.11	0.01***	1.92	0.01***	1.61
	(0.00)		(0.00)		(0.00)	
Co-directionality	1.70***	42.06	1.67***	40.29	1.62***	35.51
	(0.05)		(0.06)		(0.08)	
Country dummies				5.80		
Province dummies						19.25
Country FE	No		Yes		No	
Province FE	No		No		Yes	
Observations	2,558		2,558		2,558	

Notes: This table presents the coefficients of regressions of ethnic stratification (S) on its components – ethnic fractionalization (F), the Gini coefficient (G), average wealth (μ), and co-directionality (Δ) – and the corresponding Owen values. The units of observations are the towns and villages on which our results in Tables 1.1–1.4 are based. We define co-directionality in substractive form as $\Delta = S - 2\mu GF$ (following Remark 1 and the accompanying footnote in Section 1.3.3). The Owen values show how much each regressor contributes to the R^2 in percent. The Owen value is an extension of the Shapley value that allows for group variables, such as the country and province (ADM1 region) fixed effects in columns (2) and (3), respectively. Standard errors adjusted for clustering at the province level. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

1.C.3 Geographic control and outcome variables and their sources

This section describes all the geographical/spatial variables used in the empirical analysis and indicates the data sources. All variables (except distance to coast) are computed within a buffer area with a radius of 10 km around Afrobarometer survey locations, using the geo-coordinates provided by BenYishay et al. (2017).

Malaria suitability Mean of the Temperature Suitability Index for Plasmodium falciparum transmission in 2010 from Gething et al. (2011).

Distance to coast Distance in km from the Afrobarometer survey location to the coast.

Soil suitability Mean of the Suitability for Agriculture Index from Ramankutty et al. (2002).

Precipitation Average yearly precipitation for 1946–2014 from Schneider et al. (2015).

Altitude and ruggedness Average altitude, and ruggedness as the standard deviation of the altitude within the buffer area from Amante and Eakins (2009).

Population Average population density in 2010 from CIESIN (2016).

Past conflict (as control variable) Share of years from 1979 to the year prior to the interview in which there was a least one conflict event of the type “battle”, “violence against civilians” or “explosions/remote violence” within the buffer area according to the Armed Conflict Location and Event Dataset (ACLED), which was introduced by Raleigh et al. (2010).

Future conflict (as outcome variable) Indicator variable that is equal to one if and only if there would be at least one conflict event of the type "battle", "violence against civilians" or "explosions/remote violence" within the buffer area in the first three years after the interview according to the Armed Conflict Location and Event Dataset (ACLED).

1.C.4 Summary statistics for other indices of inequality and diversity

Table 1.C.2: Summary statistics for other indices of inequality and diversity

Variable	Obs.	Mean	Std.Dev.	Min.	Max.
Between-group Gini	21,379	0.035	0.049	0	0.317
Between-group Pol.	21,379	0.012	0.017	0	0.151
Distance-Gini	21,379	0.054	0.036	0	0.186
Between-group Theil	21,379	0.013	0.036	-0.134	0.560
Within-group Theil	21,379	0.104	0.144	0	1.171
ELF (level 1)	21,379	0.043	0.122	0	0.656
ELF (level 6)	21,379	0.135	0.218	0	0.861
ELF (level 15)	21,379	0.274	0.277	0	0.885

Notes: All indices are described in Section 1.5.5 and computed using data from the Afrobarometer surveys of round 5.

Appendix 1.D: Additional results

This appendix was published as part of the online appendix accompanying this paper.

Table 1.D.1: Ethnicity instead of languages

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-0.39**	-0.32**	-0.69***	-0.55***	-0.63***	-0.44***
	(0.16)	(0.15)	(0.19)	(0.21)	(0.14)	(0.17)
R^2	0.16	0.21	0.19	0.23	0.12	0.17
Stratification	-0.55**	-0.30	-0.91***	-0.70**	-0.87***	-0.82***
	(0.27)	(0.22)	(0.28)	(0.27)	(0.29)	(0.29)
Fractionalization	0.06	0.02	0.09*	0.08*	0.08	0.13**
	(0.05)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)
Gini	-0.02	-0.07	-0.03	-0.07	0.03	0.02
	(0.05)	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)
Average wealth	-0.08**	-0.10**	-0.15**	-0.14**	-0.12**	-0.11**
	(0.04)	(0.04)	(0.07)	(0.07)	(0.06)	(0.05)
R^2	0.16	0.21	0.19	0.23	0.13	0.17
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,318	21,318	21,295	21,295	21,249	21,249

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that ethnolinguistic groups are primarily based on the Afrobarometer's information on the respondents' ethnicity rather than their native language. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.2: Fearon (2003) instead of Putterman and Weil (2010)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-0.44***	-0.38***	-0.74***	-0.60***	-0.57***	-0.35**
	(0.16)	(0.14)	(0.16)	(0.19)	(0.12)	(0.16)
R^2	0.16	0.21	0.19	0.23	0.12	0.17
Stratification	-0.62**	-0.43**	-0.81***	-0.60**	-0.75***	-0.63**
	(0.27)	(0.21)	(0.27)	(0.27)	(0.27)	(0.27)
Fractionalization	0.06	0.04	0.05	0.03	0.07	0.09
	(0.06)	(0.04)	(0.06)	(0.05)	(0.06)	(0.06)
Gini	-0.01	-0.05	-0.03	-0.07	0.03	0.01
	(0.05)	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)
Average wealth	-0.07*	-0.09**	-0.15**	-0.13**	-0.12**	-0.11**
	(0.04)	(0.05)	(0.06)	(0.07)	(0.06)	(0.05)
R^2	0.16	0.21	0.19	0.23	0.13	0.17
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,318	21,318	21,295	21,295	21,249	21,249

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that ethnolinguistic distances between pairs of individuals are computed using the formula by Fearon (2003) rather than Putterman and Weil (2010). Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.3: Lived poverty index instead of wealth index

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-0.35**	-0.31**	-0.56***	-0.35*	-0.41***	-0.10
	(0.14)	(0.14)	(0.18)	(0.18)	(0.13)	(0.16)
R^2	0.16	0.21	0.19	0.23	0.12	0.16
Stratification	-0.26	-0.29	-0.15	0.00	-0.54*	-0.24
	(0.29)	(0.25)	(0.35)	(0.36)	(0.29)	(0.33)
Fractionalization	-0.01	0.00	-0.11	-0.12	0.00	0.01
	(0.07)	(0.05)	(0.08)	(0.09)	(0.07)	(0.08)
Gini	-0.15	-0.07	-0.07	0.09	0.27**	0.20*
	(0.10)	(0.10)	(0.11)	(0.11)	(0.13)	(0.12)
Average wealth	-0.07	-0.04	-0.04	-0.00	0.09	0.07
	(0.05)	(0.05)	(0.07)	(0.07)	(0.08)	(0.07)
R^2	0.16	0.21	0.19	0.23	0.12	0.16
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	22,155	22,155	22,131	22,131	22,080	22,080

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that individual wealth and, consequently, economic distances between pairs of individuals are based on the lived poverty index. We reverse the scale of the lived poverty index, as higher values of the original index imply that people had to go without basic necessities more often. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.4: No towns and villages with fewer than eight respondents

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-0.47**	-0.44**	-0.83***	-0.70***	-0.65***	-0.35
	(0.20)	(0.19)	(0.21)	(0.23)	(0.17)	(0.22)
R^2	0.17	0.22	0.21	0.25	0.13	0.18
Stratification	-0.65*	-0.44	-0.93**	-0.76**	-0.83**	-0.76**
	(0.37)	(0.30)	(0.38)	(0.37)	(0.34)	(0.37)
Fractionalization	0.07	0.03	0.07	0.06	0.09	0.15*
	(0.08)	(0.06)	(0.08)	(0.07)	(0.08)	(0.08)
Gini	-0.02	-0.05	-0.04	-0.05	-0.02	-0.00
	(0.05)	(0.07)	(0.06)	(0.07)	(0.05)	(0.06)
Average wealth	-0.11***	-0.12***	-0.23***	-0.22***	-0.20***	-0.17***
	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
R^2	0.18	0.23	0.21	0.26	0.13	0.18
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	17,425	17,425	17,408	17,408	17,375	17,375

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that we drop all towns and villages where the information used to compute the index of ethnic stratification (and its components) is available for fewer than eight respondents. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.5: Non-binary trust variables

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-1.13***	-0.96***	-1.84***	-1.55***	-1.39***	-1.01***
	(0.35)	(0.31)	(0.33)	(0.35)	(0.29)	(0.35)
R^2	0.22	0.27	0.25	0.29	0.15	0.20
Stratification	-1.45**	-0.90**	-2.06***	-1.59***	-1.60**	-1.48**
	(0.57)	(0.42)	(0.50)	(0.50)	(0.65)	(0.62)
Fractionalization	0.13	0.04	0.12	0.07	0.08	0.16
	(0.14)	(0.10)	(0.11)	(0.12)	(0.15)	(0.13)
Gini	-0.01	-0.15	-0.03	-0.11	0.11	0.05
	(0.12)	(0.14)	(0.12)	(0.13)	(0.11)	(0.12)
Average wealth	-0.23**	-0.32***	-0.44***	-0.39***	-0.33***	-0.28**
	(0.10)	(0.11)	(0.13)	(0.13)	(0.12)	(0.11)
R^2	0.22	0.27	0.25	0.30	0.15	0.20
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,318	21,318	21,295	21,295	21,249	21,249

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that the dependent variables range from 0–3, indicating whether the respondent trusts the respective people "not at all" (0), "just a little" (1), "somewhat" (2), or "a lot" (3). Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.6: Probit estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-2.01***	-1.75***	-2.25***	-1.78***	-1.78***	-1.10**
	(0.68)	(0.62)	(0.53)	(0.61)	(0.40)	(0.49)
Stratification	-2.77**	-1.70*	-2.53***	-1.85**	-2.43***	-2.24**
	(1.08)	(0.94)	(0.86)	(0.87)	(0.87)	(0.87)
Fractionalization	0.30	0.09	0.16	0.11	0.22	0.36*
	(0.27)	(0.24)	(0.20)	(0.20)	(0.21)	(0.20)
Gini	-0.02	-0.25	-0.07	-0.21	0.06	0.02
	(0.24)	(0.31)	(0.19)	(0.22)	(0.14)	(0.15)
Average wealth	-0.39**	-0.49**	-0.48**	-0.43*	-0.35**	-0.32*
	(0.18)	(0.22)	(0.20)	(0.22)	(0.17)	(0.17)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,318	21,318	21,295	21,295	21,249	21,249

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that we use probit maximum likelihood models instead of linear probability models. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.7: Benchmark stratification as an additional regressor

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-0.62	-0.48	-0.18	-0.51	-1.06**	-1.04**
	(0.85)	(0.68)	(0.68)	(0.70)	(0.53)	(0.49)
Benchmark Strat.	0.23	0.15	-0.73	-0.11	0.53	0.86
	(0.96)	(0.78)	(0.83)	(0.87)	(0.68)	(0.68)
R^2	0.16	0.21	0.19	0.23	0.12	0.17
Stratification	-0.57	-0.46	-0.13	-0.50	-1.06**	-1.01**
	(0.80)	(0.68)	(0.68)	(0.73)	(0.53)	(0.49)
Benchmark Strat.	-0.09	0.05	-1.07	-0.20	0.34	0.43
	(0.92)	(0.90)	(0.86)	(1.00)	(0.70)	(0.74)
Fractionalization	0.08	0.04	0.09	0.05	0.06	0.10
	(0.06)	(0.05)	(0.07)	(0.07)	(0.07)	(0.07)
Gini	-0.02	-0.06	-0.03	-0.07	0.01	0.00
	(0.06)	(0.07)	(0.06)	(0.07)	(0.05)	(0.05)
Average wealth	-0.08**	-0.10**	-0.15**	-0.14**	-0.13**	-0.12**
	(0.04)	(0.05)	(0.07)	(0.07)	(0.06)	(0.05)
R^2	0.16	0.21	0.19	0.23	0.13	0.17
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,318	21,318	21,295	21,295	21,249	21,249

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that we add benchmark stratification (defined in Proposition 1) as an additional regressor. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.8: Stratification in cities with more than 50,000 inhabitants

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	0.02	-0.35	-0.74***	-1.27***	-0.40*	-0.89***
	(0.20)	(0.24)	(0.19)	(0.20)	(0.22)	(0.23)
R^2	0.14	0.19	0.16	0.22	0.14	0.19
Stratification	0.45	-0.29	-0.69	-1.91***	-0.06	-1.25***
	(0.44)	(0.56)	(0.55)	(0.69)	(0.46)	(0.43)
Fractionalization	-0.04	0.05	0.13	0.31	-0.01	0.12
	(0.15)	(0.23)	(0.17)	(0.32)	(0.11)	(0.16)
Gini	-0.40***	-0.24**	-0.46***	-0.23	-0.34**	-0.04
	(0.13)	(0.10)	(0.10)	(0.26)	(0.14)	(0.30)
Average wealth	-0.15	-0.02	-0.24*	0.16	-0.24*	0.30*
	(0.10)	(0.12)	(0.14)	(0.22)	(0.13)	(0.17)
R^2	0.15	0.20	0.17	0.22	0.14	0.20
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	6,983	6,983	6,966	6,966	6,959	6,959

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. There are two differences: First, respondents are included only if they live within the boundaries of a city with more than 50,000 inhabitants (which reduces the sample size by 67%). Second, we compute the index of ethnic stratification and its components at the level of these cities. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.9: District-level stratification

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-0.19	-0.27	-0.44**	-0.61**	-0.53***	-0.53***
	(0.21)	(0.29)	(0.20)	(0.28)	(0.14)	(0.20)
R^2	0.16	0.21	0.19	0.23	0.12	0.17
Stratification	-0.11	-0.18	-0.51*	-0.79*	-0.50*	-0.85**
	(0.31)	(0.34)	(0.31)	(0.41)	(0.29)	(0.34)
Fractionalization	0.01	0.01	0.08	0.11	0.04	0.15**
	(0.08)	(0.07)	(0.08)	(0.08)	(0.08)	(0.07)
Gini	-0.07	-0.06	-0.06	-0.04	-0.07	-0.01
	(0.06)	(0.07)	(0.06)	(0.08)	(0.06)	(0.08)
Average wealth	-0.14***	-0.18***	-0.23***	-0.21***	-0.20***	-0.18**
	(0.05)	(0.06)	(0.07)	(0.08)	(0.08)	(0.07)
R^2	0.16	0.21	0.19	0.23	0.13	0.17
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,397	21,397	21,373	21,373	21,328	21,328

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that we compute the index of ethnic stratification and its components at the level of districts (ADM2 regions). Standard errors are adjusted for two-way clustering at the level of provinces (ADM1 regions) and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.10: Province-level stratification

	(1)	(2)	(3)
Dep. variable:	Trust in relatives	Trust in neighbors	Trust in others
Stratification	-0.23	-0.41	-0.57***
	(0.16)	(0.25)	(0.19)
R^2	0.16	0.19	0.12
Stratification	-0.18	-0.35	-0.04
	(0.42)	(0.52)	(0.53)
Fractionalization	-0.01	0.04	-0.10
	(0.11)	(0.12)	(0.15)
Gini	-0.10	-0.21***	-0.24***
	(0.07)	(0.08)	(0.09)
Average wealth	-0.03	-0.28***	-0.25**
	(0.07)	(0.10)	(0.12)
R^2	0.16	0.19	0.12
Controls	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes
Observations	21,401	21,377	21,332

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. There are two differences: First, we compute the index of ethnic stratification and its components at the level of provinces (ADM1 regions). Second, we show no specifications with province fixed effect, as these fixed effects would absorb the index of ethnic stratification and its components. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.11: Afrobarometer rounds 4 and 5 (and less informative wealth index)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-0.24***	-0.22***	-0.59***	-0.47***	-0.30***	-0.23**
	(0.07)	(0.07)	(0.16)	(0.17)	(0.10)	(0.09)
R^2	0.13	0.16	0.19	0.23	0.13	0.16
Stratification	-0.46***	-0.34**	-0.54*	-0.41	-0.39**	-0.33**
	(0.17)	(0.15)	(0.28)	(0.26)	(0.20)	(0.16)
Fractionalization	0.08*	0.05	0.01	-0.01	0.04	0.05
	(0.04)	(0.04)	(0.07)	(0.06)	(0.05)	(0.05)
Gini	0.03	0.00	-0.01	-0.00	-0.01	-0.04
	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.04)
Average wealth	-0.04	-0.05	-0.13**	-0.07	-0.05	-0.05
	(0.03)	(0.04)	(0.06)	(0.06)	(0.06)	(0.05)
R^2	0.13	0.16	0.19	0.23	0.13	0.16
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-round FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Rounds included	4 5	4 5	5	5	4 5	4 5
Observations	34,823	34,823	21,457	21,457	34,704	34,704

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. There are three differences: First, we include respondents from Afrobarometer surveys of rounds 4 and 5. Second, we compute individual wealth and, consequently, economic distances between pairs of individuals based on fewer information on assets and housing quality (see Appendix 1.C.1 for details). Third, we include country-round fixed effects. Standard errors are adjusted for two-way clustering at the level of province-round and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.12: Afrobarometer rounds 3-5 (and lived poverty index)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification	-0.20**	-0.17**	-0.43***	-0.34**	-0.29***	-0.16
	(0.08)	(0.09)	(0.15)	(0.15)	(0.10)	(0.11)
R^2	0.13	0.15	0.17	0.19	0.13	0.16
Stratification	-0.25	-0.24	-0.36	-0.38	-0.44*	-0.20
	(0.16)	(0.16)	(0.24)	(0.26)	(0.24)	(0.24)
Fractionalization	0.02	0.02	-0.01	0.01	0.03	0.00
	(0.04)	(0.03)	(0.06)	(0.07)	(0.06)	(0.06)
Gini	-0.04	-0.01	-0.05	0.00	0.17*	0.09
	(0.06)	(0.06)	(0.09)	(0.09)	(0.09)	(0.08)
Average wealth	-0.01	-0.02	-0.04	-0.05	0.10*	0.07
	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)	(0.05)
R^2	0.13	0.15	0.17	0.19	0.13	0.16
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-round FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Rounds	3 4 5	3 4 5	3 5	3 5	4 5	4 5
Observations	47,457	47,457	33,769	33,769	35,659	35,659

Notes: This table presents a robustness test on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. There are three differences: First, we include respondents from Afrobarometer surveys of rounds 3–5. Second, we compute individual wealth and, consequently, economic distances between pairs of individuals based on the lived poverty index. We reverse the scale of the lived poverty index, as higher values of the original index imply that people had to go without basic necessities more often. Third, we include country-round fixed effects. Standard errors are adjusted for two-way clustering at the level of province-round and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.13: Alternative trust measures

	(1)	(2)	(3)	(4)	(5)
Dep. variable:	Trust in local assembly		Generalized trust		
Stratification	-0.37*	-0.46**	-0.18	-0.20	-0.25
	(0.21)	(0.18)	(0.16)	(0.13)	(0.18)
R^2	0.13	0.17	0.11	0.17	0.11
Stratification	-0.36	-0.05	-0.03	0.03	-0.29
	(0.30)	(0.34)	(0.24)	(0.21)	(0.40)
Fractionalization	0.04	-0.09	-0.03	-0.05	0.07
	(0.07)	(0.08)	(0.05)	(0.06)	(0.09)
Gini	-0.12	-0.11	-0.02	-0.04	-0.19*
	(0.10)	(0.08)	(0.07)	(0.04)	(0.10)
Average wealth	-0.16**	-0.13**	-0.13**	-0.10**	-0.27***
	(0.07)	(0.06)	(0.06)	(0.04)	(0.10)
R^2	0.13	0.17	0.11	0.17	0.11
Mean dep. variable	0.49	0.49	0.20	0.20	0.20
Controls	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No
Level of indices	Local	Local	Local	Local	Provinces
Observations	19,038	19,038	20,972	20,972	20,972

Notes: This table presents regressions identical to those in Table 1.2 (panels A and B) except of the use of dependent variables that are based on alternative trust questions asked by Afrobarometer surveys of round 5. Trust in local assembly is based on the question: “How much do you trust each of the following, or haven’t you heard enough about them to say: Your metropolitan, municipal or district assembly?” We transform the answers to a binary variable, which is equal to one if respondents answer “a lot” or “somewhat,” and zero if they answer “not at all” or “just a little.” Generalized trust is based on the question: “Generally speaking, would you say that most people can be trusted or that you must be very careful in dealing with people?” It is already a binary variable in the Afrobarometer data. Trust in the local assembly and generalized trust are the dependent variables in columns (1)–(2) and (3)–(5), respectively. In addition, in column (5) we compute the index of ethnic stratification and its components at the level of provinces (ADM1 regions) instead of towns and villages. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.14: Effect heterogeneity along urban-rural divide (Afrobarometer classification)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification (S)	-0.35*	-0.36**	-0.80***	-0.61***	-0.70***	-0.50***
	(0.18)	(0.14)	(0.22)	(0.23)	(0.16)	(0.19)
S × Urban	-0.13	0.01	0.06	0.03	0.12	0.22
	(0.26)	(0.25)	(0.33)	(0.34)	(0.29)	(0.32)
R^2	0.16	0.21	0.19	0.23	0.12	0.17
Stratification (S)	-0.19	0.09	-0.53	-0.06	-0.92**	-0.46
	(0.34)	(0.28)	(0.46)	(0.45)	(0.38)	(0.35)
S × Urban	-0.65	-0.88*	-0.52	-0.98	0.00	-0.51
	(0.49)	(0.48)	(0.57)	(0.65)	(0.49)	(0.55)
Fractionalization (F)	-0.01	-0.08	-0.06	-0.12	0.06	0.02
	(0.08)	(0.07)	(0.10)	(0.09)	(0.11)	(0.09)
F × Urban	0.14	0.21*	0.20	0.28**	0.04	0.18
	(0.11)	(0.11)	(0.13)	(0.14)	(0.13)	(0.13)
Gini (G)	-0.00	-0.09	0.04	-0.04	0.03	0.01
	(0.07)	(0.07)	(0.07)	(0.09)	(0.05)	(0.06)
G × Urban	-0.06	0.06	-0.20**	-0.11	-0.00	-0.01
	(0.09)	(0.10)	(0.09)	(0.11)	(0.09)	(0.10)
Average wealth (μ)	-0.09**	-0.14***	-0.10*	-0.12**	-0.08	-0.10*
	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)	(0.06)
μ × Urban	0.01	0.08	-0.11*	-0.05	-0.09	-0.03
	(0.05)	(0.06)	(0.07)	(0.08)	(0.07)	(0.08)
R^2	0.16	0.21	0.19	0.23	0.13	0.17
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,318	21,318	21,295	21,295	21,249	21,249

Notes: This table explores effect heterogeneity along an urban-rural divide based on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that we add interaction terms between our index of ethnic stratification and its components, and a dummy variable indicating whether the respondent lives in a location that Afrobarometer classified as urban. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.15: Effect heterogeneity along urban-rural divide (geo-spatial data)

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Trust in relatives		Trust in neighbors		Trust in others	
Stratification (S)	-0.50**	-0.37**	-0.78***	-0.57**	-0.53***	-0.29
	(0.19)	(0.15)	(0.21)	(0.23)	(0.15)	(0.18)
S × City	0.14	0.02	0.04	-0.06	-0.21	-0.23
	(0.18)	(0.19)	(0.24)	(0.29)	(0.24)	(0.30)
R^2	0.16	0.21	0.19	0.23	0.12	0.17
Stratification (S)	-0.71**	-0.41*	-0.73*	-0.38	-0.52	-0.32
	(0.28)	(0.23)	(0.38)	(0.38)	(0.36)	(0.32)
S × City	0.39	0.16	-0.23	-0.46	-0.63	-0.89
	(0.49)	(0.52)	(0.62)	(0.66)	(0.55)	(0.58)
Fractionalization (F)	0.07	0.02	-0.01	-0.04	0.01	0.02
	(0.06)	(0.05)	(0.08)	(0.07)	(0.09)	(0.08)
F × City	-0.02	0.05	0.16	0.19	0.15	0.24*
	(0.12)	(0.12)	(0.12)	(0.13)	(0.13)	(0.12)
Gini (G)	0.01	-0.04	-0.02	-0.06	0.02	0.02
	(0.05)	(0.06)	(0.07)	(0.07)	(0.04)	(0.05)
G × City	-0.16**	-0.17**	-0.11	-0.14	-0.04	-0.11
	(0.07)	(0.08)	(0.11)	(0.12)	(0.09)	(0.09)
Average wealth (μ)	-0.09**	-0.09*	-0.13**	-0.13**	-0.12**	-0.10**
	(0.04)	(0.05)	(0.06)	(0.06)	(0.06)	(0.05)
μ × City	0.02	-0.01	-0.03	-0.02	-0.02	-0.02
	(0.02)	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)
R^2	0.16	0.21	0.19	0.23	0.13	0.17
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	21,318	21,318	21,295	21,295	21,249	21,249

Notes: This table explores effect heterogeneity along an urban-rural divide based on the main results reported in Table 1.2 (panels A and B); see the corresponding table notes for details. The only difference is that we add interaction terms between our index of ethnic stratification and its components, and a dummy variable indicating whether the respondent lives within the boundaries of a city with more than 50,000 inhabitants. Standard errors are adjusted for two-way clustering at the level of provinces and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Table 1.D.16: Crime and conflict in rounds 5 and 6

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable:	Fear of crime		Actual crime		Violent conflict	
Units of obs.:	Ind. respondents		Ind. respondents		Towns/villages	
Stratification	0.35***	0.25**	0.16	0.11	0.84***	0.58**
	(0.12)	(0.10)	(0.11)	(0.12)	(0.32)	(0.25)
R^2	0.11	0.13	0.07	0.09	0.50	0.61
Stratification	0.31	0.20	-0.15	-0.16	1.28**	0.91**
	(0.21)	(0.21)	(0.19)	(0.19)	(0.54)	(0.46)
Fractionalization	-0.00	0.00	0.09*	0.07	-0.12	-0.11
	(0.06)	(0.05)	(0.05)	(0.04)	(0.10)	(0.09)
Gini	0.03	0.01	0.05	0.04	-0.02	0.02
	(0.05)	(0.06)	(0.05)	(0.05)	(0.07)	(0.06)
Average wealth	0.05	0.03	-0.00	-0.01	0.11**	0.11**
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
R^2	0.10	0.13	0.07	0.09	0.50	0.61
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-round FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-group FE	Yes	Yes	Yes	Yes	No	No
Province FE	No	Yes	No	Yes	No	Yes
Observations	43,693	43,693	43,784	43,784	5,057	5,057

Notes: This table presents a robustness test on the results reported in Table 1.4; see the corresponding table notes for details. Like in Table 1.4, the units of observation are individual respondents in columns (1)–(4) and towns and villages in columns (5)–(6). There are two differences to Table 1.4: First, we include respondents from Afrobarometer surveys of rounds 5 and 6 (rather than just round 5). Second, we include country-round fixed effects. Standard errors are adjusted for two-way clustering at the level of province-round and country-ethnolinguistic group interactions. ***, **, * indicate significance at the 1, 5 and 10%-level, respectively.

Chapter 2

Born in the right place? Ministers, foreign aid, and infant mortality

Joint with Philine Widmer

2.1 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; Bommer et al., 2019; Dreher et al., 2019). 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 the cabinet is merely symbolic remains unclear. Motivated by this question, we examine whether cabinet members can engage in favoritism targeted at their birth region.

Focusing on favoritism related to health, we study whether more health aid is allocated to the birth regions of health ministers and other important ministers. Additionally, we investigate the effect on neonatal and infant mortality, i.e., whether children born in the same region as health ministers are less likely to die before reaching the age of one month or one year (World Health Organization, 2019a; World Health Organization, 2019b), and whether such changes in mortality rates are associated with the amount of aid allocated to these birth regions.

Our focus on health comes with several advantages. Health-related aid flows constitute a sizeable share of World Bank aid projects (around 30%), which leaves us with a sufficient number of projects for meaningful statistical analysis. Additionally, it is straightforward to assign health projects to ministers who might be in charge (e.g., health ministers, sanitation ministers). We can thus investigate if being in charge of the relevant portfolio (in this case, health) increases the likelihood that a cabinet member is able to influence the allocation of funds. Moreover, subnational health data is available for various African countries in different years, as provided by the Demographic and Health Surveys Program. Hence, we not only study the allocation of health-related funds but also directly analyze health outcomes. We focus on neonatal and infant mortality, which are often used as proxies for population health outcomes in settings where health data is scarce.

We compile a novel data set on cabinet members in 47 African countries between 2001 and 2014. We cover all African countries, except for those with less than 1 million inhabitants and those that did not receive any World Bank aid during the sample period. 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 75% of cabinet members. To our knowledge, we are the first to offer data on the birthplaces of African cabinet members with almost continent-wide coverage.[38] We match the birthplace information to administrative boundaries on the first subnational level (ADM1) according to the GADM database of Global Administrative Areas.[39] Based on this data, we build a panel data set of ADM1 regions, indicating which area is the birth region of which cabinet member in a given year. We combine our new data with geocoded data on health-related flows from the World Bank, which is available from AidData. The flows captured in this data set come from the International Development Association (IDA) and International Bank for Reconstruction and Development (IBRD) lending lines. AidData provides the amounts committed (in current USD) and identifies the targeted sector (health, education, infrastructure, etc.). We thus know if and how much health aid flows to an ADM1 area in a given year.[40]

As a case in point, consider Mali. From 2008 to 2010, Oumar Ibrahima Touré, born in the Timbuktu province, was in the health ministry. While he was in power, the Timbuktu province received around USD 25 million in World Bank health aid per year. Meanwhile, other regions only received an average of USD 2 million per year. In 2011, Touré was succeeded by Diallo Madeleine Ba, who originates from the Mopti province. Mopti had only received a yearly USD 2 million during Touré's term. With Ba entering office, Mopti received USD 14 million per year, while the health aid committed to Timbuktu fell to USD 3 million. Figure 2.1 illustrates the example of Mali.

— Figure 2.1 about here —

We analyze whether there is a pattern behind such observations suggesting health allocations benefit ministerial birth regions using our ADM1 panel data set. To control for region-specific time-invariant characteristics, such as a region's size, historical legacy, or natural resource endowment, we include ADM1 fixed effects. We also include country-year fixed effects to account for shocks and trends affecting the whole country (such as economic downturns). We show that regions

[38] 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 do not attain close-to-continent-wide coverage but focus on 15 countries (from independence to 2004).

[39] ADM1 regions correspond to provinces in many countries.

[40] For a brief overview of the allocation of World Bank aid and potential shortcomings, see Appendix 2.A.

receive around twice as much health aid (but do not receive more non-health aid) when the current health minister is from that region, compared to when they are not. Our estimates for health ministers are similar when including indicators for regions where the country leader, a key minister (such as economics and finance ministers), or any cabinet member was born. We also find evidence that health aid increases in the birth regions of key ministers, but the effect is only about half as much as that for health ministers. In line with Dreher et al. (2019), our results suggest that country leaders are not able to divert World Bank health aid to their birth region. Also, it seems that cabinet members other than health and key ministers do not divert health aid to their birth region. This is not surprising, as World Bank projects are typically negotiated between World Bank staff, line ministers, and powerful ministers such as ministers of economics and finance.

We believe that a causal interpretation of our findings is warranted. First, given our short observation period, there is likely little room for the confounding characteristics of regions (such as the general propensity for it to be the home of cabinet members or population densities) to change fundamentally – lending credibility to our fixed-effects approach. Second, we show that regions that send health ministers do not receive more health aid in previous years, hence suggesting that health ministers influence health aid distribution and not vice versa.

Our results are consistent across alternative specifications and robustness tests. In addition to the ADM1-level panel data set, we construct a panel data set where individual cabinet members are the units of observation. That is, we observe a given individual in all years they hold a cabinet position. Thus, we can compare the allocation of health aid to a cabinet member's birth region when they are the health minister to when they hold another designation. We find that the same cabinet member attracts more aid to their birth region as a health minister than in other cabinet positions. This finding suggests that the power health ministers yield over health aid allocation is tied to holding a health-related designation and not to particular kinds of individuals selecting into the health ministry.

To study the effect (health) ministers have on neonatal and infant mortality in their birth regions, we use data from the Demographic and Health Surveys (DHS). These surveys contain retrospective information on the health of the interviewed women and their children. We approximate a quasi-experimental setting by constructing a data set where a mother is the panel unit and each of her children is one unit of observation. Thus, we effectively compare siblings (following, e.g., Kotsadam et al., 2018 and Bruederle and Hodler, 2019).

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 compared to their siblings born in other years: infant and neonatal mortality rates are 3.1 and 4.6 deaths per 1,000 live births lower, respectively, in health ministers' birth regions. Hence, health ministers not only influence the allocation of funds but also health outcomes directly.

Next, we seek to understand whether lower mortality in health ministers' birth regions can be attributed to higher health aid flows. We do not find any evidence that the negative effect of the health minister on mortality rates increases with the health aid amounts committed to his or her region.

In sum, we draw two main conclusions from our work. First, not only country leaders but also cabinet members engage in favoritism: we document that health ministers can divert health aid to their birth regions and improve health outcomes. Our evidence underscores the suggestion that there is, indeed, more to African politics than the "big man" theory suggests. Second, as we do not find that improved health outcomes in ministerial birth regions are associated with increased health aid, our results point to the existence of channels other than World Bank aid through which ministers direct resources to their birth regions.

From a theoretical point of view, the increased allocation of health aid to health ministers' birth regions could solely reflect political capture, but it could also be consistent with aid effectiveness considerations. The latter could be the case if health ministers have informational or monitoring advantages in their birth region (e.g., they know about project locations where aid is likely to be highly effective or can better monitor project implementations in their birth region). In our study, the absence of an association between higher health aid and lower mortality in health ministers' birth regions is indicative of political capture. However, we cannot rule out that effectiveness considerations do play a role in the allocation process. We do not find statistically significant effects of health aid on mortality rates:[41] hence, if the effects of health aid at the ADM1 level are too small to be detected, such motives may simply remain undiscovered.

Our work contributes to various strands of the 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" predicts. 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 but translates into policy outcomes.

Moreover, we contribute to the broader literature on ethnic and regional favoritism. Concerning aid allocation, Dreher et al. (2019) show that more aid from China flows 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.

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). We complement the broader literature on ethnic and regional favoritism by considering cabinet members with an almost continent-wide coverage (and extending the study of infant mortality to 36 countries).

Lastly, we add to the literature on the determinants of aid allocation, focusing on cross-country allocation as well as within-country allocation. In their prominent country-level contribution, Alesina and Dollar (2000) find that donors' (geo-) political considerations predict foreign aid flows. There is evidence that countries holding a non-permanent seat in the UN Security Council receive more US aid

[41] Other contributions, however, have shown that foreign aid can decrease mortality at the subnational level (Kotsadam et al., 2018; Wayoro and Ndikumana, 2019; Cruzatti et al., 2020; Martorano et al., 2020; Chapter 3 of this thesis).

flows (Kuziemko and Werker, 2006) and more World Bank projects (Dreher et al., 2009). Faye and Niehaus (2012) present evidence that recipient administrations closely aligned with a donor receive more aid during election years, while the least-aligned recipients receive less. Similarly, Kilby (2009) finds that the conditionality of World Bank structural adjustment loan disbursements is less stringent for countries politically aligned with the US.

More recent research has focused primarily on the subnational allocation of aid. Nunnenkamp et al. (2017) analyze aid allocation in India and conclude that evidence for needs-based allocation is weak. Instead, they find that the World Bank targets districts where foreign direct investors may profit from infrastructure-related projects. Briggs (2014) and Jablonski (2014) observe the existence of strong political influence over the location of aid projects in Kenya. Francken et al. (2012) show that political factors have more influence over government aid than they do over aid from agencies. The aforementioned studies by Bommer et al. (2019) and Dreher et al. (2019) provide evidence that at least some forms of foreign aid are more likely to flow to country leaders' birth regions. In line with these contributions, our results suggest that political motives might influence the allocation of (World Bank) aid.

The remainder of the paper is structured as follows. In the next section, we describe our data. In Section 2.3, we detail the estimation strategy. The results follow in Section 2.4. We conclude in Section 2.5.

2.2 Data and data processing

We build a data set on the birthplaces of cabinet members in 47 African countries between 2001 and 2014. All African countries with more than 1 million inhabitants which received World Bank aid at least once over the observation period are included.[42] We combine the birthplace data with georeferenced data on World Bank aid projects from AidData (2017) and construct a panel data set with ADM1 regions as units of observation. Additionally, we construct a panel data set with

[42] Two countries with more than 1 million inhabitants never received any aid from the World Bank between 2001 and 2014, namely Libya and Somalia. Appendix 2.B lists all countries in our sample.

individual ministers as units of observation.[43] Finally, we use the individual-level data provided by the Demographic and Health Surveys (ICF, 2001-2014) to create a panel data set with children as the units of observation.

In the following, we present our data in more detail. The beginning of our observation period is given by the CIA World Factbook (2018), which has been providing lists of all cabinet members and their designations since 2001, while it ends with the last available data on World Bank projects in 2014.

2.2.1 Birthplaces of cabinet members

Data on cabinet members at a given point in time comes from the World Factbook. For almost all countries around the world, the World Factbook provides monthly lists in pdf format of all cabinet members, 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 do some manual cleaning to identify duplicate names.[44] We are left with 5,596 unique individuals.[45]

We tag each designation along 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.).[46] Often, cabinet members are

[43] This additional data set is described in Appendix 2.D.

[44] Inconsistencies 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.

[45] Without the six-month threshold that we apply, we would be left with 6,197 individuals.

[46] For 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.

mapped to more than one indicator in a given year:[47] 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 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 president based on the World Factbook are tagged as head 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 75% of all cabinet members (4,207 out of 5,596). 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 2.C.1 in Appendix 2.C. Con-

[47]This applies to 35% of all cabinet member-years.

cerning health ministers, the coverage is 73% (118 out of 162). Figure 2.2 shows the ADM1 regions that were the birth region of a health minister at least once in our sample period. There are 115 changes in health ministers, 49 in country leaders, 351 in key ministers, and 427 in any cabinet position.

— Figure 2.2 about here —

2.2.2 World Bank health project data

To construct variables capturing the allocation of health aid on the ADM1 level, we use georeferenced data on World Bank projects from AidData. This data 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.[48] 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 the 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, as we suspect that this is when the cabinet members exercise their power. On average, a country in our sample receives USD 25 million of health-related flows per year.[49]

[48] AidData 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.

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

2.2.3 ADM1 panel data set

Based on the information on the cabinet members' birth regions and the location of health aid projects, we build a panel data set with ADM1 regions as our unit of observation. Our sample comprises 737 ADM1 regions[50], resulting in a panel of 10,318 observations (737 $\times$14 years).

Combining the information on the ministers' designations and their birthplaces, we build an indicator 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 2.2.1, allows us to easily adjust the exact definitions of $healthmin_{ict}$, $keymin_{ict}$ and $cabinet_{ict}$. There are several designation indicators that 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* and *population*. In robustness checks, we also include ministers with these latter cabinet indicators. Furthermore, we include all cabinet positions, also, e.g., vice-ministers, in our main analyses. In robustness tests, we only include ministers in the strict sense, excluding vice-ministers, etc.

Based on AidData, we construct two variables: an indicator variable of whether a region i receives any health aid in a given year t ($healthaidD_{ict}$, with D for dummy), and the aggregated amount of all projects assigned to i in t ($healthaid_{ict}$). 202 out of 647 country-years in our sample receive some health aid. 423 out of 737 regions receive some health-related aid at least once in our sample period.

Table 2.1, Panel A, presents summary statistics for the regional panel data set. On average, a region is the birth region of the respective cabinet member

[50] From 2001 to 2011, the ADM1 regions of what is now South Sudan are included as ADM1 regions of Sudan.

in a given year with the following probability: 4% for health ministers, 5.5% for leaders, 33.5% for key ministers, and 64.5% for any cabinet member. On average, a region receives around USD 2 million of health aid in a given year, and the likelihood of a region receiving any such flows is 10.6%.

2.2.4 Birth panel data set

We combine the region-level panel data set with individual-level data from the Demographic and Health Surveys (DHS) to build a panel data set with children as the units of observation. Due to the availability of the DHS, our sample for this analysis consists of 36 countries coming from 88 surveys.[51] 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 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. There are 1,173,766 children from 430,039 mothers for which we have information on neonatal mortality and 1,028,780 children from 384,150 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. Table 2.1, Panel B, shows average neonatal (infant) mortality in our sample is 30.4 (61.2) deaths per 1,000 live births. Table 2.1, Panel B, also provides summary statistics for the same cabinet member and health aid variables as Panel A for the birth panel data set.

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, second, etc. Moreover, we use a multiple birth indicator (twins, triplets, etc.). In case of multiple births, all children are considered unique observations.

[51] See Appendix 2.B for a list of countries with information provided by the DHS.

—Table 2.1 about here —

2.3 Econometric framework

2.3.1 Health ministers' effect on aid allocation

We suppose a log-linear relationship between the amount of health aid allocated to an ADM1 region, $healthaid_{ict}$, and the indicator for a health minister's birth region, $healthmin_{ict}$:

$$ln(healthaid_{ict}) = \alpha_i + \beta_{ct} + \gamma\ healthmin_{ict} + \vartheta_{ict}$$

where α_i represents region fixed effects and β_{ct} country-year fixed effects. The coefficient of interest is γ, the effect of a region i being the health minister's birth region. In our main specification, 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:

$$\begin{aligned} ln(healthaid_{ict}) = \alpha_i + \beta_{ct} + \gamma\ healthmin_{ict} + \delta\ leader_{ict} \\ +\eta\ keymin_{ict} + \psi\ cabinet_{ict} + \vartheta_{ict} \end{aligned} \tag{2.1}$$

Given that many regions do not receive aid in a given year, $healthaid_{ict}$ often takes the value zero. We, therefore, do not estimate the log-linear relationship in equation 2.1 directly but rely on its exponential form by running a Poisson pseudo maximum likelihood (PPML) regression:[52]

$$\begin{aligned} healthaid_{ict} = exp[\alpha_i + \beta_{ct} + \gamma\ healthmin_{ict} + \delta\ leader_{ict} \\ +\eta\ keymin_{ict} + \psi\ cabinet_{ict}] + \varepsilon_{ict} \end{aligned} \tag{2.2}$$

Santos Silva and Tenreyro (2006) show that PPML is superior to simple OLS and Tobit approaches with heteroskedasticity and many zero observations in the

[52] To estimate equation (2.2), we rely on the Stata package *ppmlhdfe*, which implements PPML estimation with multiple high-dimensional fixed effects and allows for multiway-clustering (Correia et al., 2019a).

data.[53] 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). We interpret the coefficients from equation (2.2) as semi-elasticities. We use robust standard errors clustered at the country level. To analyze the extensive margin, we use a linear probability model in the spirit of equation (2.1) but with $healthaidD_{ict}$ instead of $healthaid_{ict}$ as the outcome.

There may be some concerns regarding a causal interpretation of γ. The amount of (health) aid allocated to a region and the likelihood it will be the birth region of a cabinet member might be co-determined; e.g., by local economic development and population density, inter-regional differences in capabilities to coordinate political action, and the size of the region. This concern motivates the region fixed effects: arguably, most of these confounders are time-invariant or at least do not change considerably over our 14-year observation period. General trends affecting a country as a whole will be absorbed by the country-year fixed effects. Time-varying variables, such as the discovery of natural resources, resource price shocks, or natural disasters affecting subnational areas to highly varying degrees, are not accounted for. In addition, region-specific economic trends could distort the effects: even if donors allocated aid based on need only, a positive (negative) effect of being the ministerial birth region is observed if areas send ministers when they are relatively poor (rich). We deem it unlikely that such differential, time-varying influences are sufficiently large and widespread to introduce any meaningful bias. Nevertheless, we present some checks to alleviate these concerns. Events such as localized natural disasters might lead to more health-related aid being allocated to the affected region 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 show that less health aid is allo-

[53] 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, Poisson regression becomes Poisson pseudo maximum likelihood (PPML) regression. Given that no distributional assumption is required for the dependent variable, the application of PPML regression is not restricted to count data but can be applied to any dependent variable with non-negative values (Santos Silva and Tenreyro, 2006; Correia et al., 2019a).

cated to a health minister's birth region in their first year in office, thus providing evidence against the aforementioned channel. Furthermore, 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.[54] Another concern could be that aid-induced development progress leads to a subnational area becoming a ministerial birth region. We address this concern by testing whether a region already receives more health aid in the two years before it sends a health minister, which is not the case. In sum, we believe that a causal interpretation of our allocation effects is justified.

A priori, the sign of γ in equation (2.2) 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 allocation decisions. A negative effect could point to donors that deliberately punish ministerial birth regions, which could be the case if donors distrust corrupt elites and aim at circumventing them (see Knack, 2014). It 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 would be consistent with pure political capture, as well as with effectiveness considerations. The latter would be the case if health ministers had information or monitoring 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).

2.3.2 Health ministers' effect on mortality rates

In the last part of our paper, we 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

[54] In Table 2.E.1 in Appendix 2.E, we show country-level regressions of an indicator for a change in the health minister, the country leader, key ministers, and any cabinet members on an indicator for a natural disaster in the last year. The coefficients are negative for health ministers and key ministers, and none of them are significant. The data on disasters comes from the EM-DAT database (Guha-Sapir, n.d.).

estimate the following equation:

$$mortality_{kpict} = \phi_p + \beta_{ct} + \gamma\ healthmin_{ict} + \Omega control_{kpict} + \vartheta_{kpict} \tag{2.3}$$

$mortality_{kpict}$ is either $neonatal_{kpict}$ or $infant_{kpict}$. ϕ_p are mother fixed effects that control for everything that should remain roughly constant over time for the same mother, such as the mother's education, religion, and whether they live in a rural or urban area. Including mother fixed effects (following, among others, Kotsadam et al., 2018, and Bruederle and Hodler, 2019), we estimate the treatment effect for children with at least one sibling. $control_{kpict}$ is a vector of child-level controls, as discussed in Section 2.2.4. In the spirit of the placebo test in the allocation analysis, we test whether mortality decreases in the two years before the health minister from this region comes into power.

Potentially, lower neonatal and infant mortality rates in health ministers' birth regions might be explained by these regions receiving more health aid. To investigate this possible channel, we include the interaction between the health minister dummy variable and standardized health aid:

$$\begin{aligned} mortality_{kpict} = \phi_p + \beta_{ct} + \gamma\ healthmin_{ict} + \kappa\ healthaid_{ict} \\ +\zeta\ healthmin_{ict} \times healthaid_{ict} + \Omega\ control_{kpict} + \vartheta_{kpict} \end{aligned} \tag{2.4}$$

If the reduction in mortality in health ministers' birth regions can be (partly) explained by the health aid allocated there, we expect a negative sign for ζ: the effect of the health minister should be larger in regions that receive more health aid. As the allocation of health aid is endogenous, one should be careful when interpreting coefficients κ and ζ. However, as we include mother fixed effects and thus compare siblings born at different levels of regional aid with or without the health minister originating from their region, we seek to approximate a quasi-experimental setting.

2.4 Results

2.4.1 Health ministers' effect on aid allocation

Table 2.2 presents the effect of a region being the health minister's birth region on the allocation of health aid to this region in the subsequent year. Columns (1) and (2) show that health ministers' birth regions are not more likely to receive health aid in the subsequent year when the region-born health minister is in cabinet than when he is not, i.e., we do not find an effect on the extensive margin. The coefficient is positive, and the size remains similar when including the country leader, key minister, and cabinet member controls, but it is not statistically significant. However, turning to the *amount* of health aid a region receives in column (3), we find that a region receives 85% more health aid when last year's health minister originated from the region, with a p-value lower than 0.10.[55] The size of the coefficient slightly increases when introducing the country leader, key minister, and cabinet member controls in column (4) (and is now statistically significant at the 5% level), suggesting that the birth region of a health minister receives around 100% more health aid in the subsequent year.

Hence, health ministers do not appear to have any influence over the allocation of health aid on the extensive margin, at least not measurably so. However, they can influence the amount of funds allocated. By construction, we cannot detect health ministers' influence on within-project allocation to different project location sites (recall our assumption that, for a given project, the amount is distributed uniformly across locations, see Section 2.2.2). Therefore, favoritism could manifest itself if health ministers influence the overall funds allocated to a project where their birth region is one of the project locations, or if they reshuffle projects such that those with more pre-determined overall funding are (also) assigned to their birth region.

When examining the role of cabinet members other than health ministers, we do not find an effect for the country leader (Table 2.2, columns 2 and 4). This result is in line with Dreher et al. (2019), who find that country leaders do not

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

influence the allocation of World Bank aid. Similarly, other cabinet members do not seem to exert influence, neither, except for key ministers: Column (4) suggests that a region receives around 40% more health aid if it is a key minister's birth region. In Table 2.E.2 in Appendix 2.E we replicate columns (2) and (4) from Table 2.2 but looking at the different key ministers separately; i.e., we split the key minister indicator into indicators for President/Prime Minister other than the country leader, economics/finance/development, industry/trade, agriculture, justice, and foreign affairs minister. The economics/finance/development, industry/trade, and agriculture ministers seem to be the key ministers influencing health aid allocation.

The effects we find for health and key ministers and the non-effects for country leaders and any cabinet positions are in line with the fact that World Bank projects are typically negotiated between World Bank staff and line ministers and powerful ministers such as ministers of economics and finance. The results suggest that health ministers and, to a lesser extent, key ministers use these negotiations to allocate more funds to their birth regions.

—Table 2.2 about here —

Placebo test In Table 2.3, we test whether a region receives more health aid *before* sending a health minister. A positive coefficient of $prehealthmin_{ict}$, an indicator equal to one in the two years before a region sends a health minister, would invalidate a causal interpretation of our findings. The coefficient of $prehealthmin_{ict}$ is negative and not statistically significant at conventional levels in all four columns. Furthermore, in columns (3) and (4), the effect of $healthmin_{ic,t-1}$ is even larger than in Table 2.2 and statistically significant at the 1% level. The test for the equality of $healthmin_{ic,t-1}$ and $prehealthmin_{ict}$ is rejected at the 5% level. These results strengthen our causal interpretation of health ministers' effect on the allocation of health aid.

—Table 2.3 about here —

Robustness We present several robustness checks in Appendix 2.E. In Table 2.E.3, we find a qualitatively similar result for the effect of a health minister's birth region on the amount of health aid allocated in the same year (with a smaller coefficient and lower estimation precision) and evidence that the health minister has some impact on whether any health aid is distributed to his region. Investigating the effect of a health minister's birth region on health aid allocation two years later in Table 2.E.4, we find similar results as for the effect in one year. The main results are largely unchanged when we include public health, HIV/AIDS, sanitation, and population ministers in our health minister definition in Table 2.E.5. The coefficients are somewhat smaller than in Table 2.2. By only considering health ministers, key ministers, and cabinet members when they are ministers strictly speaking (excluding vice ministers, etc.) in Table 2.E.6, the effects are similar to our main results.

As a further robustness test, we investigate whether the same cabinet member attracts more health-related flows when acting as the health minister. For this purpose, we construct a panel data set where individuals (who are in the cabinet for at least two years) are the units of observation, and we employ person fixed effects to compare the same cabinet member in different positions. The independent variable of interest is $healthmin_{mct}$, indicating whether a cabinet member m serves as a health minister at time t. $healthmin_{mct}$ retains a geography-related interpretation, though, given that we match health aid to the ministers based on their birth region (resulting in $healthaidD_{mct}$ and $healthaid_{mct}$). The data and econometric framework are described in detail in Appendix 2.D. Similar to the main analysis, the positive effect on the extensive margin is not significant: a cabinet member is not more likely to yield influence over whether any health aid goes to their birth region in the subsequent year (Table 2.4, columns 1 and 2). However, looking at amounts in column (3), we find that a cabinet member's birth region receives more health aid while the cabinet member holds the position of health minister. This result is robust when controlling for $leader_{mc,t-1}$ and $keymin_{mc,t-1}$ in column (4). The size of the coefficient suggests the same individual can attract around 250% more health aid when in the health ministry than in another ministerial office. Hence, it is the position of the health minister, rather than particular individuals selecting into health-related cabinet positions, that explains the surge in health aid. In line with the main results, we find a positive effect for the key minister. Interestingly, the coefficient of $leader_{mc,t-1}$ is negative and significant in column (4), suggesting that cabinet members might attract less health aid when in the position of country leader than when in other positions. This result provides further support for the finding that country leaders do not seem to attract World Bank aid to their birth regions (see also Dreher et al., 2019).

For the minister-level analysis, we conduct a placebo test as for the main analysis. Here, the variable of interest, $prehealthmin_{mct}$, is set to one in the two years before a cabinet member becomes a health minister. There is no evidence that the health minister has an effect in the two years before they are in the health ministry (Table 2.E.10 in Appendix 2.E). Tables 2.E.11 to 2.E.14 in Appendix 2.E replicate the robustness tests provided in Tables 2.E.3 to 2.E.6, using the minister instead of the ADM1-level panel. The results remain similar.

—Table 2.4 about here —

Effect heterogeneity In Appendix 2.E, we explore whether the health ministers' influence increases during their tenure: In Table 2.E.7, columns (1) to (4), we replicate Table 2.2 but add an interaction between the health minister variable with a variable for the number of years the health minister is in office (with the first year being equal to zero). In even columns, we additionally include interactions between the country leader, key ministers, and any cabinet members and their respective tenure. The interaction between the health minister and their tenure is relatively small, statistically insignificant for all specifications, and negative for three specifications: we do not find evidence that health ministers allocate more health aid to their birth region the longer they are in office. In columns (5) to (8), we interact the minister variables with an indicator for their first year in office. The interaction is negative in all four columns. In columns (7) and (8), it is statistically significant and of a similar size to the main effect of the health minister: the health minister allocates much less (if any) health aid during his first year in office compared to other years. This provides evidence against there being any region-specific event (such as a natural disaster) shortly before a new health minister came into office that leads to both an increase in health aid to the affected region in the subsequent year and to the new health minister originating from this region.

In Table 2.E.8 in Appendix 2.E, we analyze whether a health minister's influence is stronger during election years; i.e., whether they allocate more health aid to their region in years in which there is an election of the legislative or executive. In the run-up to elections, cabinet members might allocate funds to their birth region to garner support from their base. We replicate Table 2.2 but add an interaction between the health minister indicator and an indicator 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. The interaction terms between the health minister and whether there was any election are negative in six out of the eight columns and are relatively small and not statistically significant: Health ministers do not seem to allocate health aid strategically before elections. Also, we do not find any election-related effects for country leaders, key ministers, or any cabinet members.

Non-health aid Additionally, we investigate whether the health minister influences the allocation of non-health aid. This is the sum of aid flowing to a region in a given year that does not belong to the health category. The coefficients are positive but much smaller than for health aid and not statistically significant (Table 2.E.9 in Appendix 2.E).[56]

2.4.2 Health ministers' effect on mortality rates

As the previous section shows, health ministers' birth regions are favored in the allocation of World Bank health aid. In this section, we study health outcomes, specifically neonatal and infant mortality. Neonatal and infant mortality are often viewed as proxies for population health outcomes in settings with scarce health data. We first analyze whether health ministers influence mortality in their birth regions. We then evaluate whether the health ministers' influence on mortality rates in their birth region, if there is any, interacts with World Bank health aid.

[56] We also do not find any evidence of an effect of the health minister on non-health aid when using the minister-level panel and person fixed effects (Table 2.E.15 in Appendix 2.E).

Table 2.5 shows the effect of health ministers on neonatal and infant mortality in their birth region. Columns (1) to (4) provide results for neonatal mortality, and columns (5) to (8) for infant mortality. Columns (1) and (5) 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 3.1 and 4.6 deaths per 1,000 live births, respectively. These effects correspond to 10 (7.5) percent of the average neonatal (infant) mortality in our sample. Hence, health ministers not only impact the allocation of health-related funds, but they seem to ameliorate actual health outcomes.

A possible explanation for the lowered mortality is that health ministers' birth regions receive more health aid and that this health aid is effective in reducing mortality. To determine if this is the case, we first estimate the effect of health aid on mortality (columns 2 and 6 in Table 2.5). Given that ministers influence the allocation of health aid mostly in the subsequent year (or even later), and that we are agnostic about whether health aid is more important before or after birth, we consider health aid allocated in the birth year of the child. For a more straightforward interpretation of effects, we standardize health aid. The coefficient estimates have opposite signs and are not statistically significant.

Next, we include both the health minister indicator and health aid in the regression (columns 3 and 7). The coefficient of the health minister remains largely unchanged compared to columns 1 and 5 (where health aid was not included as a regressor). In columns (4) and (8), we also add the interaction between the health minister indicator and health aid. The interaction coefficient is positive and not statistically significant. Hence, we do not find any evidence that health ministers have a more negative effect on mortality, with more health aid being allocated to their birth region. Consequently, the health ministers' favorable effect on mortality does not appear to be driven by World Bank health aid. Accordingly, health ministers likely also channel resources other than World Bank aid to their region.

An increased allocation of health aid to health ministers' birth regions is consistent with pure political capture. It could also reflect aid effectiveness considerations. For example, health ministers may have informational and monitoring advantages. The absence of an association between health aid and a health minister's effect on mortality is indicative of political capture rather than informational and monitoring advantages. However, as we generally do not find health aid has any mortality effect, we cannot rule out that effectiveness motives do play a role in the allocation process.[57] These motives may simply remain undetected if the effects of health aid allocated to an ADM1 region on children born in this region are absent or minute.

—Table 2.5 about here —

Placebo test Along the lines of our placebo test on aid allocation, we examine here if neonates or infants are less likely to die in the two years before the region-born health minister comes into power. In Table 2.E.16 in Appendix 2.E, we test whether $prehealthmin_{ict}$ has an effect on mortality. In odd columns (where we do not control for $healthmin_{ic,t-1}$), the coefficient of $prehealthmin_{ict}$ is either small or positive, suggesting that $prehealthmin_{ict}$ does not decrease mortality. This result also holds when controlling for whether the country leader, a key minister, or any cabinet member was born in that region (columns 3 and 7). When controlling for $healthmin_{ic,t-1}$ in even columns, the coefficient $prehealthmin_{ict}$ is negative but smaller than $healthmin_{ic,t-1}$ and not statistically significant.

Robustness We control for $leader_{ic,t-1}$, $keymin_{ic,t-1}$, and $cabinet_{ic,t-1}$ in Table 2.E.17 in Appendix 2.E. All results related to health ministers remain similar. We do not uncover any effect of the leader or cabinet members in general on mortality, but we find some evidence that key ministers negatively impact infant mortality. In Table 2.E.18, we control for the mother's age at birth, its square,

[57] Other contributions, however, have shown that foreign aid can decrease mortality at the subnational level in Nigeria (Kotsadam et al., 2018), Ivory Coast (Wayoro and Ndikumana, 2019), and 13 African countries (Martorano et al., 2020). Cruzatti et al. (2020) find opposing effects: World Bank aid seems to lower infant mortality, while Chinese aid seems to raise it at the local level across 53 countries (but lower it at the country-level). Additionally, Chapter 3 of this thesis shows that World Bank health aid reduces infant mortality at the local level in 25 African countries.

and dummy variables 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, in addition to the other birth characteristics.[58] The results remain similar. In Table 2.E.19, we check whether health ministers have an effect on the mortality of children born in two years and whether such an effect might be driven by health aid given in the year before the child's birth. The effect of the health minister is still negative but smaller and not statistically significant. Hence, the health ministry's effect on mortality rates seems to be quite limited. The interaction between the health ministry and health aid is still positive (and even statistically significant for neonatal mortality). In Tables 2.E.20 and 2.E.21, we use the broader definition of health ministers and only ministers in the narrow sense, respectively. We obtain similar results as in Table 2.5. The same holds true when we control for standardized population in Table 2.E.22,[59] and use the log amount of health aid[60] in Table 2.E.23.[61] In some surveys, the respondents were asked how long they had lived in their current place of residence. This allows us to restrict our sample to children who were born after their mother moved to the given region. We replicate Table 2.5 using this smaller sample in Table 2.E.25. We thereby seek to 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 or aid commitments). The results remain qualitatively the same. Finally, to follow the approach applied in our aid allocation regressions more closely, we also provide results for the effect of the health minister (and health aid) on the average neonatal and infant mortality at the ADM1 level (using ADM1 region and country-year fixed effects) in Table 2.E.26. The effect of the health minister

[58] We do not include these additional birth characteristics in our main specifications, because they might themselves be outcomes of the region's representation in the cabinet or of health aid.

[59] Data on population come from CIESIN (2018), which are available for the years 2000, 2005, 2010, and 2015. To get a proxy for the population in every year, we interpolate the data after having computed the population in the ADM1 regions.

[60] We add a constant (1) before taking the log so we do not lose observations with zero health aid.

[61] We use the committed amounts to study the effect of foreign aid, like, e.g., Dreher et al. (2019b) and Cruzatti et al. (2020). Other contributions use the disbursed amounts to investigate aid effectiveness, e.g., Dreher and Lohmann (2015). We use the disbursed instead of committed amounts in Table 2.E.24 and obtain similar results.

is negative and statistically significant in all specifications and does not seem to be driven by health aid. (All robustness tables are in Appendix 2.E.)

Effect heterogeneity In Table 2.E.27, we evaluate whether the health minister's effect on mortality changes with tenure. In columns (1) to (4), we interact the health minister variable with his or her tenure. Columns (1) and (2) show results for neonatal, and columns (3) and (4) for infant mortality. The health minister coefficient is significantly negative. The interaction coefficient is positive (but only significantly so for neonatal mortality). Hence, if anything, the health minister's impact on mortality decreases over time. In columns (2) and (4), we additionally include the amount of health aid, the interaction between the health minister and health aid, and the triple interaction between the health minister, tenure, and health aid. The triple interaction is statistically insignificant for both neonatal and infant mortality.

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 always negative but only significant in column (5). So, if anything, health ministers yield a stronger influence over mortality rates in their birth region during their first year in office. The triple interaction between the health minister and first-year indicators with health aid is negative and significant for neonatal mortality but of a similar size as the positive interaction between the health minister and aid. All in all, Table 2.E.27 provides some evidence that health ministers have a stronger influence on mortality rates early on in their term. Meanwhile, for the allocation of health aid (see Section 2.4.1), we find that health ministers have a low or zero impact in their first year. The timing of effects provides another piece of evidence to suggest the lower mortality rates cannot be explained by increased aid allocation, further supporting the notion that health ministers are able to direct resources other than World Bank health aid to their birth regions.

2.5 Conclusions

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 introduce a novel data set on the birthplaces of cabinet members in 47 African countries from 2001 to 2014. We combine this hand-collected data with geocoded data on the location of World Bank aid projects (from AidData) and geocoded data on neonatal and infant mortality (from the Demographic and Health Surveys).

In sum, our evidence suggests that health ministers favor their birth regions. First, including region and country-year fixed effects, we show that a region receives more World Bank health aid when the current health minister originates from this region. We also observe this pattern when looking at the same minister over time: more aid is allocated to a cabinet member's birth region when they are holding office as a health minister compared to other (non-health-related) positions. This finding suggests that it is the position of health minister and not the personal characteristics of health ministers that is responsible for the effect. With our rigorous use of fixed effects and placebo tests, we believe that a causal interpretation of these findings is justified.

Second, we find some evidence that neonatal and infant mortality is lower in health ministers' birth regions. However, these findings do not seem to be driven by the World Bank health aid amounts committed to health ministers' birth regions. These results are based on regressions with mother fixed effects: essentially, we compare children born while a health minister is in power to their siblings born in other years. While we do not evaluate the effect of aid on mortality in an experimental setting, we seek to approximate a quasi-experimental setting through the use of mother fixed effects.

Taken together, we document favoritism of health ministers along two margins: health aid and health outcomes. We conclude that not only can country leaders (as shown in previous literature) exert influence over the allocation of funds but so can cabinet members. Hence, broad representation within cabinets (see Francois et al., 2015) seems to translate into actual power. 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 ministerial favoritism likely manifests itself in diverse ways.

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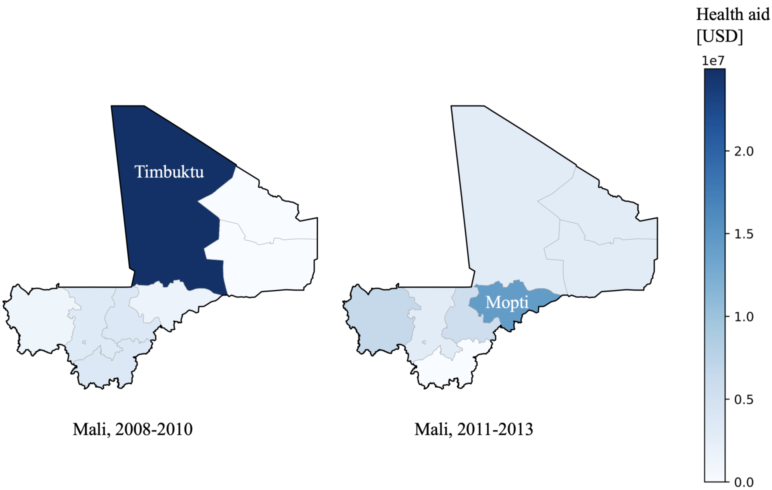
Figures



Notes: From 2008 to 2010, Timbuktu was the birth region of the current health minister, Touré. During Touré's term, Timbuktu received USD 25 million in World Bank health aid per year (map on the left), while other provinces received an average of 2 million. In 2011, Touré was succeeded by the new health minister, Ba, and Mopti became the health minister's birth region (map on the right). During Ba's term, Mopti received USD 14 million per year, while other regions, on average, received USD 3 million.

Figure 2.1: Yearly average of World Bank health aid in Mali, by ADM1 region

Notes: ADM1 regions (dark) that were the birth region of a health minister at least once, 2001-2014.

Figure 2.2: Birthplaces (ADM1) of health ministers, 2001-2014

Tables

Table 2.1: Summary statistics

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Panel A: ADM1 panel data set					
$healthmin_{ict}$	10,318	0.040	0.197	0	1
$leader_{ict}$	10,318	0.055	0.229	0	1
$keymin_{ict}$	10,318	0.335	0.472	0	1
$cabinet_{ict}$	10,318	0.645	0.479	0	1
$healthaidD_{ict}$	10,318	0.106	0.308	0	1
$healthaid_{ict}$ (in billion USD)	10,318	0.002	0.011	0.000	0.420
Panel B: Birth panel data set					
$healthmin_{ict}$	1,173,766	0.055	0.228	0	1
$leader_{ict}$	1,173,766	0.075	0.263	0	1
$keymin_{ict}$	1,173,766	0.410	0.492	0	1
$cabinet_{ict}$	1,173,766	0.741	0.438	0	1
$healthaidD_{ict}$	1,173,766	0.154	0.361	0	1
$healthaid_{ict}$ (in billion USD)	1,173,766	0.002	0.012	0.000	0.297
$neonatal_{kpict}$	1,173,766	30.422	171.745	0	1,000
$infant_{kpict}$	1,028,780	61.196	239.689	0	1,000

Notes: This table provides information on the region panel data set with ADM1 regions as unit of observation in Panel A, and the birth panel data set with children as unit of observation in Panel B. All variables are explained in the main text.

Table 2.2: Health ministers and allocation of health aid in the subsequent year

	(1)	(2)	(3)	(4)
	$healthaidD_{ict}$	$healthaidD_{ict}$	$healthaid_{ict}$	$healthaid_{ict}$
	ext. margin	ext. margin	in USD	in USD
$healthmin_{ic,t-1}$	0.011	0.013	0.615*	0.741**
	(0.016)	(0.016)	(0.351)	(0.378)
$leader_{ic,t-1}$		-0.014		-0.256
		(0.017)		(0.488)
$keymin_{ic,t-1}$		0.003		0.369***
		(0.006)		(0.125)
$cabinet_{ic,t-1}$		-0.009		-0.223
		(0.008)		(0.181)
Obs.	9,581	9,581	9,581	9,581

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a region receives any health aid in a given year ($healthaidD_{ict}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{ict}$) in columns (3) and (4). The independent variables are indicators for whether 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 ($cabinet_{ic,t-1}$) was born in the region. $healthmin_{ic,t-1}$ 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. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering at the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.3: Health ministers and allocation of health aid – Placebo test

	(1)	(2)	(3)	(4)
	$healthaidD_{ict}$	$healthaidD_{ict}$	$healthaid_{ict}$	$healthaid_{ict}$
	ext. margin	ext. margin	in USD	in USD
$healthmin_{ic,t-1}$	0.017	0.018	0.738***	0.894***
	(0.020)	(0.020)	(0.193)	(0.230)
$prehealthmin_{ict}$	-0.000	-0.000	-0.432	-0.393
	(0.030)	(0.030)	(0.419)	(0.416)
$leader_{ic,t-1}$		-0.007		0.117
		(0.023)		(0.583)
$keymin_{ic,t-1}$		-0.002		0.370**
		(0.007)		(0.162)
$cabinet_{ic,t-1}$		-0.006		-0.285
		(0.009)		(0.181)
Obs.	8,130	8,130	8,130	8,130
p-value diff	0.508	0.501	0.017	0.010

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a region receives any health aid in a given year ($healthaidD_{ict}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{ict}$) in columns (3) and (4). The independent variables are indicators for whether 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 ($cabinet_{ic,t-1}$) was born in the region. $prehealthmin_{ict}$ is an indicator equal to 1 in the two years before the region becomes the birth region of the health minister. $healthmin_{ic,t-1}$ 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. All specifications include ADM1 and country-year fixed effects. The row at the bottom of the table reports the p-values of an F-test of whether the coefficients of $healthmin_{ic,t-1}$ and $prehealthmin_{ict}$ are the same. Robust standard errors (in parentheses) adjusted for clustering at the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.4: Person FE: Health ministers and the allocation of health aid in the subsequent year

	(1)	(2)	(3)	(4)
	$healthaidD_{ict}$	$healthaidD_{ict}$	$healthaid_{ict}$	$healthaid_{ict}$
	ext. margin	ext. margin	in USD	in USD
$healthmin_{mc,t-1}$	0.024	0.025	0.926***	1.255***
	(0.019)	(0.019)	(0.307)	(0.390)
$leader_{mc,t-1}$		-0.063		-1.839**
		(0.039)		(0.922)
$keymin_{mc,t-1}$		0.003		0.296*
		(0.006)		(0.158)
Obs.	14,639	14,639	2,475	2,475

Notes: Panel with ministers as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a minister's birth region receives any health aid in a given year ($healthaidD_{mct}$) in columns (1) and (2) and the amount of health aid the region receives ($healthaid_{mct}$) in columns (3) and (4). The independent variables are indicators for whether the minister was a health minister ($healthmin_{mc,t-1}$), country leader ($leader_{mc,t-1}$), or key minister ($keymin_{mc,t-1}$) in the previous year. $healthmin_{mc,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. $leader_{mc,t-1}$ includes the effective head of state. $keymin_{mc,t-1}$ 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. All specifications include person and country-year fixed effects. To achieve convergence, we drop separate observations in columns (3) and (4). Robust standard errors (in parentheses) adjusted for clustering at the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.5: Health ministers and mortality in the subsequent year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-1}$	-3.059**		-3.082**	-3.222**	-4.564**		-4.540**	-4.735**
	(1.253)		(1.252)	(1.293)	(2.151)		(2.153)	(2.204)
$healthaid_{ict}$		0.020	0.159	0.076		-0.400	-0.173	-0.290
		(0.224)	(0.212)	(0.207)		(0.241)	(0.266)	(0.339)
$healthmin_{ic,t-1} \times healthaid_{ict}$				0.451				0.653
				(0.480)				(0.603)
Obs.	1,141,664	1,270,470	1,141,664	1,141,664	998,173	1,118,894	998,173	998,173

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 4) or one year ($infant_{kpict}$; columns 5 to 8), scaled by 1,000. The independent variables are indicators for whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), and the standardized amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. 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 country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Appendix 2.A: How World Bank aid is allocated

The World Bank provides financial and technical assistance for low- and middle-income countries by lending money to these governments. The two main funds are the International Bank for Reconstruction and Developmet (IBRD) and the International Development Association (IDA). Low-interest loans and grants are allocated to projects in various sectors (Ottenhoff, 2011). Although the World Bank has some rules to guide the allocation of projects, the enforcement of these rules is not always ensured (Warner, 2010). The Bank should conduct cost-benefit analyses whenever possible, but Warner (2010, p. 8) finds that the number of projects where economic rates of return are reported has been declining since the 1970es (by 37 points). Furthermore, Warner (2010) reports that, if a cost-benefit analysis is conducted, the alternatives considered tend to focus on minor changes, such as alternative funding mechanisms, instead of different locations, beneficiaries or the alternative of not conducting the project at all. Moreover, cost-benefit analyses are often conducted after the decision to implement the project (which may set adverse incentives). Hence, although the World Bank has the policy of making sure that its projects promote the development goals of the recipient country, the (pre-approval) evaluation of the projects could be improved. This suggests that the allocation of projects could be influenced by political incentives, and the governments of recipient countries might have some margin to influence the allocation decision.

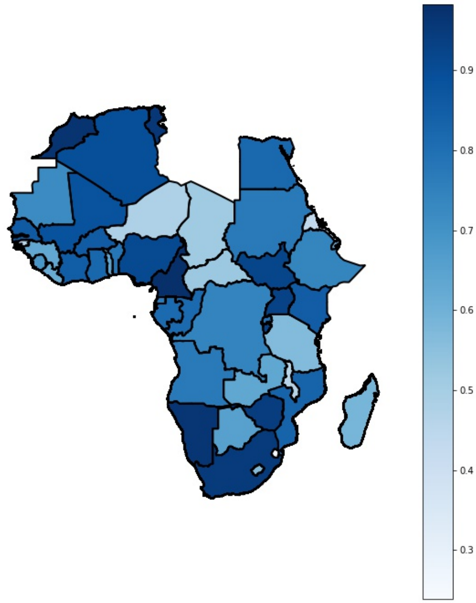
Appendix 2.B: Countries in our sample

Number of Demographic and Health Surveys (DHS) rounds in parentheses

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

40. Sudan (0)
41. Swaziland (1)
42. Tanzania (4)
43. Togo (2)
44. Tunisia (0)
45. Uganda (3)
46. Zambia (3)
47. Zimbabwe (3)

Appendix 2.C: 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 2.C.1: Share of birthplaces identified, 2001-2014

Appendix 2.D: Health ministers' effect on aid allocation: Minister panel

2.D.1 Data set

We construct a panel data set where individuals (who are in the cabinet for at least two years) are the units of observation, to compare the same cabinet member in different positions. Here, the independent variable of interest is $healthmin_{mct}$, indicating whether a cabinet member m serves as a health minister at time t. $healthmin_{mct}$ retains a geography-related interpretation, though, given that we match health aid to the ministers based on their birth region (resulting in $healthaidD_{mct}$ and $healthaid_{mct}$). The minister panel data set is based on 3,213 individuals who are cabinet members for more than one year and for whom birthplace information is available. In total, it comprises 15,703 minister-years. The mode of the time in office is 2 years (24.4% of all individuals). On average, an individual is in office for almost 5 years (4.9 years). 70% of ministers are in office for 5 years or less. There are 84 ministers (2.6%) who hold office for the entire observation period of 14 years.[62] 118 cabinet members are health ministers. They stay in this ministry for, on average, 3.4 years. 23.7% of health ministers are in the cabinet before becoming health minister and 25.4% stay in the cabinet after leaving the health ministry. In our sample, there are 53 switches from any position in the cabinet to the health ministry (or from the health ministry to another position), 26 to the country leader, and 431 to a key minister position.

Table 2.D.1 provides summary statistics for the minister panel. On average, 2.6% of cabinet members are health ministers in a given year, 3.6% are the country leader and 28.4% a key minister.

2.D.2 Econometric framework

To estimate the effect that being in the health ministry has for a given cabinet member, we introduce person fixed effects λ_m instead of region fixed effects α_i. We

[62] The countries with the most long-term cabinet members are Namibia (10 ministers), Uganda (9), Cameroon (9), Zimbabwe (7), Republic of Congo (6) and Eritrea (5).

therefore compare the same minister in different cabinet positions. We estimate the following equation (note the change of index from region i to minister m):

$$healthaid_{mct} = exp[\lambda_m + \beta_{ct} + \gamma \; healthmin_{mct} + \delta \; leader_{mct} \\ + \eta \; keymin_{mct} + \psi \; cabinet_{mct}] + \varepsilon_{mct} \qquad (2.5)$$

As suggested by Correia et al. (2019b), we try to avoid a lack of convergence by dropping observations which are separated by a fixed effect. More specifically, we drop regions that never receive any health aid and country-years that do not receive any health aid.

This approach can shed light on whether health ministers' birth regions receive more health aid because health ministers have different personal characteristics than other cabinet members, or because the position of health minister differs from other cabinet positions.

Table 2.D.1: Summary statistics minister panel data set

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
$healthmin_{mct}$	15,703	0.026	0.158	0	1
$leader_{mct}$	15,703	0.036	0.186	0	1
$keymin_{mct}$	15,703	0.284	0.451	0	1
$healthaidD_{mct}$	15,703	0.129	0.335	0	1
$healthaid_{mct}$ (in billion USD)	15,703	0.002	0.013	0.000	0.420

Notes: This table presents summary statistics on the minister panel data set with ministers as unit of observation. All variables are explained in the Appendix 2.D.

Appendix 2.E: Additional results

Table 2.E.1: Do natural disasters lead to switches in cabinet positions?

	(1) $healthmin_{ct}$	(2) $leader_{ct}$	(3) $keymin_{ct}$	(4) $cabinet_{ct}$
$disaster_{c,t-1}$	-0.036	0.027	-0.010	0.044
	(0.053)	(0.031)	(0.071)	(0.056)
Obs.	601	601	601	601

Notes: Panel with countries as the unit of observations. Estimates based on linear regressions. The dependent variables are indicators whether at least one new health minister ($healthmin_{ct}$; column 1), country leader ($leader_{ct}$, column 2), key minister ($keymin_{ct}$; column 3) or any cabinet member ($cabinet_{ct}$; column 4) comes into 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. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.2: Which key ministers are important?

	(1) $healthaidD_{ict}$ ext. margin	(2) $healthaid_{ict}$ in USD
$healthmin_{ic,t-1}$	0.014	0.668*
	(0.016)	(0.349)
$leader_{ic,t-1}$	-0.016	-0.395
	(0.017)	(0.377)
$PMother_{ic,t-1}$	-0.005	0.173
	(0.011)	(0.206)
$econ_{ic,t-1}$	0.009	0.559**
	(0.008)	(0.242)
$indutrade_{ic,t-1}$	0.002	0.329*
	(0.013)	(0.185)
$agr_{ic,t-1}$	0.014*	0.327*
	(0.008)	(0.192)
$jus_{ic,t-1}$	0.005	0.370
	(0.016)	(0.336)
$foreign_{ic,t-1}$	-0.007	-0.226
	(0.012)	(0.345)
$cabinet_{ic,t-1}$	-0.009	-0.184
	(0.008)	(0.176)
Obs.	9,581	9,581

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in column (1), and on PPML in column(2). Dependent variables are an indicator for whether a region receives any health aid in a given year ($healthaidD_{ict}$) in column (1), and the amount of health aid the region receives ($healthaid_{ict}$) in column (2). The independent variables are indicators whether last year's health minister ($healthmin_{ic,t-1}$), country leader ($leader_{ic,t-1}$), key ministers, or any cabinet member ($cabinet_{ic,t-1}$) was born in the region. 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. $leader_{ic,t-1}$ 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. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.3: Health ministers and contemporary allocation of health aid

	(1)	(2)	(3)	(4)
	$healthaidD_{ict}$	$healthaidD_{ict}$	$healthaid_{ict}$	$healthaid_{ict}$
	ext. margin	ext. margin	in USD	in USD
$healthmin_{ict}$	0.021*	0.023*	0.489	0.553*
	(0.012)	(0.012)	(0.308)	(0.310)
$leader_{ict}$		-0.009		-0.118
		(0.021)		(0.394)
$keymin_{ict}$		0.006		0.169
		(0.008)		(0.135)
$cabinet_{ict}$		-0.010		-0.225
		(0.008)		(0.206)
Obs.	10,318	10,318	10,318	10,318

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a region receives any health aid in a given year ($healthaidD_{ict}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{ict}$) in columns (3) and (4). The independent variables are indicators whether this year's health minister ($healthmin_{ict}$), country leader ($leader_{ict}$), key minister ($keymin_{ict}$) or any cabinet member ($cabinet_{ict}$) was born in the region. $healthmin_{ict}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. $leader_{ict}$ includes the effective head of state. $keymin_{ict}$ 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. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.4: Health ministers and allocation of health aid in two years

	(1)	(2)	(3)	(4)
	$healthaidD_{ict}$	$healthaidD_{ict}$	$healthaid_{ict}$	$healthaid_{ict}$
	ext. margin	ext. margin	in USD	in USD
$healthmin_{ic,t-2}$	0.018	0.018	0.636**	0.709**
	(0.014)	(0.014)	(0.314)	(0.283)
$leader_{ic,t-2}$		-0.016		-0.764
		(0.021)		(0.488)
$keymin_{ic,t-2}$		-0.001		0.089
		(0.008)		(0.208)
$cabinet_{ic,t-2}$		-0.002		-0.159
		(0.008)		(0.174)
Obs.	8,844	8,844	8,844	8,844

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a region receives any health aid in a given year ($healthaidD_{ict}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{ict}$) in columns (3) and (4). The independent variables are indicators whether the health minister ($healthmin_{ic,t-2}$), country leader ($leader_{ic,t-2}$), key minister ($keymin_{ic,t-2}$) or any cabinet member ($cabinet_{ic,t-2}$) two years ago was born in the region. $healthmin_{ic,t-2}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. $leader_{ic,t-2}$ includes the effective head of state. $keymin_{ic,t-2}$ 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. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.5: (Public) health ministers and allocation of health aid in the subsequent year

	(1) $healthaidD_{ict}$ ext. margin	(2) $healthaidD_{ict}$ ext. margin	(3) $healthaid_{ict}$ in USD	(4) $healthaid_{ict}$ in USD
$healthmin_{ic,t-1}$	0.003	0.004	0.515	0.626*
	(0.013)	(0.013)	(0.314)	(0.343)
$leader_{ic,t-1}$		-0.014		-0.255
		(0.017)		(0.498)
$keymin_{ic,t-1}$		0.003		0.352***
		(0.006)		(0.124)
$cabinet_{ic,t-1}$		-0.008		-0.210
		(0.008)		(0.183)
Obs.	9,581	9,581	9,581	9,581

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a region receives any health aid in a given year ($healthaidD_{ict}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{ict}$) in columns (3) and (4). The independent variables are indicators whether 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 ($cabinet_{ic,t-1}$) was born in the region. $healthmin_{ic,t-1}$ includes all health-related ministries: health, public health, HIV/AIDS, population, sanitation. $leader_{ic,t-1}$ 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. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.6: Health ministers (narrow) and allocation of health aid in the subsequent year

	(1)	(2)	(3)	(4)
	$healthaidD_{ict}$	$healthaidD_{ict}$	$healthaid_{ict}$	$healthaid_{ict}$
	ext. margin	ext. margin	in USD	in USD
$healthmin_{ic,t-1}$	0.011	0.012	0.644*	0.751**
	(0.017)	(0.017)	(0.365)	(0.381)
$leader_{ic,t-1}$		-0.014		-0.274
		(0.017)		(0.481)
$keymin_{ic,t-1}$		0.003		0.356***
		(0.006)		(0.131)
$cabinet_{ic,t-1}$		-0.005		-0.140
		(0.009)		(0.181)
Obs.	9,581	9,581	9,581	9,581

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a region receives any health aid in a given year ($healthaidD_{ict}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{ict}$) in columns (3) and (4). The independent variables are indicators whether 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 ($cabinet_{ic,t-1}$) was born in the region. $healthmin_{ic,t-1}$ 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. Only ministers in the narrow sense are included, excluding e.g., vice ministers and ministers of state. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.7: Allocation of health aid and tenure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$healthaidD_{ict}$ ext. margin	$healthaidD_{ict}$ ext. margin	$healthaid_{ict}$ in USD	$healthaid_{ict}$ in USD	$healthaidD_{ict}$ ext. margin	$healthaidD_{ict}$ ext. margin	$healthaid_{ict}$ in USD	$healthaid_{ict}$ in USD
Interaction with:	$tenure_{ic,t-1}$	$tenure_{ic,t-1}$	$tenure_{ic,t-1}$	$tenure_{ic,t-1}$	$firstyear_{ic,t-1}$	$firstyear_{ic,t-1}$	$firstyear_{ic,t-1}$	$firstyear_{ic,t-1}$
$healthmin_{ic,t-1}$	0.011	0.013	0.803	0.922*	0.024	0.025	0.787**	0.920***
	(0.021)	(0.021)	(0.506)	(0.525)	(0.016)	(0.016)	(0.357)	(0.313)
$healthmin_{ic,t-1} \times var$	0.000	-0.000	-0.160	-0.151	-0.034	-0.035	-0.657*	-0.821**
	(0.006)	(0.006)	(0.229)	(0.202)	(0.026)	(0.026)	(0.387)	(0.414)
$leader_{ic,t-1}$		-0.005		-0.150		-0.020		-0.402
		(0.021)		(0.686)		(0.019)		(0.467)
$leader_{ic,t-1} \times var$		-0.004		-0.041		0.024		0.801
		(0.005)		(0.132)		(0.037)		(0.707)
$keymin_{ic,t-1}$		-0.000		0.370**		0.006		0.378**
		(0.007)		(0.156)		(0.008)		(0.169)
$keymin_{ic,t-1} \times var$		0.002		-0.013		-0.007		0.011
		(0.002)		(0.078)		(0.013)		(0.337)
$cabinet_{ic,t-1}$		-0.012		-0.238		-0.008		-0.324*
		(0.008)		(0.213)		(0.008)		(0.192)
$cabinet_{ic,t-1} \times var$		0.002		0.015		-0.001		0.241
		(0.002)		(0.050)		(0.011)		(0.203)
Obs.	9,581	9,581	9,581	9,581	9,581	9,581	9,581	9,581

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in columns (1), (2), (5), (6), and on PPML in columns (3), (4), (7), (8). Dependent variables are an indicator for whether a region receives any health aid in a given year ($healthaidD_{ict}$) in column (1), (2), (5), (6), and the amount of health aid the region receives ($healthaid_{ict}$) in column (3), (4), (7), (8). $healthmin_{ic,t-1}$, $leader_{ic,t-1}$, $keymin_{ic,t-1}$ and $cabinet_{ic,t-1}$ are defined as in Table 2.2. In columns (1) to (4), the minister indicators are interacted with $tenure_{ic,t-1}$, the number of years the respective minister is already in office (with the first year being equal to zero). In columns (5) to (8), they are interacted with $firstyear_{ic,t-1}$, an indicator variable for the first year in which the respective minister is in office. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.8: Allocation of health aid and elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$healthaidD_{ict}$ ext. margin	$healthaidD_{ict}$ ext. margin	$healthaid_{ict}$ in USD	$healthaid_{ict}$ in USD	$healthaidD_{ict}$ ext. margin	$healthaidD_{ict}$ ext. margin	$healthaid_{ict}$ in USD	$healthaid_{ict}$ in USD
Interaction with:	leg_{ict}	leg_{ict}	leg_{ict}	leg_{ict}	$exec_{ict}$	$exec_{ict}$	$exec_{ict}$	$exec_{ict}$
$healthmin_{ic,t-1}$	0.013	0.014	0.631*	0.782*	0.005	0.007	0.629*	0.781*
	(0.016)	(0.017)	(0.364)	(0.401)	(0.018)	(0.018)	(0.378)	(0.416)
$healthmin_{ic,t-1} \times var$	-0.008	-0.010	-0.135	-0.277	0.032	0.028	-0.084	-0.252
	(0.024)	(0.024)	(0.498)	(0.458)	(0.022)	(0.022)	(0.467)	(0.545)
$leader_{ic,t-1}$		-0.020		-0.419		-0.019		-0.529
		(0.017)		(0.527)		(0.017)		(0.537)
$leader_{ic,t-1} \times var$		0.027		0.669		0.026		0.928
		(0.028)		(0.682)		(0.025)		(0.704)
$keymin_{ic,t-1}$		0.004		0.397***		0.004		0.384***
		(0.007)		(0.133)		(0.007)		(0.140)
$keymin_{ic,t-1} \times var$		-0.006		-0.244		-0.002		-0.143
		(0.014)		(0.210)		(0.012)		(0.206)
$cabinet_{ic,t-1}$		-0.010		-0.206		-0.010		-0.180
		(0.009)		(0.195)		(0.009)		(0.196)
$cabinet_{ic,t-1} \times var$		0.006		-0.398		0.007		-0.626
		(0.009)		(0.454)		(0.010)		(0.483)
Obs.	9,581	9,581	9,581	9,581	9,581	9,581	9,581	9,581

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in columns (1), (2), (5), (6), and on PPML in columns (3), (4), (7), (8). Dependent variables are an indicator for whether a region receives any health aid in a given year ($healthaidD_{ict}$) in column (1), (2), (5), (6), and the amount of health aid the region receives ($healthaid_{ict}$) in column (3), (4), (7), (8). $healthmin_{ic,t-1}$, $leader_{ic,t-1}$, $keymin_{ic,t-1}$ and $cabinet_{ic,t-1}$ are defined as in Table 2.2. The minister indicators are interacted with leg_{ict} (columns 1-4) and with $exec_{ict}$ (columns 5-8), indicator variables whether there is an election of the legislative or the executive, respectively, in the current year. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.9: Health ministers and allocation of non-health aid in the subsequent year

	(1)	(2)	(3)	(4)
	$otheraidD_{ict}$	$otheraidD_{ict}$	$otheraid_{ict}$	$otheraid_{ict}$
	ext. margin	ext. margin	in USD	in USD
$healthmin_{ic,t-1}$	0.005	0.006	0.133	0.138
	(0.017)	(0.017)	(0.243)	(0.264)
$leader_{ic,t-1}$		-0.013		-0.034
		(0.028)		(0.123)
$keymin_{ic,t-1}$		0.013		0.097
		(0.013)		(0.148)
$cabinet_{ic,t-1}$		-0.005		0.097
		(0.013)		(0.145)
Obs.	9,581	9,581	9,581	9,581

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a region receives any non-health aid in a given year ($otheraidD_{ict}$) in columns (1) and (2), and the amount of non-health aid the region receives ($otheraid_{ict}$) in columns (3) and (4). The independent variables are indicators whether 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 ($cabinet_{ic,t-1}$) was born in the region. $healthmin_{ic,t-1}$ 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. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.10: Person FE: Health ministers and allocation of health aid – Placebo test

	(1)	(2)	(3)	(4)
	$healthaidD_{mct}$	$healthaidD_{mct}$	$healthaid_{mct}$	$healthaid_{mct}$
	ext. margin	ext. margin	in USD	in USD
$healthmin_{mc,t-1}$	0.039	0.038	1.770***	2.902***
	(0.036)	(0.037)	(0.431)	(0.876)
$prehealthmin_{mct}$	0.046	0.047	-0.423	-0.434
	(0.055)	(0.055)	(2.040)	(2.047)
$leader_{mc,t-1}$		-0.104		1.391
		(0.123)		(1.475)
$keymin_{mc,t-1}$		-0.002		1.166
		(0.011)		(0.805)
Obs.	5,975	5,975	685	685
p-value diff	0.845	0.829	0.175	0.070

Notes: Panel with ministers as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a minister's birth region receives any health aid in a given year ($healthaidD_{ict}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{ict}$) in columns (3) and (4). The independent variables are indicators whether the minister was a health minister ($healthmin_{mc,t-1}$), country leader ($leader_{mc,t-1}$), or key minister ($keymin_{mc,t-1}$) in the previous year. $prehealthmin_{ict}$ is an indicator equal to 1 in the two years before the minister becomes a health minister. $healthmin_{mc,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. $leader_{mc,t-1}$ includes the effective head of state. $keymin_{mc,t-1}$ 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. All specifications include person and country-year fixed effects. To achieve convergence, we drop separated observations in columns (3) and (4). The row at the bottom of the table reports the p-values of an F-test of whether the coefficients of $healthmin_{mc,t-1}$ and $prehealthmin_{mct}$ are the same. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.11: Person FE: Health ministers and contemporary allocation of health aid

	(1) $healthaidD_{mct}$ ext. margin	(2) $healthaidD_{mct}$ ext. margin	(3) $healthaid_{mct}$ in USD	(4) $healthaid_{mct}$ in USD
$healthmin_{mct}$	0.015	0.019	1.056	1.211*
	(0.017)	(0.017)	(0.663)	(0.656)
$leader_{mct}$		-0.045		1.733
		(0.036)		(1.113)
$keymin_{mct}$		0.011		0.288***
		(0.007)		(0.088)
Obs.	15,703	15,703	2,784	2,784

Notes: Panel with ministers as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a minister's birth region receives any health aid in a given year ($healthaidD_{ict}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{ict}$) in columns (3) and (4). The independent variables are indicators whether the minister was a health minister ($healthmin_{mct}$), country leader ($leader_{mct}$), or key minister ($keymin_{mct}$) this year. $healthmin_{mct}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. $leader_{mct}$ includes the effective head of state. $keymin_{mct}$ 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. All specifications include person and country-year fixed effects. To achieve convergence, we drop separated observations in columns (3) and (4). Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.12: Person FE: Health ministers and allocation of health aid in two years

	(1) $healthaidD_{mct}$ ext. margin	(2) $healthaidD_{mct}$ ext. margin	(3) $healthaid_{mct}$ in USD	(4) $healthaid_{mct}$ in USD
$healthmin_{mc,t-2}$	0.012	0.014	1.027***	1.111***
	(0.022)	(0.022)	(0.316)	(0.312)
$leader_{mc,t-2}$		-0.043		0.238
		(0.029)		(0.953)
$keymin_{mc,t-2}$		0.005		0.114
		(0.008)		(0.227)
Obs.	13,453	13,453	2,204	2,204

Notes: Panel with ministers as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a minister's birth region receives any health aid in a given year ($healthaidD_{mct}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{mct}$) in columns (3) and (4). The independent variables are indicators whether the minister was a health minister ($healthmin_{mc,t-2}$), country leader ($leader_{mc,t-2}$), or key minister ($keymin_{mc,t-2}$) two years ago. $healthmin_{mc,t-2}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. $leader_{mc,t-2}$ includes the effective head of state. $keymin_{mc,t-2}$ 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. All specifications include person and country-year fixed effects. To achieve convergence, we drop separated observations in columns (3) and (4). Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.13: Person FE: (Public) health ministers and allocation of health aid in the subsequent year

	(1) $healthaidD_{mct}$ ext. margin	(2) $healthaidD_{mct}$ ext. margin	(3) $healthaid_{mct}$ in USD	(4) $healthaid_{mct}$ in USD
$healthmin_{mc,t-1}$	0.003	0.005	0.451	0.739
	(0.014)	(0.014)	(0.439)	(0.541)
$leader_{mc,t-1}$		-0.063		-1.788*
		(0.040)		(0.924)
$keymin_{mc,t-1}$		0.003		0.256
		(0.006)		(0.167)
Obs.	14,639	14,639	2,475	2,475

Notes: Panel with ministers as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a minister's birth region receives any health aid in a given year ($healthaidD_{mct}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{mct}$) in columns (3) and (4). The independent variables are indicators whether the minister was a health minister ($healthmin_{mc,t-1}$), country leader ($leader_{mc,t-1}$), or key minister ($keymin_{mc,t-1}$) in the previous year. $healthmin_{mc,t-1}$ includes all health-related ministries: health, public health, HIV/AIDS, population, sanitation. $leader_{mc,t-1}$ includes the effective head of state. $keymin_{mc,t-1}$ 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. All specifications include person and country-year fixed effects. To achieve convergence, we drop separated observations in columns (3) and (4). Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.14: Person FE: Health ministers (narrow) and allocation of health aid in the subsequent year

	(1)	(2)	(3)	(4)
	$healthaidD_{mct}$	$healthaidD_{mct}$	$healthaid_{mct}$	$healthaid_{mct}$
	ext. margin	ext. margin	in USD	in USD
$healthmin_{mc,t-1}$	0.034*	0.036*	1.116***	1.558***
	(0.019)	(0.019)	(0.229)	(0.292)
$leader_{mc,t-1}$		-0.062		-1.809**
		(0.039)		(0.840)
$keymin_{mc,t-1}$		0.008		0.409**
		(0.005)		(0.195)
Obs.	14,639	14,639	2,475	2,475

Notes: Panel with ministers as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a minister's birth region receives any health aid in a given year ($healthaidD_{mct}$) in columns (1) and (2), and the amount of health aid the region receives ($healthaid_{mct}$) in columns (3) and (4). The independent variables are indicators whether the minister was a health minister ($healthmin_{mc,t-1}$), country leader ($leader_{mc,t-1}$), or key minister ($keymin_{mc,t-1}$) in the previous year. $healthmin_{mc,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. $leader_{mc,t-1}$ includes the effective head of state. $keymin_{mc,t-1}$ includes key ministers in the sense of Francois et al. (2015), excluding the head of state. Only ministers in the narrow sense are included, excluding e.g., vice ministers and ministers of state. All specifications include person and country-year fixed effects. To achieve convergence, we drop separated observations in columns (3) and (4). Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.15: Person FE: Health ministers and allocation of non-health aid in the subsequent year

	(1) $otheraidD_{mct}$ ext. margin	(2) $otheraidD_{mct}$ ext. margin	(3) $otheraid_{mct}$ in USD	(4) $otheraid_{mct}$ in USD
$healthmin_{mc,t-1}$	0.040	0.042	-0.360	-0.321
	(0.032)	(0.033)	(0.233)	(0.238)
$leader_{mc,t-1}$		-0.163**		-0.266
		(0.063)		(0.290)
$keymin_{mc,t-1}$		0.005		0.084
		(0.010)		(0.101)
Obs.	14,639	14,639	8,576	8,576

Notes: Panel with ministers as the unit of observation. Estimates based on OLS in columns (1) and (2) and on PPML in columns (3) and (4). Dependent variables are an indicator for whether a minister's birth region receives any non-health aid in a given year ($otheraidD_{mct}$) in columns (1) and (2), and the amount of non-health aid the region receives ($otheraid_{mct}$) in columns (3) and (4). The independent variables are indicators whether the minister was a health minister ($healthmin_{mc,t-1}$), country leader ($leader_{mc,t-1}$), or key minister ($keymin_{mc,t-1}$) in the previous year. $healthmin_{mc,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. $leader_{mc,t-1}$ includes the effective head of state. $keymin_{mc,t-1}$ 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. All specifications include person and country-year fixed effects. To achieve convergence, we drop separated observations in columns (3) and (4). Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.16: Health ministers and mortality – Placebo

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-1}$		-2.941*		-2.904*		-4.554*		-4.732*
		(1.525)		(1.546)		(2.284)		(2.357)
$prehealthmin_{ict}$	-0.774	-1.334	-0.435	-1.303	0.330	-1.126	0.175	-1.251
	(0.941)	(1.157)	(1.129)	(1.153)	(2.356)	(2.407)	(2.575)	(2.438)
$leader_{ic,t-1}$			-0.885	-0.823			1.046	1.140
			(1.426)	(1.412)			(2.073)	(2.067)
$keymin_{ic,t-1}$			-0.449	-0.494			-2.877**	-2.932**
			(0.716)	(0.701)			(1.163)	(1.160)
$cabinet_{ic,t-1}$			-0.517	-0.415			1.464	1.612
			(0.744)	(0.750)			(1.259)	(1.253)
Obs.	1,175,392	1,046,369	1,046,369	1,046,369	1,054,787	933,962	933,962	933,962
p-value diff		0.318		0.323		0.299		0.292

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 4) or one year ($infant_{kpict}$; columns 5 to 8), scaled by 1,000. The independent variables are indicators 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 ($cabinet_{ic,t-1}$), an indicator for whether the child was born up to two years before a health minister from this region got into power ($prehealthmin_{ict}$), and the standardized amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ and $prehealthmin_{ict}$ include 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. The row at the bottom of the table reports the p-values of an F-test of whether the coefficients of $healthmin_{ic,t-1}$ and $prehealthmin_{ict}$ are the same. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.17: Health ministers and mortality – Controlling for other ministers

	(1) $neonatal_{kpict}$	(2) $neonatal_{kpict}$	(3) $neonatal_{kpict}$	(4) $infant_{kpict}$	(5) $infant_{kpict}$	(6) $infant_{kpict}$
$healthmin_{ic,t-1}$	-2.997**	-3.021**	-3.170**	-4.659**	-4.637**	-4.855**
	(1.281)	(1.280)	(1.322)	(2.174)	(2.175)	(2.223)
$healthaid_{ict}$		0.170	0.083		-0.157	-0.286
		(0.210)	(0.206)		(0.267)	(0.335)
$healthmin_{ic,t-1} \times healthaid_{ict}$			0.477			0.727
			(0.458)			(0.621)
$leader_{ic,t-1}$	-1.417	-1.441	-1.485	0.200	0.228	0.133
	(1.143)	(1.141)	(1.127)	(1.902)	(1.915)	(1.886)
$keymin_{ic,t-1}$	-0.467	-0.474	-0.482	-2.869**	-2.864**	-2.880**
	(0.671)	(0.669)	(0.671)	(1.077)	(1.079)	(1.079)
$cabinet_{ic,t-1}$	-0.470	-0.468	-0.458	1.492	1.491	1.506
	(0.667)	(0.667)	(0.671)	(1.134)	(1.134)	(1.133)
Obs.	1,141,664	1,141,664	1,141,664	998,173	998,173	998,173

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 3) or one year ($infant_{kpict}$; columns 4 to 6), scaled by 1,000. The independent variables are indicators 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 ($cabinet_{ic,t-1}$) , and the standardized amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ 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 on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.18: Health ministers and mortality: Controlling for mother's age at birth and birth spacing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-1}$	-3.183**		-3.207**	-3.337**	-4.796**		-4.766**	-4.956**
	(1.266)		(1.266)	(1.306)	(2.170)		(2.174)	(2.228)
$healthaid_{ict}$		0.011	0.168	0.089		-0.455*	-0.212	-0.328
		(0.235)	(0.220)	(0.219)		(0.240)	(0.261)	(0.331)
$healthmin_{ic,t-1} \times healthaid_{ict}$				0.415				0.632
				(0.478)				(0.599)
Obs.	1,117,985	1,246,364	1,117,985	1,117,985	976,842	1,097,124	976,842	976,842

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 4) or one year ($infant_{kpict}$; columns 5 to 8), scaled by 1,000. The independent variables are indicators for whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), and the standardized amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. 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, and additionally mother's age (squared) and dummy variables 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. All specifications include mother and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering at the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.19: Health ministers and mortality in two years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-2}$	-0.272		-0.248	-0.601	-1.676		-1.677	-1.850
	(0.898)		(0.905)	(0.955)	(1.825)		(1.831)	(1.834)
$healthaid_{ic,t-1}$		-0.326	-0.171	-0.357		-0.154	0.006	-0.076
		(0.306)	(0.327)	(0.306)		(0.366)	(0.389)	(0.351)
$healthmin_{ic,t-2} \times healthaid_{ic,t-1}$				1.199**				0.591
				(0.561)				(1.414)
Obs.	999,416	1,141,664	999,416	999,416	865,303	998,173	865,303	865,303

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 4) or one year ($infant_{kpict}$; columns 5 to 8), scaled by 1,000. The independent variables are indicators whether the child was born in the same region as the health minister two years before the child was born ($healthmin_{ic,t-2}$), and the standardized amount of health aid committed to the region one year before the child's birth year ($healthaid_{ic,t-1}$). $healthmin_{ic,t-2}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. 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 on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.20: (Public) health ministers and mortality

	(1)	(2)	(3)	(4)	(5)	(6)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-1}$	-3.114***	-3.134***	-3.294***	-4.509**	-4.490**	-4.688**
	(1.055)	(1.052)	(1.076)	(1.843)	(1.846)	(1.873)
$healthaid_{ict}$		0.171	0.061		-0.159	-0.297
		(0.210)	(0.205)		(0.268)	(0.337)
$healthmin_{ic,t-1} \times healthaid_{ict}$			0.585			0.758
			(0.459)			(0.593)
Obs.	1,141,664	1,141,664	1,141,664	998,173	998,173	998,173

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 3) or one year ($infant_{kpict}$; columns 4 to 6), scaled by 1,000. The independent variables are indicators whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), and the standardized amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ includes all health-related ministries: health, public health, HIV/AIDS, population, sanitation. 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 on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.21: Health ministers (narrow) and mortality

	(1)	(2)	(3)	(4)	(5)	(6)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-1}$	-2.694**	-2.719**	-2.860**	-4.652**	-4.626**	-4.833**
	(1.167)	(1.165)	(1.213)	(2.266)	(2.268)	(2.323)
$healthaid_{ict}$		0.157	0.078		-0.171	-0.289
		(0.211)	(0.207)		(0.265)	(0.339)
$healthmin_{ic,t-1} \times healthaid_{ict}$			0.432			0.658
			(0.485)			(0.608)
Obs.	1,141,664	1,141,664	1,141,664	998,173	998,173	998,173

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 3) or one year ($infant_{kpict}$; columns 4 to 6), scaled by 1,000. The independent variables are indicators whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), and the standardized amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. Only ministers in the narrow sense are included, excluding e.g., 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 on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.22: Health ministers and mortality – Controlling for population

	(1)	(2)	(3)	(4)	(5)	(6)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-1}$		-3.077**	-3.162**		-4.564**	-5.953**
		(1.243)	(1.478)		(2.127)	(2.357)
$healthaid_{ict}$	0.019	0.159	0.076	-0.401	-0.172	-0.287
	(0.224)	(0.211)	(0.206)	(0.241)	(0.266)	(0.340)
pop_{ict}	-0.647	-0.162	-0.117	0.761	0.845	0.926
	(2.464)	(2.195)	(2.214)	(3.706)	(3.383)	(3.236)
$healthmin_{ic,t-1} \times healthaid_{ict}$			0.459			0.496
			(0.504)			(0.669)
$healthmin_{ic,t-1} \times pop_{ict}$			-0.053			1.136
			(0.522)			(0.782)
Obs.	1,270,470	1,141,664	1,141,664	1,118,894	998,173	998,173

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 3) or one year ($infant_{kpict}$; columns 4 to 6), scaled by 1,000. The independent variables are indicators whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), the standardized amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$) and standardized population in this year (pop_{ict}). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. 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 on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.23: Health ministers and mortality – Log aid

	(1) $neonatal_{kpict}$	(2) $neonatal_{kpict}$	(3) $neonatal_{kpict}$	(4) $infant_{kpict}$	(5) $infant_{kpict}$	(6) $infant_{kpict}$
$healthmin_{ic,t-1}$		-3.058**	-2.905*		-4.559**	-5.091**
		(1.253)	(1.434)		(2.152)	(2.465)
$healthaid_{ict}$	-0.024	-0.014	-0.011	-0.066	-0.060	-0.069
	(0.042)	(0.043)	(0.044)	(0.058)	(0.069)	(0.069)
$healthmin_{ic,t-1} \times healthaid_{ict}$			-0.057			0.194
			(0.169)			(0.194)
Obs.	1,270,470	1,141,664	1,141,664	1,118,894	998,173	998,173

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 3) or one year ($infant_{kpict}$; columns 4 to 6), scaled by 1,000. The independent variables are indicators whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), and the log amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. 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 on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.24: Health ministers and mortality – Disbursed amounts

	(1)	(2)	(3)	(4)	(5)	(6)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-1}$		-3.069**	-3.177**		-4.542**	-4.643**
		(1.248)	(1.291)		(2.155)	(2.198)
$healthaid_{ict}$	-0.063	0.080	0.010	-0.411*	-0.193	-0.260
	(0.192)	(0.181)	(0.178)	(0.224)	(0.231)	(0.291)
$healthmin_{ic,t-1} \times healthaid_{ict}$			0.400			0.386
			(0.445)			(0.599)
Obs.	1,270,470	1,141,664	1,141,664	1,118,894	998,173	998,173

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 3) or one year ($infant_{kpict}$; columns 4 to 6), scaled by 1,000. The independent variables are indicators whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), and the amount of health aid disbursed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. 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 on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.25: Health ministers and mortality – Non-movers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-1}$	-4.851**		-4.898**	-5.180**	-1.807		-1.802	-2.155
	(2.172)		(2.141)	(2.202)	(2.815)		(2.798)	(2.649)
$healthaid_{ict}$		-0.023	0.094	-0.138		-0.151	-0.009	-0.329
		(0.290)	(0.295)	(0.415)		(0.423)	(0.514)	(0.756)
$healthmin_{ic,t-1} \times healthaid_{ict}$				0.521				0.731
				(0.439)				(1.235)
Obs.	302,753	358,933	302,753	302,753	252,749	302,451	252,749	252,749

Notes: Panel with children as the unit of observation. Estimates based on OLS. Sample reduced to children born after their mothers moved to the current place of living. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 4) or one year ($infant_{kpict}$; columns 5 to 8), scaled by 1,000. The independent variables are indicators whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), and the standardized amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. 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 on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.26: Health ministers and mortality – Regional-level analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$neonatal_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$	$infant_{kpict}$
$healthmin_{ic,t-1}$	-2.234**		-2.226**	-2.275**	-3.536**		-3.509**	-3.638**
	(0.878)		(0.885)	(0.884)	(1.362)		(1.372)	(1.415)
$healthaid_{ict}$		-0.060	-0.035	-0.069		-0.177	-0.125	-0.215
		(0.124)	(0.133)	(0.186)		(0.184)	(0.192)	(0.283)
$healthmin_{ic,t-1} \times healthaid_{ict}$				0.142				0.373
				(0.294)				(0.408)
Obs.	6,813	7,376	6,813	6,813	6,628	7,197	6,628	6,628

Notes: Panel with ADM1 regions as the unit of observation. Estimates based on OLS. Dependent variables are the average neonatal mortality ($neonatal_{kpict}$; columns 1 to 4) and the average infant mortality ($infant_{kpict}$; columns 5 to 8) of the children born in a given year in an ADM1 region, scaled by 1,000. The independent variables are indicators whether last year's health minister ($healthmin_{ic,t-1}$) was born in the region, and the standardized amount of health aid committed to the region ($healthaid_{ict}$). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. All ministers in the broad sense are included, e.g., also vice ministers and ministers of state. Child-level controls are the share of boys, the average birth order, and the share of multiple births in the ADM1 region. All specifications include ADM1 and country-year fixed effects. Robust standard errors (in parentheses) adjusted for clustering on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 2.E.27: Health ministers and mortality – Tenure

Interaction with:	(1) $neonatal_{kpict}$ $tenure_{ic,t-1}$	(2) $neonatal_{kpict}$ $tenure_{ic,t-1}$	(3) $infant_{kpict}$ $tenure_{ic,t-1}$	(4) $infant_{kpict}$ $tenure_{ic,t-1}$	(5) $neonatal_{kpict}$ $firstyear_{ic,t-1}$	(6) $neonatal_{kpict}$ $firstyear_{ic,t-1}$	(7) $infant_{kpict}$ $firstyear_{ic,t-1}$	(8) $infant_{kpict}$ $firstyear_{ic,t-1}$
$healthmin_{ic,t-1}$	-4.221***	-4.424***	-5.300*	-5.542**	-1.462	-1.890	-4.145*	-4.217*
	(1.467)	(1.512)	(2.662)	(2.717)	(1.359)	(1.349)	(2.270)	(2.359)
$healthmin_{ic,t-1} \times var$	1.116**	1.118*	0.690	0.831	-3.555*	-3.060	-0.962	-1.144
	(0.542)	(0.564)	(1.075)	(1.102)	(1.826)	(1.882)	(2.630)	(2.765)
$healthaid_{ict}$		0.071		-0.297		0.068		-0.291
		(0.207)		(0.339)		(0.208)		(0.339)
$healthmin_{ic,t-1} \times healthaid_{ict}$		0.413		1.095**		1.005**		0.463
		(0.691)		(0.471)		(0.392)		(0.766)
$healthmin_{ic,t-1} \times var \times healthaid_{ict}$		0.153		-0.687		-1.377***		0.450
		(0.596)		(0.769)		(0.336)		(0.749)
Obs.	1,141,664	1,141,664	998,173	998,173	1,141,664	1,141,664	998,173	998,173

Notes: Panel with children as the unit of observation. Estimates based on OLS. Dependent variables are an indicator for whether a child died before reaching the age of one month ($neonatal_{kpict}$; columns 1 to 4) or one year ($infant_{kpict}$; columns 5 to 8), scaled by 1,000. The independent variables are indicators whether the child was born in the same region as last year's health minister ($healthmin_{ic,t-1}$), and the standardized amount of health aid committed to the region in the child's birth year ($healthaid_{ict}$). $healthmin_{ic,t-1}$ includes only health ministers in a narrow sense, excluding public health ministers and other health-related positions. All ministers in the broad sense are included, e.g., also vice ministers and ministers of state. In columns (1) to (4), the minister indicators are interacted with $tenure_{ic,t-1}$, the number of years the respective minister is already in office (with the first year being equal to zero). In columns (5) to (8), they are interacted with $firstyear_{ic,t-1}$, an indicator variable for the first year in which the respective minister is in office. 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 on the country level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Chapter 3

Local effects of World Bank aid on infant mortality

3.1 Introduction

Over the past decades, health outcomes have greatly improved around the globe. People live longer and healthier lives, and mortality rates have decreased. However, large disparities remain between different world regions, with African countries especially lagging behind. For instance, the infant mortality rate, measured as the number of children dying before reaching the age of one year per 1,000 live births, is more than six times higher in African than in European countries.[63] A large fraction of foreign aid has been spent by high-income countries and international organizations over the past decades, aiming at improving health outcomes in poorer countries. This paper investigates whether these efforts have been effective. Looking at 25 African countries and using mother fixed effects, I test whether children born close to health-related World Bank projects are less likely to die before their first birthday than their siblings born before any project was implemented. To the best of my knowledge, I am the first to study the local

[63] See World Health Organization (2020) and https://www.who.int/gho/child_health/mortality/neonatal_infant_text/en/.

effects of health-related projects by the World Bank using mother fixed effects across several African countries.[64]

I combine geocoded data on World Bank projects from AidData with data on mothers and their children from Demographic and Health Surveys (DHS). AidData provides information on the locations of World Bank projects and the categorization of these projects, such as whether they are related to health or education. The DHS provide information on the births and eventual deaths of the interviewed mothers' children. For each child, I construct an indicator variable for whether they died before their first birthday. Furthermore, many DHS clusters (enumeration areas) are geocoded. Hence, these two datasets can be combined by merging spatially close projects to each DHS cluster. For each project, AidData additionally provides information on the first and last year in which any money was disbursed. I consider a project to be active in all years between the first and last disbursement. I construct a treatment dummy variable indicating whether any health-related project was active in some of the years before a child was born within 30km of the birthplace of the child. Using mother fixed effects, I effectively compare siblings born before and after the implementation of health-related projects to test whether children born after are less likely to die before their first birthday. This approach controls for time-invariant mother- and cluster-specific characteristics. I further include either country- or first subnational level (ADM1) region-year fixed effects,[65] which control for country- or region-specific shocks. Additionally, I control for birth characteristics. The identifying assumption is that, conditional on birth characteristics and ADM1 region-year fixed effects, there are no events related to health aid disbursements that lead to differences in infant mortality between siblings born before and after a health aid project. I do not find evidence for negative pre-treatment trends in infant mortality rates, lending credibility to my estimation strategy.

I find that children born in the vicinity of a health-related project are less likely to die within their first year of life than their siblings: Health aid disbursements reduce infant mortality by 7.5 deaths per 1,000 live births (which corresponds to

[64] See below for a discussion of related contributions.
[65] ADM1 regions are provinces in many countries.

10 percent of the mean). I also find the log disbursed amount of health aid (instead of the dummy treatment variable) has a negative effect on infant mortality. The main result remains similar when considering different buffer sizes around the DHS clusters, but the effect seems to decrease at distances larger than 30km. I provide some evidence that the effect of health aid increases over time. My main result also remains robust when including indicators for the allocation of other types of aid, which do not have an effect themselves.

In the second part of this paper, I investigate effect heterogeneity. It is often argued that aid should be allocated to poor and well-governed countries and subnational regions, i.e., where the need for aid is highest and success is more likely due to the existence of strong political institutions (e.g., Dollar and Pritchett, 1998; Briggs, 2018). I investigate whether health aid is indeed more effective in more disadvantaged and better-governed areas (or at least not less effective than in other areas). I use pre-treatment levels of infant mortality, urbanity, and nighttime lights to measure a subnational region's need for health aid. I provide evidence that health aid projects might be more effective in more rural areas and in areas with higher infant mortality and lower nighttime light per capita; i.e., in more disadvantaged regions. These results suggest that the allocation of more health aid to richer areas within countries (see, e.g., Briggs, 2018) is indeed problematic. To test whether the effect on infant mortality depends on subnational institutions, I use two measures for the strength of local institutions. First, I use precolonial centralization in ethnolinguistic homelands. Second, I compute an index based on respondents' answers to questions on local institutions from the Afrobarometer surveys. I find no evidence that health aid projects are more effective in areas with stronger local institutions. Next, I turn to the role of economic development and political institutions at the country level. The effect of health aid does not seem to be higher in countries with lower log GDP per capita or stronger country-level institutions (if anything, my results suggest that it might be lower in democracies).[66]

Many papers have tried to find an effect of aggregate foreign aid on country-

[66] In some additional heterogeneity analyses in Appendix 3.B, I also investigate whether projects that are evaluated as successful shortly after finalization have a higher effect on infant mortality. I find no evidence that this is the case.

level outcomes, especially economic growth. However, the evidence has been inconclusive, with some papers even showing negative effects. Easterly (2009) and Qian (2015) provide reviews of this literature. Similarly, attempts to determine the effect of health aid on health outcomes at the country level have delivered mixed results (see, e.g., Williamson, 2008; Mishra and Newhouse, 2009; Dietrich, 2011; Wilson, 2011; Doucouliagos et al., 2019; Pickbourn and Ndikumana, 2019). An increasing amount of geocoded data on both aid disbursements and outcome variables is available. Given the mixed evidence of the effectiveness of foreign aid at the country level, these new data make it possible to study whether aid projects at least have an impact in their immediate vicinity.

This paper contributes to the more recent literature making use of such data. Employing both a difference-in-differences approach and mother fixed effects, Kotsadam et al. (2018) show that Official Development Assistance (ODA) reduces infant mortality at the local level in Nigeria and Wayoro and Ndikumana (2019) find the same effect of World Bank aid projects in the Ivory Coast.[67] I extend the analysis of the local effect of foreign aid on infant mortality using mother fixed effects to 25 African countries and focus on health-related aid projects. Widmer and Zurlinden (2020) also study the effect of World Bank health aid on infant mortality across African countries, but they focus on aid allocated to ADM1 regions and the short-term effects of aid. They do not find evidence that aid reduces infant mortality. I study the effect of health aid in smaller geographic areas and in the short to longer term.

Other studies investigating the effect of aid on infant mortality across several countries do not use mother fixed effects. Martorano et al. (2020) employ a difference-in-differences approach by comparing areas of spatially close DHS clusters to study the effects of Chinese aid in 13 African countries. Their results show that Chinese projects can improve education outcomes and reduce child mortality and that social sector projects have a greater effect than economic projects.[68]

[67] Other single-country studies investigate the effect of aid on health outcomes in Malawi (De and Becker, 2015; Marty et al., 2017) and Uganda (Odokonyero et al., 2018) using instrumental variables, difference-in-differences, and matching approaches.

[68] Martorano et al. (2020) consider health, education, water, sanitation, government, civil society, social infrastructure and services, and food and non-food commodity assistance projects as social sector-related and energy generation and supply, banking, financial and business services,

They do not look at health aid specifically. Employing an instrumental variables approach, Cruzatti et al. (2020) provide evidence that Chinese health aid increases infant mortality at the local level but reduces mortality at the country-level across 53 countries. They find that World Bank projects reduce infant mortality at the subnational level. Unlike Cruzatti et al. (2020), I control for all time-invariant DHS cluster-specific characteristics by using mother fixed effects. Cluster-specific characteristics, such as the propensity to receive aid, the disease prevalence, and the strength of institutions, may affect, for instance, migration patterns or the DHS' choice of sampling locations and could hence bias the estimation of the effect of health aid on infant mortality. Martorano et al. (2020) are able to control for time-invariant characteristics of small areas of spatially close DHS clusters. Using mother fixed effects is even more restrictive because they control for the characteristics of a single DHS cluster as well as for time-invariant mother-specific characteristics.[69] Finally, unlike the previous papers, I try to shed light on the question whether the local effect of health aid depends on the level of need and the strength of institutions.

This paper is also related to the broader literature on the subnational effects of foreign aid, investigating the relationship between foreign aid and local economic growth (Dreher and Lohmann, 2015; Dreher et al., 2019b), conflict (van Weezel, 2017; Gehring et al., 2019), corruption (Isaksson and Kotsadam, 2018a), trade union involvement (Isaksson and Kotsadam, 2018b), and the distribution of economic activity across space (Bluhm et al., 2020).

The remainder of this paper is structured as follows: In Section 3.2, I describe the data on World Bank projects and children's health outcomes, and in Section 3.3, I detail my empirical approach. The results on the local effect of health aid on infant mortality are presented in Section 3.4. Section 3.5 describes the heterogeneity analyses. Section 3.6 concludes.

agriculture, forestry, fishing, industry, mining, construction, trade, transport and communications projects as economic-related.

[69] Using cluster fixed effects, Greßer and Stadelmann (2019) provide some evidence that World Bank projects reduce the number of children a parent loses in 38 countries. They do not employ mother fixed effects. Furthermore, they do not take into account the timing of the aid disbursements relative to the children's birth or death, while I only consider projects (in a certain period) before a child's birth.

3.2 Data

I combine two main sources of data. For information on health aid, I use the data on disbursements and locations of World Bank projects provided by AidData (2017). For information on mortality, I use the Demographic and Health Surveys (ICF, 2014).

The georeferenced data on World Bank projects include projects from the International Development Association (IDA) and International Bank for Reconstruction and Development (IBRD) product lines between 1995 and 2014. This dataset provides information on the locations of the project, the first and the last transactions, the total transaction amount in constant 2011 USD, and the project sector (such as health or agriculture). Because I need to know the exact location of the projects, I only include the projects with coordinates corresponding to an exact location.[70] Figure 3.C.1(a) in Appendix 3.C shows all precisely geocoded project locations in the 25 countries in my sample.[71]

The Demographic and Health Surveys (DHS) provide information on all live births of the interviewed mothers. Because I rely on mother fixed effects, I only keep children with at least one sibling. There are 1,313,013 children of 400,346 mothers born between 1995 and 2014, living in 31,342 geocoded clusters (enumeration areas) in 403 ADM1 regions of 25 countries.[72] The information comes from 86 surveys. Figure 3.C.1(b) in Appendix 3.C presents the DHS clusters in my sample.

I merge the World Bank projects with the DHS clusters by only keeping the projects located within 50km of at least one DHS cluster. There are 333 matched projects across 3,413 locations, with a total disbursed amount of USD 9.74 billion. Focusing only on health projects, there are 55 matched projects across 715

[70] AidData provides the precision code of the coordinates for each project location. I only keep the projects with a precision code of 1, corresponding to an exact location. Less precise locations correspond, for example, to the first and second subnational level (ADM1 and ADM2) regions.

[71] The countries in my sample are Benin, Burkina Faso, Burundi, Cameroon, Chad, Comoros, Ivory Coast, the Democratic Republic of the Congo, Ghana, Guinea, Kenya, Lesotho, Liberia, Madagascar, Malawi, Mali, Mozambique, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, Togo, Uganda, and Zambia.

[72] I merge the DHS clusters with the ADM1 boundaries from the GADM dataset (Global Administrative Areas, 2018).

locations, with a total disbursed amount of USD 2.26 billion.[73] Figure 3.1 shows the DHS clusters in my sample, with a 30km buffer (which I use in the main specifications), and the project locations in Kenya.

— Figure 3.1 about here —

Main independent variables The main independent variable is the dummy variable $healthaid_{imvrct}$, equal to one if there was at least one active health aid project in at least one of the four years before child i of mother m was born (in year t), within a distance of 30km to the DHS cluster v in ADM1 region r in country c. I consider a project to be active in all years between the first and the last transaction. Children that are not treated in the last four years, but within five or more years before their birth, are not included in the control group. In my main specifications, I restrict the sample to children born between 1999 and 2014.[74] Table 3.1 provides descriptive statistics. 14.5 percent of children are born within 30km of an active health project in the four previous years.

I additionally construct variables for the disbursed amount. One project can be located in several different locations. As there is no location-specific information on disbursements, I divide the total disbursed amount by the number of project-specific locations, assuming that disbursements are allocated evenly across locations. Similarly, there is no information on the amount disbursed in each year. I divide the total disbursed amount by the number of active years, i.e., the number of years between the first and the last transaction year, assuming that the World Bank disburses the money more or less evenly over the years (or that the received money is spent evenly).

[73] To give a few examples, the first projects in the dataset, starting in the year 1995, are the *Health, Fertility and Nutrition Project* in Cameroon, the *Sexually Transmitted Infections Project* in Kenya, both ending in 2001, the *Health Sector Recovery Project* in Mozambique, ending in 2003, and the *Second Health and Population Project* in Burundi, ending in 2009.

[74] For children born earlier, the control group cannot be defined properly. The reason is that the earliest disbursements provided in AidData occurred in 1995. Hence, I might identify children as being untreated that were in fact treated in the four years before their birth, but before 1995.

Dependent variables My main dependent variable is an indicator variable for whether a child i of mother m born in cluster v in ADM1 region r in country c in year t died before reaching the age of one year, $mortality_{imvrct}$. Following the common definition of infant mortality, which is the number of children dying before their first year of life per 1,000 live births, I scale this variable by 1,000. I also construct variables indicating whether the child died before its first month of life (neonatal mortality) and between its first month and first year (post-neonatal mortality). In Table 3.1, we see that 75 out of 1,000 children die before reaching the age of one year, and 39 die before reaching the age of one month. I also test if there is an effect on whether a professional birth attendant was present at the delivery of the child (doctors, nurses, professional midwives) and whether the child was born in a health facility. Health facilities include public and private hospitals, as well as clinics and health posts. This information is provided by the DHS but only for the children born in the last three or five years before the interview (depending on the survey) and not for all live births of the interviewed mothers.

Other data and variables I control for birth characteristics, which are provided by the DHS: the gender of the child, indicator variables for whether the birth was the mother's first, second, etc., and a multiple birth indicator (for twins, triplets, etc.). In case of multiple births, all children are treated as unique observations.

— Table 3.1 about here —

3.3 Estimation strategy

Because the DHS provides information on all live births of an interviewed mother, I can use mother fixed effects, thereby effectively comparing siblings born before and after aid projects. This strategy controls for all mother- and cluster-specific characteristics, such as the education of the mother, her religion, and her underlying health condition, as well as a DHS cluster's likelihood to receive aid, the disease prevalence, and the strength of institutions. To control for country-specific shocks, I include country-year fixed effects. In my preferred specifications, I use ADM1 region-year fixed effects instead of country-year fixed effects. These fixed effects control, for instance, for the total amount of aid and other (health-related) funds allocated to a country or region in a given year, for natural disasters or natural resource discoveries, or whether a region sends an important political figure.

To be more precise, I run the following specification to estimate the effect of health aid on infant mortality:

$$mortality_{imvrct} = \beta healthaid_{imvrct} + \alpha_{mvrc} + \delta_{rct} + \theta \boldsymbol{X}_{imvrct} + \epsilon_{imvrct} \quad (3.1)$$

for child i, mother m, cluster v, ADM1 region r, country c, and year t.

The main outcome variable, $mortality_{imvrct}$, is an indicator variable for whether child i died in its first year of life. In the main specification, $healthaid_{imvrct}$ is a dummy variable indicating whether any health project was active within a distance of 30km from the DHS cluster in the four years before the child was born. α_{mvrc} are mother fixed effects, and δ_{rct} are ADM1 region-year fixed effects.[75] $\boldsymbol{X}_{imvrct}$ is a vector of the birth characteristics described in Section 3.2. I estimate Equation 3.1 using a linear probability model. Standard errors are clustered at the DHS cluster level.

The identifying assumption is that, conditional on birth characteristics and ADM1 region-year fixed effects, there are no events related to health aid disbursements that lead to differences in infant mortality between siblings born before and after a health aid project. To lend credibility to this assumption, I check whether there are any negative trends in mortality before the first health-related aid project started. I construct indicator variables equal to 1 if the child was born 0-1, 1-2, 2-3, 3-4, 4-5, and 5-6 years before the first health aid project started. Figure 3.C.2 in Appendix 3.C shows the result of regressing infant mortality on these indicator variables, controlling for birth characteristics, mother fixed effects, and country-year (Figure 3.C.2a) or ADM1 region-year fixed effects (Figure 3.C.2b). Because children born less than one year before the start of a health project might already

[75] In the specifications with country-year instead of ADM1 region-year fixed effects, δ_{rct} is replaced by κ_{ct}.

be benefiting from it, I do not include the dummy for whether the child was born 0-1 years before the first project in the lower panel. Including country-year fixed effects, five out of the six coefficients are close to zero or small and positive and not statistically significant. The coefficient of whether the child was born 1-2 years before the first project is positive, large, and statistically significant. This picture remains similar when including ADM1 region-year instead of country-year fixed effects and when not including the 0-1 dummy variable. In sum, there is no evidence of negative pre-treatment trends. A short-run positive pre-treatment trend might exist, which would imply that I underestimate the magnitude of the negative effect of health aid projects on mortality.

Relying on the DHS data comes with two limitations. First, to ensure the anonymity of survey respondents, DHS clusters are randomly displaced by up to 2km for urban clusters, 5km for rural clusters, and 10km for one percent of rural clusters. Hence, I might identify some children as treated (untreated), although in reality, they live more (less) than 30km away from any World Bank project. This noise can lead to a downward bias of my results.

Second, for most surveys, there is no information covering the mother's place of residence prior to the time of the interview. Ideally, I would only include children of mothers who always lived at their current place of residence. One might worry that the allocation of health aid projects affects moving patterns, which could impact the effect on infant mortality. The direction of this bias is ex-ante unclear.[76] Another problem is that I could identify children as being treated (untreated), although they were born before the mother moved to a cluster less (more) than 30km away from a health aid project. This would lead to a downward bias of my results. In some surveys, the mothers were asked for how long they had lived in their current place of residence. This information is available for around 40% of my sample. Only keeping children born after their mother moved to the current place of residence reduces this smaller sample by another 20%.

[76] If mothers with healthier children (that are less likely to die within their first year of life) are more likely to move closer to a project, I would overestimate the magnitude of the negative effect on infant mortality. If mothers with weaker, sickly children (who are more likely to die before reaching their first birthday) are more likely to move closer to aid projects, I might underestimate the magnitude of the negative effect.

In robustness tests, I show that the effect of health aid is still negative (though smaller and not statistically significant when including all control variables and fixed effects) when only including children from the smaller sample and when only including children born after their mother moved to the sample location.

3.4 The local effects of World Bank health aid on child health outcomes

3.4.1 Effect of health aid on infant mortality

Table 3.2 presents the effect of World Bank health aid on infant mortality. The independent variable is an indicator for whether there was at least one active health aid project in at least one of the four years before the child was born within 30km of the DHS cluster. The first column controls for birth characteristics. In the second column, I add country-year fixed effects, and in the third, mother fixed effects. Column (4) includes ADM1 region-year instead of country-year fixed effects. The coefficient of health aid is negative and statistically significant throughout, providing evidence that a child born in the vicinity of a health aid project is less likely to die before its first birthday than its siblings born in other years. The coefficient from column (4), my preferred specification, suggests that health aid disbursements reduce infant mortality by 7.5 deaths per 1,000 live births among children born in their vicinity. This corresponds to 10 percent of the mean.

— Table 3.2 about here —

Various robustness tests are presented in Tables 3.D.2 to 3.D.7 in Appendix 3.D. First, I replicate Table 3.2 using the total amount of health aid disbursed in the four years before the child was born in logs, instead of using an indicator variable (Table 3.D.2).[77] I control for the log population in the buffer with a

[77]So observations with zero disbursements are not lost, I add a constant (one) before taking the log.

radius of 30km around the DHS cluster in all specifications in Table 3.D.2.[78] The effect of health aid is negative and statistically significant in all specifications.

In my main specifications, I only control for birth characteristics that are unlikely to be influenced by health aid. In Table 3.D.3, I also control for other birth characteristics that might affect infant mortality: the mother's age at birth, its square, and indicator variables for birth spacing; i.e., 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.[79] Because these characteristics might be outcomes of health aid, I do not include them in my main specifications. Including them leaves the results largely unchanged.

In Table 3.D.4, Panel A, I replicate column (4) in Table 3.2 using different lag structures: I use dummy variables indicating whether there was an active project in the year before, in the two years before, up to 10 years before the child was born (column 4 shows the result from Table 3.2, column 4).[80] The coefficients are negative across all specifications and statistically significant in eight out of the ten regressions. The coefficient sizes suggest that health aid projects reduce infant mortality by 3.4 to almost 12 deaths out of 1,000 live births. The coefficients tend to increase with the considered time period, suggesting that the effect of health aid on health outcomes becomes larger over time. The full effect of health aid projects might, in fact, only materialize once they are finalized (for instance, if they involve the construction of new health infrastructure). I adapt my treatment by only considering the last year of a project, instead of all active years: A child is treated if at least one project ended within x years before their birth. Panel B of Table 3.D.4 replicates Panel A using this alternative treatment specification. Most coefficients (except for columns 1 and 3) increase in size compared to the corresponding coefficients in Panel A, and many remain statistically significant. Again, the coefficients tend to be higher the longer the considered time period.

[78] Data on population were taken from CIESIN (2005), which are available for the years 1990 and 1995, and from CIESIN (2018), which are available for the years 2000, 2005, 2010, and 2015. To get a proxy for the population in every year, I interpolate the data after having computed the population in the 30km buffers around the DHS clusters. This procedure leaves me without information on the populations of 10 DHS clusters (i.e., 824 children).

[79] Descriptive statistics are provided in Table 3.D.1 in Appendix 3.D.

[80] Children who are not treated within x years but in year $x-1$ before their birth or earlier are excluded from the control group.

I replicate column (4) in Table 3.2 using different buffer sizes around the DHS clusters in Table 3.D.5: I investigate the effect of health aid projects within 5, 10, 15, 20, 25, 35, 40, 45, and 50km. In Panel A, I replicate column (4) of Table 3.2 using these different buffer sizes (column 6 presents again the result of Table 3.2, column 4). To make the coefficients more comparable across the different specifications, I only include children in the control group that were never treated within 50km (instead of, e.g., within 30km for column 6) in Panel B. First, note that my main result remains similar when using this adapted control group (Panel B, column 6). Second, the remaining 18 coefficients across both panels are all negative. The effect remains comparable in both panels when using a 25km instead of a 30km buffer. In Panel B, the effect also remains large and statistically significant for all smaller buffers and for the 35km buffer. The coefficients for the 40, 45, and 50km buffers are smaller and statistically insignificant in both panels (the control group for the treatment within 50km, and hence the coefficient, is the same in both panels). These results indicate that the effect of health aid projects on mortality decreases with distances larger than 30 to 35km.

In Table 3.D.6, I replicate Table 3.2, including two dummy variables for whether there was any other active aid project in the four years before the child was born: one dummy variable for social aid (other than health aid) and one for economic aid. Largely following Martorano et al. (2020), I consider social aid as projects related to education, water, sanitation, public administration, law and justice, pensions and insurance, and other social services (excluding health aid projects); and economic aid as projects related to banking and finance, industry and trade, agriculture, forestry and fishing, energy, transportation (including roads and railways), information technology, and construction. The effect of health aid remains similar in size and statistically significant in all columns. The coefficient of social aid is positive or small and never statistically significant. The coefficient for economic aid is negative and statistically significant in columns (1) and (2). However, in the more conservative specifications in columns (3) and (4), the coefficient turns positive and is no longer statistically significant. These results suggest that it is, indeed, health aid and not other aid projects that has an impact on infant mortality.

In Table 3.D.7, I restrict my sample to the children born after their mother moved to their current place of residence to control for potential effects on moving patterns.[81] In Panel A, I only keep the children of mothers that were asked and answered the question on how long they have been living there (leading to a reduction in the sample size of around 60%). In Panel B, I further reduce the sample by another 20% by only keeping the children born after their mother moved to their current place of residence. In the first three columns in both panels, the coefficients are similar in size to the effect in the full sample (although not statistically significant in column 3). In both panels, the coefficient in column (4) is still negative but smaller than in the full sample.

Taken together, these robustness tests support my main finding that health aid projects reduce infant mortality in their vicinity.

3.4.2 Other mortality measures and health-related outcomes

Table 3.3 presents results on the effect of health aid on other mortality measures and health-related outcomes. First, I decompose infant mortality into neonatal (mortality in the first month) and post-neonatal mortality (mortality between the first month and first year). The coefficients are negative for both post-neonatal (column 1) and neonatal mortality (column 2), while only statistically significant for the former. To investigate the effect on neonatal mortality, I can increase the sample by including the children that are less than one year but more than one month old at the time of the interview. By enlarging the sample size like this, the coefficient is statistically significant at the 10% level.

Next, I test whether births close to health-related projects are more likely to be attended by professional birth attendants (doctors, nurses, professional midwives) and to happen in a health facility (public and private hospitals, clinics, health posts). This information is only available for children born in the past three or five years, reducing the sample size. The effect on birth attendance is positive, while the effect on health facilities is negative. Neither are statistically significant.

— Table 3.3 about here —

[81]See Section 3.3 for a discussion of the potential implications of missing information on the mother's place of residence before the date of the interview.

3.5 The role of institutions and the need for foreign aid

There is a consensus in the international community that foreign aid should be allocated to poor and relatively well-governed countries, i.e., where the need for aid is highest and success is more likely due to the existence of strong political institutions. The World Bank, for instance, emphasized the importance of directing aid flows to poorer countries with sound political institutions in the study *Assessing Aid* (Dollar and Pritchett, 1998). Another example is the Commitment to Development Index by the Center for Global Development: Aid flowing to low-income countries and to countries with relatively strong institutions receives a higher weight in the construction of the index.[82] Similarly, it has been argued that within countries, aid should be allocated based on need, i.e., to areas that are poor and, hence, in need of additional funds (see, e.g., Briggs, 2018). Furthermore, not only might country-level but also subnational institutions play a crucial role in the success of foreign aid, suggesting that projects should be deployed in subnational regions with strong institutions.

However, there is evidence that bilateral and multilateral aid is often allocated in line with political interests and to favor certain countries or subnational regions (Alesina and Dollar, 2000; Kuziemko and Werker, 2006; Dreher et al., 2009; Faye and Niehaus, 2012; Jablonski, 2014; Bommer et al., 2019; Dreher et al., 2019a; Widmer and Zurlinden, 2020), implying that aid flows are based on criteria other than economic development and institutions. There is indeed evidence that aid does not reach the poorest regions within countries (Briggs, 2017; Nunnenkamp et al., 2017; Briggs, 2018; Öhler et al., 2019).

Yet, aid could be less effective in poorer areas; for instance, because it might be more difficult to reach the recipients, or to implement a project in areas that might be characterized by malfunctioning authorities.[83] The scarce evidence on whether

[82] See https://www.cgdev.org/cdi-methodology and Robinson et al. (2018).

[83] Evidence shows that the strength of institutions and economic development are correlated at the country level (see, e.g., Dollar and Kraay, 2003; Easterly and Levine, 2003; Glaeser et al., 2004). Michalopoulos and Papaioannou (2013) find that nighttime light intensity is higher in areas that had more centralized ethnic groups during precolonial times, suggesting that this relationship also holds at the subnational level.

aid is more effective if allocated based on need is mixed. Kotsadam et al. (2018) show that the effect of aid on health outcomes is higher in rural and Muslim-dominated areas in Nigeria; i.e., in arguably disadvantaged regions. Dreher et al. (2019b) do not find evidence that the effect of aid on economic development is reduced by favoritism, which might imply that aid is not less effective when it is not allocated (purely) based on need. Furthermore, the empirical evidence on the role of country-level institutions is inconclusive (see, e.g., Easterly, 2009), while little is known about the importance of local institutions.[84] Hence, where aid might have the highest impact remains an open question.

I try to shed light on this question in the context of health-related aid and health outcomes. I investigate whether the effect of World Bank health aid on infant mortality, my proxy for health outcomes, depends on the level of need and the strength of political institutions within and across countries. Thereby, I cannot exploit an experimental setting. Hence, I cannot exclude the possibility that different projects are allocated to locations with different characteristics. However, shedding light on where health aid is particularly effective can still generate important insights on past World Bank projects, and thus, lessons can be drawn for the future.

3.5.1 Local need

I use three different measures to capture a subnational area's need for health aid: the pre-treatment levels of infant mortality, urbanity, and nighttime light intensity. In the following tables, odd columns control for birth-specific characteristics, mother fixed effects, and country-year fixed effects (replicating column 3 of Table 3.2), while even columns include ADM1 region-year instead of country-year fixed effects (replicating column 4 of Table 3.2).[85]

[84]Concerning the effectiveness of health aid, there are two studies on the importance of institutions at the country level. Dietrich (2011) provides some evidence that health aid has a higher effect on immunization rates in countries with higher corruption. She argues that corrupt leaders have incentives to comply with donor objectives in the health sector to enable embezzlement of other aid flows. In contrast, Doucouliagos et al. (2019) find that health aid is more effective in reducing infant mortality at the country level in better-governed countries.

[85]Descriptive statistics for the additional variables in this section are provided in Table 3.D.1 in Appendix 3.D.

First, I proxy need by the level of infant mortality in 1994, before the sample starts. If aid is more effective where the need for aid is highest, the effect of health aid on infant mortality should be higher in areas with higher infant mortality. In Table 3.4, I add interactions between health aid and whether the child was born in an ADM1 region with high infant mortality in 1994. I construct dummy variables equal to one for ADM1 regions with infant mortality higher than the 25th (columns 1 and 2), the 50th (columns 3 and 4), and the 75th percentile (columns 5 and 6). The coefficients of the interaction terms are positive in the first two columns (but not statistically significant) and negative in the other columns. For the ADM1 regions with infant mortality higher than the 75th percentile, the effect is even statistically significant at the 1% level when controlling for country-year fixed effects. These results suggest that infant mortality might be reduced more in the regions with the highest mortality levels.

— Table 3.4 about here —

Second, I consider the degree of urbanity. Because child health outcomes tend to be worse in rural areas (see, e.g., Van de Poel et al., 2007; Ameye and De Weerdt, 2020), health aid might lead to larger improvements in these areas. However, the effect could, in fact, be higher in urban areas because children might be more easily reached in cities than in more remote areas. In Table 3.5, I interact health aid with an indicator for whether the child was born in an urban area. To obtain a measure for urbanity, I use the information provided by the DHS about whether the DHS cluster is located in an urban or rural area (columns 1 and 2). Additionally, I construct an indicator for whether the DHS cluster is located in a city with more than 50,000 inhabitants in 1990 (columns 3 and 4). For this purpose, I merge the DHS clusters with polygons of African cities provided by the Africapolis dataset (OECD/SWAC, 2018), which also provides information on the population in 1990. The interaction terms are positive and statistically significant when including country-year fixed effects. Including ADM1 region-year fixed effects, the effects remain positive and even fairly large for children born in cities with more than 50,000 inhabitants, but they are no longer statistically significant. These results suggest that, if anything, health aid projects are less effective in cities than in rural areas. One might argue that children born in urban areas are unlikely to benefit from projects located (too far) outside the city. Hence, I replicate my specifications from Table 3.5 in Table 3.D.8 but only consider projects located within a distance of 10km (columns 1 to 4) and 20km (columns 5 to 8). The results are weaker than in Table 3.5 for children born in urban enumeration areas but remain similar for children born in cities with more than 50,000 inhabitants.

— Table 3.5 about here —

Third, I use nighttime light intensity to measure an area's need for aid. Nighttime lights are increasingly used to measure subnational economic development (e.g., Henderson et al., 2012; Hodler and Raschky, 2014). Furthermore, Bruederle and Hodler (2018) show that nighttime lights are related to indicators of human development, such as school attendance and infant mortality. Hence, aid might have a higher effect in areas with lower nighttime light intensity, i.e., with lower levels of economic and human development. However, children in areas with lower nighttime light intensity might be more difficult to reach, which would dampen the effect of aid projects.

Data on nighttime light intensity are provided by the U.S. National Oceanic and Atmospheric Administration (NOAA).[86] These data come from weather satellite recordings, which measure light emissions from the earth's surface in nights without cloud cover. Following the majority of the economic literature, I use the stable lights series (National Oceanic and Atmospheric Administration, 2013), which filters out light from the sun and moon and ephemeral lights such as the aurora and fires. These series contain light from settlements and industry. Annual average light intensity ranges from 0 to 63, where higher values correspond to more intense nighttime light. I compute average nighttime light intensity in 1994 (before my sample starts) in the 30km buffers around the DHS clusters, the ADM1 regions, and the ethnic homelands.[87]

[86] Bennett and Smith (2017) provide an overview of the nighttime light products and their use in economics and other disciplines.

[87] For the year 1994, stable lights are available from two satellites – I use the data from the most recent one. Ethnic homelands correspond to the classification by Murdock (1959). See Section 3.5.2 for more information on these homelands.

I include interactions between a dummy indicating whether the child was born in an area with high nighttime light intensity per capita and the health aid dummy in Table 3.6. Nighttime light intensity captures both economic development and population density. Hence, I use nighttime light per capita to obtain a better proxy for economic development (as opposed to urbanity). Panels A, B, and C include dummy variables equal to one if nighttime light per capita in the area is higher than the 25th, 50th, and 75th percentile, respectively. In columns (1) and (2), the area corresponds to the 30km buffer around the DHS cluster, in columns (3) and (4) to the ethnic homeland, and in columns (5) and (6) to the ADM1 region. For the 30km buffers, the interaction terms are positive, relatively large, and even statistically significant when including country-year fixed effects in all three panels. For ADM1 regions, the interactions are positive, relatively large, and even statistically significant in four out of the six specifications. At the ethnic homeland level, the interaction term is more noisy: It is positive in Panel A and C (and even large and statistically significant in Panel A) but slightly negative in Panel B. Still, taken together, these results provide some evidence that health aid projects might be less effective in areas with higher nighttime light intensity than in regions with the lowest levels of nighttime light.

— Table 3.6 about here —

Overall, I find no evidence that health aid is less effective in subnational areas with higher need, measured by mortality, urbanity, and nighttime light, in Tables 3.4, 3.5 and 3.6. If anything, health aid might be more effective in these areas. These results hence support the idea that more health aid, or at least not less, should be allocated to locations with higher need.

3.5.2 Local institutions

Because there is no measure of the quality of subnational institutions across African countries, I construct two proxies for the strength of political institutions at the local level. First, I use information on precolonial centralization from the Ethnographic Atlas (Murdock, 1967). The Ethnographic Atlas provides a variable capturing the number of jurisdictional hierarchy levels beyond the local community, ranging from no political authority beyond the community, petty chiefdoms, larger chiefdoms, states, and large states. The data from the Ethnographic Atlas can be combined with Murdock's (1959) map of ethnic homelands. I merge the geocoded information on the DHS cluster with this map of ethnic homelands.[88] Following earlier literature (e.g., Michalopoulos and Papaioannou, 2013), I assume ethnicities with no political authority beyond the local community or living in a petty chiefdom are not centralized, and ethnicities living in larger chiefdoms, states, and larger states are centralized. I construct an indicator variable for whether a child was born in the homeland of a centralized ethnic group in precolonial times. This variable is supposed to be a proxy for whether the child was born in an area with stronger institutions today.[89] Earlier research has shown that the degree of precolonial centralization has an effect on institutions today: Gennaioli and Rainer (2007) provide evidence that public goods provision is higher in countries with more centralized ethnic groups, and Gennaioli and Rainer (2006) show that ethnic centralization is related to institutional quality as measured by control of corruption and rule of law indices. Michalopoulos and Papaioannou (2013) provide evidence for an association between precolonial ethnic centralization and regional development measured by nighttime light.

Second, I construct an index for the strength of local political institutions from the Afrobarometer surveys, which were geocoded by BenYishay et al. (2017). These surveys ask respondents about their opinions on governance and society. I use the answers to questions on how much respondents trust their elected local government councilors, how many of them they think are corrupt, how often they think local government councilors try their best to listen to what people like themselves have to say, and whether they approve or disapprove of the way they have performed their jobs over the past 12 months. Answer categories range between 0 and 3, where higher numbers indicate stronger institutions (I have to rescale the answers on corruption so that larger numbers mean less corruption). I construct a dummy variable for each variable (equal to 0 if the original variable

[88]Because 299 out of 31,342 clusters cannot be merged, I lose 10,092 out of 1,313,013 children.

[89]Information on precolonial centralization is not available for 51 out of 509 homelands (i.e., for 2,945 clusters and 101,826 children).

takes the values zero or one, and equal to 1 for values two and three) and take the average to obtain a measure for local political institutions. By taking the average across all individuals living in the same ethnic homeland (ADM1 region), I construct an index for the strength of local political institutions in the ethnic homeland (ADM1 region) level.[90] I use survey round 3, conducted in 2005 and 2006, which is the earliest round asking the relevant questions. This reduces my sample from 25 to 12 countries.[91] Ideally, I would have a measure of local institutions before 1995. To check whether results differ across rounds, I additionally use survey round 4, conducted in 2008 and 2009.

Table 3.7 presents the results. Because the level of economic development might explain part of the potential effect of local institutions, I add interactions of the health aid dummy with the log of nighttime light[92] and with the log of population in all specifications. The interaction with the log of nighttime light is positive in nine out of 10 specifications and statistically significant at the ADM1 level, supporting my previous results suggesting that health aid projects have weaker effects in areas with higher nighttime light intensity. In columns (1) and (2), I add an interaction with the dummy indicating whether the ethnic homeland was centralized in precolonial times. The interaction terms are small and statistically insignificant and of different signs across the two columns. In columns (3) to (6), I use the index of local institutions based on the Afrobarometer survey from round 3. In the third and fourth (fifth and sixth) column, I use the index at the ethnic homeland (ADM1) level. In columns (7) to (10), I replicate columns (3) to (6), using the index based on round 4 instead of round 3. The effect of the Afrobarometer index is positive in seven out of the eight specifications but not statistically significant. Note that at least at the ADM1 level, the coefficient sizes are comparable across rounds (suggesting that the index does not change considerably across rounds).

In sum, I do not find evidence that health aid projects are more effective

[90] Appendix 3.A provides further information on the questions, answer categories, and the construction of the index.

[91] The countries are Benin, Ghana, Kenya, Lesotho, Madagascar, Mali, Mozambique, Malawi, Nigeria, Senegal, Uganda, and Zambia.

[92] Following earlier literature (e.g., Michalopoulos and Papaioannou, 2013), I add 0.01 before taking the log.

in areas with stronger local institutions. However, my measures are relatively imprecise. First, they are measured at the ethnic homeland or the ADM1 level. Ideally, I would have a measure of local institutions at the very local level. Second, the information on centralization comes from precolonial times and might thus be fairly noisy. Third, the index based on Afrobarometer surveys reduces my sample to 12 countries and is based on only four questions.

— Table 3.7 about here —

3.5.3 Country-level characteristics

Finally, I investigate whether the effect of health aid depends on country-level GDP per capita and institutions in Table 3.8. I use log GDP per capita in 1994 provided by the World Development Indicators (World Bank, 2019a).[93] To obtain a measure for the strength of institutions at the country level, I create an indicator for whether a country is democratic in the year 1994. The Polity IV Project (Marshall et al., 2019) provides the Polity2 Score that 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, I construct an indicator variable for whether a country is democratic in a given year, equal to one for scores between +6 and +10.

Additionally, I use three measures from the Worldwide Governance Indicators Project (World Bank, 2019b): control of corruption, government effectiveness, and rule of law. These three measures are based on different perception-based data sources, such as household and firm surveys, and information provided by non-governmental and multilateral organizations and commercial business information providers. The underlying variables are standardized and aggregated to the respective indicator. Control of corruption measures the extent to which public power is perceived (by individuals, firms, and the above-mentioned institutions) to be exercised for private gain. Government effectiveness aggregates perceptions of the quality and independence of public and civil service and of the government's formulation and implementation of and commitment to policies.

[93] There is no information on the GDP per capita for Liberia prior to 2000.

Rule of law measures aggregated perceptions on the quality of contract enforcement, property rights, the police, the courts, and the likelihood of crime and violence. The indicators are measured in units of a standard normal distribution, with mean zero, standard deviation one, and ranging between approximately -2.5 and +2.5. Higher values indicate better governance. I use the values in 1996, the earliest year available.[94]

In the first two columns, I interact the health aid dummy with log GDP per capita. In the other columns, I also add interactions between the health aid dummy and an indicator for whether a country is democratic, the level of control of corruption, the rule of law, and government effectiveness. The interaction terms with log GDP per capita are positive in all columns, which might indicate that projects are less effective in richer countries. However, the coefficients are not statistically significant. The positive interaction term with the dummy variable indicating whether a country is democratic is statistically significant, but only when including country-year fixed effects in column (3). Hence, if anything, projects are less effective in more democratic countries. The interactions with rule of law and government effectiveness are positive, while the interaction with control of corruption is negative, but none are statistically significant. Taken together, I do not find evidence that projects are more effective in countries with stronger institutions.

— Table 3.8 about here —

3.6 Conclusions

To investigate whether health-related aid has an impact on health outcomes at the local level, I combine data on World Bank project locations and geocoded data from the Demographic and Health Surveys from 25 African countries. I study whether children born close to health aid projects are less likely to die within their first year of life. Using mother fixed effects, I compare siblings born

[94]See http://info.worldbank.org/governance/wgi/Home/Documents and Kaufmann et al. (2011) for further information on the underlying data sources and the methodology used to construct the indicators.

before and after health aid projects. My findings show that health aid provided by the World Bank indeed reduces infant mortality. To get a sense of the size of the effect, I conducted a rough back-of-the-envelope calculation. The World Health Organization (WHO) reports 344,063,273 births in the countries in my sample between 1999 and 2014. 14.5 percent of the children in my sample are born within 30km and four years of an active health aid project. Such health aid projects reduce infant mortality by 7.5 deaths per 1,000 live births. Hence, my main estimation results suggest that the lives of 374,168 infants were saved by World Bank health aid projects. For the period spanning 1999 to 2014, the WHO estimates that at the age of one, children were expected to live another 58 years, implying that almost 22 million life-years were saved.

My additional results suggest that the effect of health aid is stronger in subnational areas where the need for aid is higher. I do not find evidence that these projects are more effective if institutions are stronger. However, more research (especially in experimental settings) is needed to shed light on the question of which projects can save the most lives in which locations and under which circumstances. Given that the resources of the World Bank and other donors are scarce and that the effects may be huge, answering this question should be a top priority.

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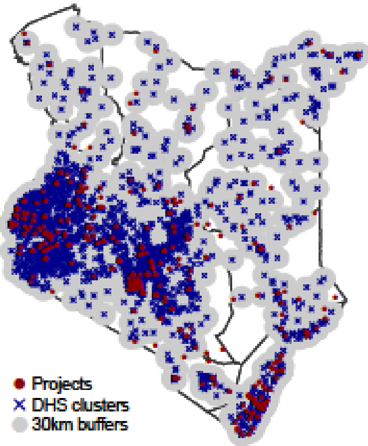
Figures



Notes: This figure shows the DHS clusters with 30km buffers and the World Bank projects in Kenya.

Figure 3.1: DHS clusters with 30km buffers and World Bank project locations in Kenya

Tables

Table 3.1: Descriptive statistics

Variable	Obs.	Mean	Std.Dev.	Min.	Max.
Dependent variables					
Infant mortality	1,227,595	75.405	264.044	0	1,000
Postneonatal mortality	1,227,567	39.419	194.591	0	1,000
Neonatal mortality	1,309,348	35.554	185.176	0	1,000
Professional birth attendant	419,703	0.493	0.500	0	1
Health facility	416,020	0.502	0.500	0	1
Main independent variables					
Health aid, 4y, 30km	1,081,341	0.145	0.352	0	1
Health aid, amount, 4y, 30km	1,081,341	3.57e+05	1.74e+06	0.000	4.11e+07
Birth characteristics					
Male	1,313,013	0.507	0.500	0	1
Birth order	1,313,013	3.687	2.340	1	19
Multiple births	1,313,013	0.038	0.192	0	1

Notes: All variables are described in the text. The sample includes all children for which the treatment variables and at least one of the dependent variables is non-missing. The dependent variables are the different mortality measures, scaled by 1,000, and the dummies indicating whether the child was born in the presence of a professional health attendant and in a health facility. The main independent variables are dummies indicating whether there was any health aid and the amount of health aid within 30km of the DHS clusters in at least one of the four years before the birth. Birth characteristics are a dummy whether the child is male, the birth order, and a dummy for multiple births.

Table 3.2: Effect of health aid on infant mortality

	(1) Controls	(2) Country-year FE	(3) Mother & Country-year FE	(4) Mother & ADM1-year FE
Health aid	-13.657***	-7.152***	-4.250*	-7.536***
	(0.811)	(0.953)	(2.229)	(2.821)
Birth controls	Yes	Yes	Yes	Yes
Country-year FE	No	Yes	Yes	No
ADM1-year FE	No	No	No	Yes
Mother FE	No	No	Yes	Yes
Observations	1,039,607	1,039,607	962,317	962,292

Notes: The dependent variable is an indicator for whether a child died before reaching the age of one year, scaled by 1,000. The independent variable is an indicator for whether at least one health aid project was active in the four years before the child was born within 30km of the DHS cluster. Birth controls are gender, indicator variables for birth order, and for multiple births. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.3: Effects of health aid on other mortality measures and health outcomes

	(1)	(2)	(3)	(4)	(5)
	Post-neonatal	Neonatal	Neonatal	Birth attendant	Health facility
		Infant sample	Neonatal sample		
Health aid	-4.533**	-3.008	-3.217*	0.008	-0.010
	(2.158)	(1.929)	(1.827)	(0.011)	(0.011)
Birth controls	Yes	Yes	Yes	Yes	Yes
Country-year FE	No	No	No	No	No
ADM1-year FE	Yes	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes
Observations	962,271	962,271	1,065,129	277,268	273,615

Notes: The dependent variables are indicators of whether a child died between the age of one month and one year, and before one month (both scaled by 1,000), whether professional health personnel were present at the delivery, and whether the child was delivered in a health facility. The independent variable is an indicator for whether at least one health aid project was active in the four years before the child was born within 30km of the DHS cluster. Birth controls are gender, indicator variables for birth order, and for multiple births. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.4: Health aid in regions with high infant mortality in 1994

	(1)	(2)	(3)	(4)	(5)	(6)
	Higher than p25		Higher than p50		Higher than p75	
Health aid	-5.850	-14.967**	-1.650	-6.786	-1.055	-6.750**
	(4.506)	(7.188)	(3.077)	(4.290)	(2.494)	(3.374)
Health aid × High mortality	2.000	8.459	-5.122	-1.316	-12.391***	-2.502
	(4.984)	(7.809)	(4.190)	(5.693)	(4.491)	(6.139)
Birth controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-year FE	Yes	No	Yes	No	Yes	No
ADM1-year FE	No	Yes	No	Yes	No	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	962,290	962,267	962,290	962,267	962,290	962,267

Notes: The dependent variable is an indicator for whether a child died before reaching the age of one year, scaled by 1,000. The independent variables are an indicator for whether at least one health aid project was active in the four years before the child was born within 30km of the DHS cluster and an interaction with this indicator. Health aid is interacted with an indicator for whether the child was born in an ADM1 region with infant mortality higher than the 25/50/75th percentile in 1994. Birth controls are gender and indicator variables for birth order, and for multiple births. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.5: Health aid in urban areas and cities

	(1)	(2)	(3)	(4)
Interaction with:	Urban areas (DHS)		City (pop >50,000)	
Health aid	-7.516***	-8.390***	-7.148***	-8.626***
	(2.849)	(3.218)	(2.632)	(3.043)
Health aid × Urban	7.596*	2.662		
	(3.919)	(4.429)		
Health aid × City (pop>50,000)			9.484**	6.001
			(4.222)	(5.272)
Birth controls	Yes	Yes	Yes	Yes
Country-year FE	Yes	No	Yes	No
ADM1-year FE	No	Yes	No	Yes
Mother FE	Yes	Yes	Yes	Yes
Observations	962,317	962,292	962,311	962,286

Notes: The dependent variable is an indicator for whether a child died before reaching the age of one year, scaled by 1,000. The independent variables are an indicator for whether at least one health aid project was active in the four years before the child was born within 30km of the DHS cluster and an interaction with this indicator. Health aid is interacted with an indicator for whether the child was born in an urban enumeration area or in a city with more than 50,000 inhabitants. Birth controls are gender and indicator variables for birth order and for multiple births. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.6: Health aid in areas with high nighttime light intensity per capita in 1994

	(1)	(2)	(3)	(4)	(5)	(6)
	30km buffer		Ethnic homeland		ADM1 region	
Panel A: Higher than p25						
Health aid	-11.541***	-12.025***	-16.731**	-23.978***	-10.064**	-17.949***
	(3.782)	(4.336)	(6.865)	(8.748)	(4.140)	(5.340)
Health aid × High NTL pc	10.523**	7.331	13.576*	18.013**	7.399	13.922**
	(4.335)	(5.118)	(7.151)	(9.161)	(4.617)	(6.281)
Panel B: Higher than p50						
Health aid	-9.518***	-10.767***	-3.941	-7.128	-11.245***	-11.972***
	(3.522)	(4.055)	(4.473)	(5.404)	(3.366)	(4.001)
Health aid × High NTL pc	8.134*	5.806	-0.591	-0.987	11.310***	8.640
	(4.190)	(5.023)	(5.017)	(6.197)	(4.133)	(5.638)
Panel C: Higher than p75						
Health aid	-7.723***	-9.045***	-6.340**	-8.547***	-8.939***	-11.252***
	(2.723)	(3.218)	(2.585)	(3.126)	(2.931)	(3.384)
Health aid × High NTL pc	9.383**	5.967	7.378	3.350	10.078**	12.191**
	(4.229)	(5.509)	(4.734)	(6.152)	(4.050)	(6.116)
Birth controls	Yes	Yes	Yes	Yes	Yes	Yes
Country-year FE	Yes	No	Yes	No	Yes	No
ADM1-year FE	No	Yes	No	Yes	No	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes
Area	Buffer	Buffer	Homeland	Homeland	ADM1	ADM1
Observations	962,317	962,292	954,737	954,713	962,317	962,292

Notes: The dependent variable is an indicator for whether a child died before reaching the age of one year, scaled by 1,000. The independent variables are an indicator for whether at least one health aid project was active in the four years before the child was born within 30km of the DHS cluster and an interaction with this indicator. Health aid is interacted with an indicator for whether the child was born in an area with nighttime light per capita higher than the 25/50/75th percentile in 1994 in Panels A/B/C, respectively. Nighttime light per capita is measured in the 30km buffer around the DHS cluster (columns 1 and 2), in the ethnic homeland (columns 3 and 4), and in the ADM1 region (columns 5 and 6). Birth controls are gender and indicator variables for birth order and for multiple births. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.7: Health aid and local institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Ethnic homeland		Ethnic homeland		ADM1 region		Ethnic homeland		ADM1 region	
	Centralized		AB index, round 3		AB index, round 3		AB index, round 4		AB index, round 4	
Health aid	3.580	-1.111	-57.992	-55.179	-30.222	-91.042	-46.590	-69.029	-25.023	-88.990
	(33.754)	(44.779)	(53.283)	(66.741)	(29.989)	(56.450)	(52.641)	(69.429)	(30.242)	(57.814)
Health aid × Centralized	1.458	-1.216								
	(4.681)	(6.127)								
Health aid × AB index			11.414	11.664	18.670	31.765	-15.027	23.600	10.876	26.386
			(30.799)	(40.026)	(28.977)	(40.273)	(27.806)	(37.129)	(25.670)	(33.240)
Health aid × Log NTL	1.380	-0.596	3.038	2.315	3.334**	3.848*	1.719	1.943	3.085**	3.444*
	(1.490)	(2.288)	(2.412)	(3.276)	(1.358)	(2.223)	(1.952)	(2.889)	(1.269)	(2.084)
Health aid × Log pop	-0.450	-0.546	3.979	3.443	1.861	5.761	3.941	3.811	1.763	5.774
	(2.270)	(2.951)	(3.713)	(4.569)	(1.913)	(4.357)	(3.449)	(4.308)	(1.944)	(4.186)
Birth controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
ADM1-year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area	Homeland	Homeland	Homeland	Homeland	ADM1	ADM1	Homeland	Homeland	ADM1	ADM1
Observations	879,459	879,433	549,980	549,950	571,685	571,670	554,911	554,881	606,080	606,067

Notes: The dependent variable is an indicator for whether a child died before reaching the age of one year, scaled by 1,000. The independent variables are an indicator for whether at least one health aid project was active in the four years before the child was born within 30km of the DHS cluster and interactions with this indicator. Health aid is interacted with an indicator for whether the DHS cluster is located in a centralized homeland and with an index for local political institutions at the homeland and ADM1 level constructed from Afrobarometer surveys round 3 and 4. Additionally, interactions of health aid with log nighttime light and log population are included. Birth controls are gender and indicator variables for birth order and for multiple births. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.8: Health aid and country-level economic development and institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Interaction with:	Log GDP pc		Democracy		Control of corruption		Rule of law		Govt. eff.	
Health aid	-26.879	-26.477	-43.295*	-39.832	-31.234	-29.885	-15.115	-17.787	-23.162	-20.012
	(24.725)	(31.561)	(26.008)	(33.117)	(26.054)	(32.856)	(27.454)	(35.677)	(25.894)	(32.925)
Health aid × Log GDP pc	4.284	3.594	6.569	5.498	4.723	3.893	2.647	2.397	3.870	2.935
	(4.582)	(5.941)	(4.717)	(6.112)	(4.649)	(5.997)	(4.868)	(6.385)	(4.635)	(6.001)
Health aid × Democracy			10.158**	8.531						
			(4.692)	(5.918)						
Health aid × Control of corruption					-2.526	-2.346				
					(5.212)	(6.412)				
Health aid × Rule of law							3.291	2.448		
							(3.729)	(4.749)		
Health aid × Government effectiveness									1.729	3.373
									(4.480)	(5.583)
Birth controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ADM1-year FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	930,982	930,957	930,982	930,957	930,982	930,957	930,982	930,957	930,982	930,957

Notes: The dependent variable is an indicator for whether a child died before reaching the age of one year, scaled by 1,000. The independent variables are an indicator for whether at least one health aid project was active in the four years before the child was born within 30km of the DHS cluster and an interaction with this indicator. Health aid is interacted with log GDP per capita in 1994, an indicator for whether the country was democratic in 1994 and variables capturing control of corruption, rule of law, and government effectiveness in 1996. Birth controls are gender and indicator variables for birth order and for multiple births. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Appendix 3.A: Index of local political institutions based on Afrobarometer surveys

To construct the index of local political institutions, I use the following questions:

- *How much do you trust each of the following, or haven't you heard enough about them to say: Your local government council?* Answers: 0: *not at all*, 1: *just a little*, 2: *somewhat*, 3: *a lot*

- *How many of the following people do you think are involved in corruption, or haven't you heard enough about them to say: Local government councilors?* Answers: 0: *none*, 1: *some of them*, 2: *most of them*, 3: *all of them.* This variable is rescaled, such that the value 3 corresponds to *none* and 0 corresponds to *all of them.*

- *How much of the time do you think the following try their best to listen to what people like you have to say: Local government councilors?* Answers: 0: *never* 1: *only sometimes*, 2: *often*, 3: *always.*

- *Do you approve or disapprove of the way the following people have performed their jobs over the past twelve months, or haven't you heard enough about them to say: Local government councilor?* Answers: 1: *strongly disapprove*, 2: *disapprove*, 3: *approve*, 4: *strongly approve.* The answers are rescaled to the range 0 to 3.

The Afrobarometer clusters (enumeration areas) have the same precision codes as the AidData dataset. To compute the index at the ethnic homeland level, I keep clusters whose geocodes correspond to exact locations. For ADM1 regions, I keep clusters with geocodes corresponding to an ADM1 level or a smaller area (up to an exact location).

Appendix 3.B: Project evaluations and the effect of World Bank health aid on infant mortality

I test whether better-evaluated health projects have a higher effect on infant mortality. For completed projects, AidData provides the evaluation scores by the World Bank Independent Evaluation Group (IEG), an independent unit within the World Bank Group. Before the start of a project, its objectives are determined. At the end of the project, the IEG evaluates whether these objectives were achieved. Consider two of the earliest projects in the dataset: the *Health, Fertility and Nutrition Project* in Cameroon and the *Health Sector Recovery Project* in Mozambique. The objective of the project in Cameroon was to support the government in implementing its population and health policies. Most of the project's funds were allocated with the goal to increase the coverage and quality of primary health care services. The project was rated as highly unsatisfactory because most objectives were not achieved. For instance, although over 1,000 individuals received some training in primary health care, and 13 new health centers and hospitals were built, access and use of health care services did not seem to increase.[95] The objective of the project in Mozambique was to help the government improve the physical health of the population, especially by decreasing infant and child mortality.[96] It was rated as satisfactory because the health status of the population improved, and infant and child mortality rates decreased during the project's implementation.[97] These evaluations are summarized in the project outcome score, measured on a scale of one to six (ranging from highly unsatisfactory to highly satisfactory), which is provided by AidData.[98]

I compute the share of projects with an evaluation ranging from moderately satisfactory to highly satisfactory and the average evaluation score within the

[95] See https://projects.worldbank.org/en/projects-operations/document-detail/P000411 for more details on the project in Cameroon.

[96] Child mortality is the number of children dying before their fifth birthday.

[97] See https://projects.worldbank.org/en/projects-operations/document-detail/P001792 for more details on the project in Mozambique.

[98] See http://ieg.worldbankgroup.org/sites/default/files/Data/HarmonizeEvalCriteria.pdf and http://ieg.worldbankgroup.org/data for more information on the IEG evaluation criteria.

30km buffer around the DHS cluster.[99] In Table 3.B.1, I interact the health aid indicator with these two variables. In columns (1) and (2), I use the share of projects with a high evaluation, and in columns (3) and (4), the average evaluation score. None of the interaction terms is statistically significant, and they are even positive in three out of four specifications. Overall, there is no evidence that projects that receive a better evaluation by the IEG have a higher effect on infant mortality. These results might suggest that the effects of World Bank health projects go beyond what can be captured by the IEG evaluations.

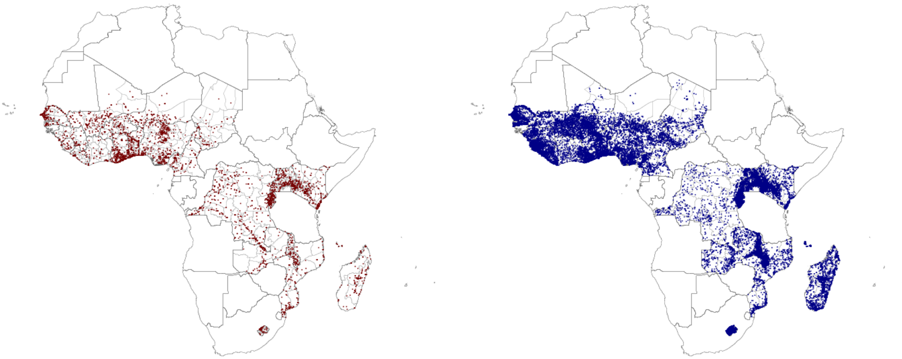
Table 3.B.1: Effect heterogeneity: Evaluation of project outcome

	(1)	(2)	(3)	(4)
Outcome:	Share w/ high score		Average score	
Health aid	-7.309*	-11.803**	-1.148	-14.831
	(4.263)	(4.893)	(8.815)	(11.155)
Health aid × Outcome	4.117	8.103	-0.841	1.948
	(4.859)	(6.082)	(2.008)	(2.662)
Birth controls	Yes	Yes	Yes	Yes
Country-year FE	Yes	No	Yes	No
ADM1-year FE	No	Yes	No	Yes
Mother FE	Yes	Yes	Yes	Yes
Observations	949,200	949,174	949,200	949,174

Notes: The dependent variable is an indicator for whether a child died before reaching the age of one year, scaled by 1,000. The independent variables are an indicator for whether at least one health aid project was active in the four years before the child was born within 30km of the DHS cluster and an interaction with this indicator. Health aid is interacted with the share of projects with positive evaluations and with the average project evaluation in the 30km buffer. Birth controls are gender and indicator variables for birth order and for multiple births. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

[99] Because only completed projects are evaluated, my sample size decreases by 13,755 children.

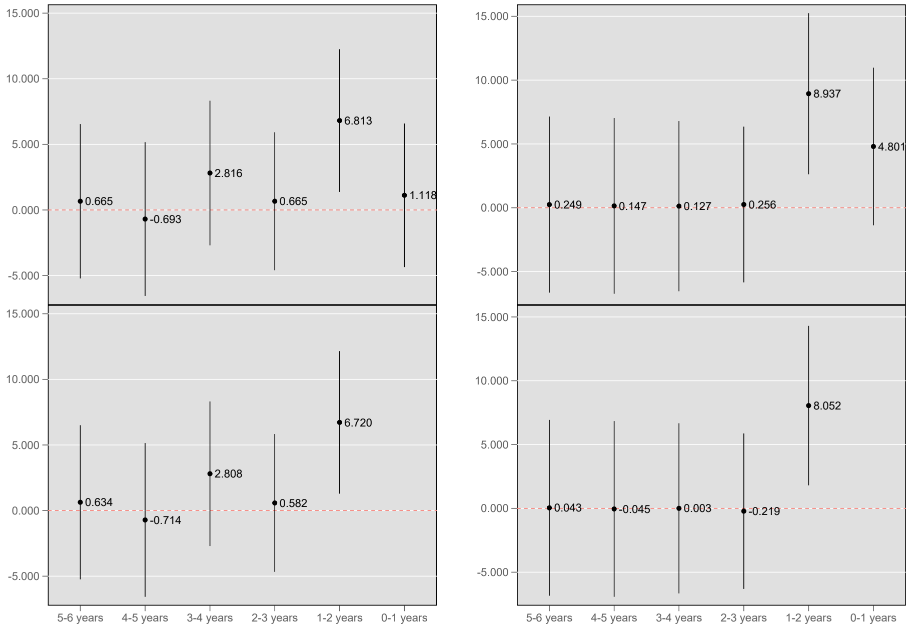
Appendix 3.C: Additional figures



(a) World Bank project locations, 1995-2014 (b) DHS clusters

Notes: Sub-figure (a) shows the precisely geocoded World Bank project locations, and Sub-figure (b) the DHS clusters in the 25 countries in the sample.

Figure 3.C.1: World Bank project locations and DHS clusters



(a) Mother and country-year fixed effects (b) Mother and ADM1 region-year fixed effects

Notes: This figure plots the results of regressing infant mortality on indicator variables for whether the child was born 0-1, 1-2, 2-3, 3-4, 4-5, and 5-6 years before the first project within 30km of the DHS cluster. The dots indicate the coefficient sizes, and the black lines indicate the corresponding 95%-confidence intervals. The lower panel does not include the 0-1 dummy. The regression controls for gender, indicator variables for birth order, multiple births, mother fixed effects, and country-year (Sub-figure a) or ADM1 region-year fixed effects (Sub-figure b), respectively. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level.

Figure 3.C.2: Pre-treatment trends

Appendix 3.D: Additional tables

Table 3.D.1: Additional descriptive statistics

Variable	Obs.	Mean	Std.Dev.	Min.	Max.
Additional birth characteristics					
Mother's age at birth	1,313,013	26.376	6.532	9	50
Birth spacing, months	1,068,163	34.890	19.096	9	298
Other aid categories					
Social aid, 4y, 30km	1,065,721	0.240	0.427	0	1
Economic aid, 4y, 30km	1,059,175	0.297	0.457	0	1
Local characteristics					
Urban	1,313,013	0.240	0.427	0	1
City (>50,000 inhabitants)	1,312,998	0.114	0.318	0	1
NTL in 30km buffer (1994)	1,313,013	0.568	2.197	0.000	38.332
NTL in, homeland (1994)	1,302,921	0.364	1.666	0.000	22.893
NTL in ADM1 (1994)	1,313,013	0.895	4.185	0.000	48.059
Population in 30km buffer	1,312,189	4.85e+05	7.69e+05	3.770	9.56e+06
Population in homeland (1994)	1,302,921	2.63e+06	3.43e+06	7,387.200	1.85e+07
Population in ADM1 (1994)	1,313,013	1.13e+06	1.11e+06	4,087.000	6.54e+06
Centralized homeland	1,201,095	0.551	0.497	0	1
AB index, homeland, round 3	749,624	0.530	0.165	0.036	0.950
AB index, homeland, round 4	755,555	0.490	0.143	0.000	0.929
AB index, ADM1, round 3	779,030	0.520	0.164	0.076	0.844
AB index, ADM1, round 4	927,179	0.505	0.150	0.107	0.911
Country-level characteristics					
Democratic (1994)	1,313,013	0.301	0.459	0	1
Control of corruption (1996)	1,313,013	-0.769	0.473	-1.648	0.112
Rule of law (1996)	1,313,013	-0.884	0.558	-1.928	0.098
Government effectiveness (1996)	1,313,013	-0.768	0.494	-1.720	0.076
Log GDP per capita (1994)	1,268,723	5.532	0.517	4.798	6.686

Notes: This table presents descriptive statistics for additional variables used in the robustness and heterogeneity analyses. The sample includes all children for which the treatment variables and at least one of the dependent variables are non-missing. All variables are described in the text. The additional birth characteristics are the mother's age at birth and the number of months since the previous birth. The other aid variables are indicators for whether there was any social (other than health) or economic aid project in the four years before the child's birth, within 30km of the DHS cluster. Local characteristics are dummies indicating whether the DHS cluster is in an urban area and in a city with more than 50,000 inhabitants; nighttime light and population in the 30km buffer, ethnic homeland, and ADM1 region; a dummy indicating whether the cluster is in a centralized homeland; and the Afrobarometer indices (rounds 3 and 4) in the homeland and ADM1 region. Country-level characteristics are dummies whether the country was democratic in 1994, the values of control of corruption, rule of law, and government effectiveness in 1996, and log GDP per capita in 1994.

Table 3.D.2: Effects of health aid on infant mortality: Disbursed amounts in logs

	(1) Controls	(2) Country-year FE	(3) Mother & Country-year FE	(4) Mother & ADM1-year FE
Health aid	-0.977***	-0.529***	-0.349**	-0.638***
	(0.059)	(0.069)	(0.167)	(0.211)
Birth controls	Yes	Yes	Yes	Yes
Country-year FE	No	Yes	Yes	No
ADM1-year FE	No	No	No	Yes
Mother FE	No	No	Yes	Yes
Log population	Yes	Yes	Yes	Yes
Observations	1,039,607	1,039,607	962,317	962,292

Notes: This table presents a robustness test of Table 3.2; see the corresponding table notes for details. The only differences are that the independent variable is the disbursed amount (+1) in logs in the four years before the child was born within 30km of the DHS cluster and a control for the log population in the 30km buffer around the DHS cluster. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level.. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.D.3: Effects of health aid on infant mortality: Controlling for mother's age and birth spacing

	(1) Controls	(2) Country-year FE	(3) Mother & Country-year FE	(4) Mother & ADM1-year FE
Health aid	-10.465***	-4.301***	-4.570**	-7.612***
	(0.777)	(0.921)	(2.228)	(2.820)
Birth controls	Yes	Yes	Yes	Yes
Country-year FE	No	Yes	Yes	No
ADM1-year FE	No	No	No	Yes
Mother FE	No	No	Yes	Yes
Observations	1,030,789	1,030,789	948,669	948,625

Notes: This table presents a robustness test of Table 3.2; see the corresponding table notes for details. 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. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level.. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.D.4: Effects of health aid on infant mortality: Different lag structures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years before birth	1	2	3	4	5	6	7	8	9	10
Panel A: Active years										
Health aid	-3.682	-4.311*	-3.419	-7.536***	-6.843**	-5.292*	-5.728*	-6.964*	-11.894***	-9.687**
	(2.254)	(2.395)	(2.643)	(2.821)	(2.931)	(3.197)	(3.397)	(3.832)	(4.337)	(4.798)
Observations	1,178,155	1,109,304	1,040,420	962,292	886,139	789,553	712,913	622,804	533,134	440,037
Panel B: End of project										
Health aid	-0.400	-6.120	-1.956	-10.747*	-12.147*	-7.480	-13.225*	-11.866	-18.920*	-15.434
	(5.773)	(5.167)	(5.504)	(5.987)	(6.265)	(7.075)	(7.654)	(8.877)	(10.762)	(11.918)
Observations	1,022,963	953,621	889,222	825,003	764,835	686,854	625,697	550,986	477,792	402,176
Birth controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year FE	No	No	No	No	No	No	No	No	No	No
ADM1-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents robustness tests of Table 3.2, column (4); see the corresponding table notes for details. In Panel A, the only difference is the lag structure: I consider projects in the 1, 2, ..., 10 years before the child's birth. Panel B replicates Panel A, but only considers the last year of a project instead of all active years: A child is treated if a project ended in the 1, 2, ..., 10 years before the child's birth. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.D.5: Effects of health aid on infant mortality: Different buffer sizes around DHS clusters

	(1) 5km	(2) 10km	(3) 15km	(4) 20km	(5) 25km	(6) 30km	(7) 35km	(8) 40km	(9) 45km	(10) 50km
Panel A: Main sample										
Health aid	-5.611	-3.845	-3.923	-3.778	-6.172**	-7.536***	-3.895	-3.200	-2.301	-2.512
	(4.546)	(3.772)	(3.270)	(3.066)	(2.973)	(2.821)	(2.783)	(2.723)	(2.649)	(2.590)
Observations	990,057	982,357	976,315	970,489	965,956	962,292	958,790	955,574	952,749	950,346
Panel B: Adapted control group										
Health aid	-14.168**	-10.087*	-7.990*	-7.627*	-9.367**	-8.386**	-6.763**	-4.888	-3.705	-2.512
	(6.917)	(5.548)	(4.557)	(4.098)	(3.776)	(3.450)	(3.185)	(3.001)	(2.782)	(2.590)
Observations	739,965	774,944	804,036	829,363	852,294	873,319	893,781	913,325	931,491	950,346
Birth controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year FE	No	No	No	No	No	No	No	No	No	No
ADM1-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table present robustness tests of Table 3.2, column (4); see the corresponding table notes for details. In Panel A, the only difference is that buffer sizes of 5, 10, 15,.., 50km are considered. Panel B replicates Panel A but restricts the control group to children who were never treated within 50km. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.D.6: Other aid

	(1) Controls	(2) Country-year FE	(3) Mother & Country-year FE	(4) Mother & ADM1-year FE
Health aid	-10.413***	-6.006***	-5.062**	-9.000***
	(0.897)	(1.041)	(2.299)	(2.908)
Social aid	1.377	0.185	-0.140	3.734
	(1.079)	(1.102)	(1.990)	(2.310)
Economic aid	-10.972***	-4.238***	2.466	1.003
	(0.969)	(1.008)	(1.902)	(2.133)
Birth controls	Yes	Yes	Yes	Yes
Country-year FE	No	Yes	Yes	No
ADM1-year FE	No	No	No	Yes
Mother FE	No	No	Yes	Yes
Observations	1,012,393	1,012,393	931,407	931,379

Notes: This table presents a robustness test of Table 3.2; see the corresponding table notes for details. The only difference is that I include dummies for whether there were any social (other than health) and economic projects by the World Bank in the 30km buffer in the four years before the child's birth. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.D.7: Children born after mothers moved to current place of residence

	(1) Controls	(2) Country-year FE	(3) Mother & Country-year FE	(4) Mother & ADM1-year FE
Panel A: Moving information available				
Health aid	-12.637***	-8.118***	-3.630	-4.169
	(1.380)	(1.676)	(4.123)	(4.892)
Observations	388,071	388,071	342,501	342,468
Panel B: Born after mother moved here				
Health aid	-13.235***	-9.225***	-3.444	-1.563
	(1.510)	(1.858)	(4.978)	(5.922)
Observations	304,032	304,032	255,546	255,507
Birth controls	Yes	Yes	Yes	Yes
Country-year FE	No	Yes	Yes	No
ADM1-year FE	No	No	No	Yes
Mother FE	No	No	Yes	Yes

Notes: This table presents a robustness test of Table 3.2; see the corresponding table notes for details. In Panel A, the only difference is that I only keep children of mothers who were asked and answered the question since when have they lived in their current place of residence. In Panel B, I further restrict the sample to the children born after their mother moved there. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Table 3.D.8: Health aid in urban areas and cities: Different buffer sizes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance to nearest project:	10km	10km	10km	10km	20km	20km	20km	20km
Interaction with:	Urban areas (DHS)		City (pop >50,000)		Urban areas (DHS)		City (pop >50,000)	
Health aid	-4.678	-3.888	-6.222	-7.220	-3.336	-3.597	-4.041	-5.102
	(6.440)	(6.574)	(4.783)	(4.996)	(3.555)	(3.861)	(3.158)	(3.502)
Health aid × Urban	7.806	0.060			4.764	-0.402		
	(7.210)	(7.539)			(4.511)	(4.987)		
Health aid × City (pop>50,000)			13.302**	7.498			8.318*	4.884
			(6.093)	(6.841)			(4.589)	(5.485)
Birth controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year FE	Yes	No	Yes	No	Yes	No	Yes	No
ADM1-year FE	No	Yes	No	Yes	No	Yes	No	Yes
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	982,375	982,357	982,369	982,351	970,514	970,489	970,508	970,483

Notes: This table presents a robustness test of Table 3.5; see the corresponding table notes for details. The only difference is that I consider buffer sizes of 10km or 20km instead of 30km. Standard errors (in parentheses) are adjusted for clustering at the DHS cluster level. ***, **, * indicate significance at the 1, 5, and 10%-level, respectively.

Curriculum Vitae

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EDUCATION

Ph.D. in Economics and Finance, University of St.Gallen, 2020

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VISITS

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Research Assistant, SIAW, University of St.Gallen, 2015–2020

Research Fellow, Busara Center for Behavioral Economics, Kenya, 01/2015–04/2015

Junior Assistant, Institute of Information Systems, 2013–2015

TEACHING

Poverty Alleviation (Undergraduate), Lecturer, University of St.Gallen, 2020

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Public Economics (Undergraduate), Teaching Assistant, University of St.Gallen, 2015–2017

Introduction to Microeconomics (Undergraduate), Instructor, University of Bern, 2014

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