The role of historical Christian missions in the location of World Bank aid in Africa

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July 12, 2019

Abstract

We document a positive and sizable correlation between the location of historical Christian missions and the within-country allocation of World Bank-financed development projects in Africa. The analysis is conducted on a geospatial grid of cells with a size of roughly 55×55 kilometers. The correlation is conditioned on observable geographical and historical factors that have shaped missionaries' settlement decisions. We do not find evidence that areas with historical missions display higher aid effectiveness, as measured by project ratings and survey-based development indicators. Using data on national elections, we document the existence of a political aid cycle specific to mission areas: The arrival of new projects is lower in the year of a turnover in the presidential office. We argue that durable political connections between mission areas and central governments may be one explanation for the correlation between missions and aid, but we cannot completely rule out other factors.

Keywords: Development Aid; Christian Missions; Political Favoritism; Africa
JEL-codes: F35; I3; N37; N77; O19

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1 Introduction

Where does foreign development aid go? This question is of central importance in the aid effectiveness debate. The World Bank has an explicit goal to end extreme poverty, and to focus on the poorest segment of the population (World Bank Group, 2013). This strategy would suggest that aid allocation should be guided by efficiency and need, but several empirical studies seem to suggest that allocation is biased by political and strategic considerations. Most of the early research on this subject has focused on the cross-country and across-time dimensions.[1] However, the more recent literature on the determinants of within-country aid allocation has come to similar conclusions: Various kinds of favoritism are important in explaining the spatial allocation of development aid (Dreher et al., 2019; Jablonski, 2014; Masaki, 2018).

We add to this literature by studying the role of history in shaping the present-day within-country allocation of aid. Although development aid in its present form is a post-World War II phenomenon, similar activities implemented by Westerners in developing countries began much earlier.[2] In particular, they can be traced back to the work by Christian missionaries, who were particularly active at the end of the 19th century. The missionary effort was primarily driven by proselytization motives, but it was not restricted to conversion. Missions provided the locals with a wide range of education and health services, primarily to boost the odds of conversion. In some ways, mission stations can be considered as the ancestors of modern micro-development projects.

In our main empirical analysis, we compare a snapshot of the location of mission stations in Africa in 1903 to the precise locations of World Bank-funded projects in 1995–2014. The unit of analysis is derived from a grid of 55×55 kilometer square cells covering the African mainland and Madagascar.[3] The results imply that the presence of (at least) one mission station increases the probability that an area is allocated a development project by approximately 50 %. We take several empirical measures to alleviate concerns of omitted variables bias. First, we control for country dummies in all specifications, because the first step of aid allocation is at the country level. Second,

[1] For example, Alesina and Dollar (2000) find colonial history and co-voting in the UN to be major predictors of donor-recipient foreign aid flows. Along the same lines, Dreher, Sturm, and Vreeland (2009) show that the World Bank allocates disproportionately more development projects to countries during their tenure as temporary members of the UN Security Council.

[2] The beginning of modern development aid coincides with the establishment of the World Bank in 1944, and the launch of the U.S.-sponsored Marshall Plan in 1948, aimed at reconstructing European economies after WWII.

[3] The main results are robust to collapsing the data to administrative levels 1 and 2 (regions and districts, respectively), as well as to ethnic homelands, as defined in Murdock (1959). Results available on request.

we address the non-random selection of missionaries into specific locations. To this end, we always exclude areas covered by desert or dense forest, and control for historical and geographical factors that guided the missionaries' settlement decisions according to historical sources.

Third, we show that the correlation is robust when restricting the sample to areas that are more likely to be similar: areas that intersect the coastline or one of the main rivers, and subsamples obtained by propensity score matching. Fourth, we show that the link between historical missions and aid survives also when controlling flexibly for present-day population density. Finally, the test developed by Oster (2019) to assess the extent of omitted variable bias suggests that only a small part of the estimated correlation is likely to be driven by unobservable factors.[4]

Our findings relate to the recent literature on the long-lasting effects of Christian missionary activities on development. Several convincing pieces of evidence point to large effects of both Catholic and Protestant missions on present-day education (Caicedo, 2018; Castelló-Climent, Chaudhary, & Mukhopadhyay, 2018; Mantovanelli, 2014; Meier zu Selhausen, 2014; Nunn, 2014; Okoye & Pongou, 2017; Waldinger, 2017), health (Cagé & Rueda, 2017; Calvi & Mantovanelli, 2018) and income (Caicedo, 2018; Chen, Wang, & Yan, 2014).[5] The literature also documents that the effects of missionary interventions are not explained by the persistence of infrastructure (e.g. schools or hospitals). Instead, it seems to be explained by the transmission of new values, introduction of better practices, and an increase in non-cognitive skills, such as collaborative behavior.

The literature described above offers one possible explanation for the correlation between the location of historical missions and the present-day geographical allocation of aid. Aid donors always face a trade-off between need and effectiveness and might decide to channel aid towards areas where the probability of success is higher. Areas that hosted historical missions might be more suited to successfully implement aid projects, thanks to higher levels of social and human capital. This would be consistent with recent evidence that more developed areas get more aid (Briggs, 2017; Nunnenkamp, Öhler, & Andrés, 2017).

[4]In a previous version of this paper (Alpino & Hammersmark, 2017) we also analyzed the allocation of Chinese-financed aid. The correlation between historical missions and Chinese aid is unstable across specifications, and it is not consistent across different sources of missionary data. In this version we focus only on World Bank-funded projects.

[5]Additional papers in this literature include Cagé and Rueda (2016), who find that the introduction of the printing press by Protestant missionaries facilitated the birth of newspapers and in turn the accumulation of social capital, and Kudo (2017) who finds that missionary-educated women marry later, and are less likely to get married with a polygamous husband. The positive effects of missionaries are also found outside developing countries. For example, Andersen, Bentzen, Dalgaard, and Sharp (2017) document positive effects of monasteries in medieval England.

In the second part of the paper (section 3) we put this hypothesis to an empirical test. We employ two strategies to this end. First, we compare the performance of projects implemented in the vicinity of missions to those further away, using the ratings by the World Banks's Independent Evaluation Group (IEG) as a proxy for project performance. We are not able to find a correlation between the vicinity to mission stations and project ratings, but the estimates are rather imprecise and thus does not allow to draw definitive conclusions. Second, we exploit information on start and end dates of the aid projects to implement a triple differences strategy. We compare areas that receive aid at different points in time to test whether aid arrival affects the level of development and, more importantly, whether the effect is higher in the vicinity of mission stations. Our proxies of development are constructed with georeferenced survey data from the Demographic and Health Survey (DHS), and include measures of wealth and access to public utilities. Under the identifying assumption that development trends (not levels) between areas with and without missions as of 1903 are comparable in the period 1995–2014, we find no evidence that mission presence matters for aid effectiveness. As with the first strategy, we cannot precisely estimate the absence of an effect, and thus the conclusion from this exercise is suggestive rather than definitive.

The last part of the paper (section 4) investigates whether favoritism can explain the correlation between aid and missions. First, we investigate the role of *political* favoritism. There is evidence from Africa that funds from the World Bank and the African Development Bank has been diverted to politically important areas, either competitive electoral districts (Masaki, 2018), strongholds of the incumbent regime (Briggs, 2014; Jablonski, 2014), or birth regions of presidents (Dreher et al., 2019). In light of the fact that areas close to historical missions have higher social capital and are more developed, it is reasonable to suspect that they are also politically more important. Mission areas may also have had strong connections with the central government in colonial times, which may have persisted over time. We put this hypothesis to an empirical test by estimating the existence of a political aid cycle specific to mission areas. Using a balanced annual panel and a specification that includes country-year as well as cell fixed effects, we find that areas in the proximity of mission stations experience a 40 % drop in the probability of receiving a new aid project in the year of a presidential turnover. A corresponding increase takes place in election years when the incumbent is reelected, although this estimate is less precise.

We also investigate the role of religious favoritism. There is evidence that Christian missionaries have been successful in converting indigenous peoples to Christianity (Nunn, 2010; Waldinger, 2017). As World Bank donors are predominantly Western and

Christian countries, they might prefer to channel aid to areas with a large Christian share of population. To probe this mechanism, we add the latter variable as a control in our baseline specification. Controlling for religion has virtually no effect on the correlations between mission presence and aid. Using the same empirical strategy, we also investigate the role of education, for which there is extensive evidence that missionary interventions matter. Contrary to religion, the inclusion of education in the baseline specification weakens the correlation between mission presence and aid. In light of this finding, we cannot rule out that human capital plays a role.[6]

2 Spatial correlation between aid and historical missions

2.1 Empirical strategy

We aim at testing whether the presence of historical mission stations is correlated with the present-day allocation of aid. The empirical strategy exploits within-country variation in missionary activity and aid across Africa. Since the location of missions is predetermined and does not vary over time, we collapse the temporal dimension of aid allocation into a cross-sectional dataset. The unit of observation consists of contiguous grid cells at a resolution of 0.5×0.5 degrees, which at the equator roughly corresponds to 55×55 kilometers. We superimpose the grid on the African continent, and keep the part covering the continental mainland and Madagascar.[7] Cells are split by borders, in order to make sure that aid projects are geographically assigned to the correct country. We further assign cells to the two highest subnational administrative levels from the GADM database of Global Administrative Areas (ADM1, corresponding to states or governorates; and ADM2 corresponding to districts, municipalities or communes), using cell center points ("centroids"). The baseline specification to be estimated by OLS is

$$EverAid_{ik} = \beta \cdot Mission_{ik} + \delta_k + \mathbf{X}_{ik}\gamma + \varepsilon_{ik} \ , \tag{1}$$

where i and k are indexes for cell and country, respectively. $EverAid_{ik}$ is binary variable equal to one if cell i had at least one active aid project in the period of study, and zero otherwise. $Mission_{ik}$ is an indicator equal to one if at least one historical mission was located in cell i, and zero otherwise.

[6] We stress that this exercise is problematic, because the regression controls for a covariate that is not predetermined with respect to mission presence. These findings are therefore suggestive and must be interpreted with caution.

[7] We drop cells in South Sudan to ensure consistent country fixed effects.

The baseline estimation only exploits within-country variation. The inclusion of country dummies δ_k controls for all time-invariant country-level characteristics, many of whom are important determinants of foreign aid.[8] To test for robustness, we also run models with dummies for subnational administrative divisions (ADM 1 or 2), instead of at the country-level.

The vector $\mathbf{X}_{ik}$ contains control variables at the cell level, including a number of historical and geographical factors, described in Section 2.2.2. The error term ε_{ik} is allowed to be spatially correlated within a radius of 220 kilometers (approximately four times the length of the side of a cell) from the centroid of each cell. This means that clusters are unique to each cell, and that a typical landlocked cluster covers about 45 contiguous cells.[9] Clustered standard errors are estimated using the estimator developed by Conley (1999).[10] This type of standard errors is the most appropriate because missions are highly clustered in our data (see Figure 1).

Coefficient β has a causal interpretation if and only if $\mathbf{X}_{ik}$ and δ_k contain all relevant determinants of mission locations that are also correlated with present-day aid allocation. Failure to include important controls will bias the size of the coefficient. The most obvious threat to a causal interpretation of β is non-random selection of mission stations. Missions are predetermined with respect to present-day aid, but they may have located in areas that were more suitable for missionary work, and areas where missionaries could survive and be self-sustained. If these areas for some reason, other than mission station presence, are more or less likely to be selected for aid projects, β will be biased. We have detailed information about the determinants of the location of mission stations from historical sources, notably from Johnson (1967) and Robinson (1915). Moreover, there is an increasing amount of detailed and spatially disaggregated historical data available, so we are able to plausibly control for most of these determinants: pre-colonial ethnic institutions, distance from coast and rivers, water accessibility, malaria prevalence, altitude, terrain characteristics, historical population and cities, distance from historical routes. On top of controlling for these factors, we also calculate the Oster (2019) bound to formally assess the extent of the bias due to unobserved controls.

A second source of bias is the lack of common support in the distribution of the control variables. Even if our set of controls $\mathbf{X}_{ik}$ fully accounts for the selection problem, the simple OLS estimator will still be biased if the cells hosting a historical mission (the

[8] For example, to be eligible for IDA (International Development Association) funds from the World Bank, a country must be below a threshold level of GNP per capita (Galiani, Knack, Xu, & Zou, 2017).

[9] Coastal clusters are obviously smaller.

[10] The standard errors are calculated using the Stata program *x_ols*, written by Jean-Pierre Dube, available at http://economics.uwo.ca/faculty/conley/.

treated observations) are very different in their covariates compared to cells without a historical mission (the control observations), and if the control function is incorrectly specified (Imbens & Rubin, 2015). We deal with this issue in several ways: first, our sample always excludes cells covered for more than 90 % by deserts and/or forests in the 18th century. Second, we run additional regressions on samples restricted to coastal cells and to those that intersect one of the main African rivers.[11] Third, we restrict the analysis to subsamples obtained using propensity score matching.

Finally, a third source of bias derives from the spatial nature of our data. If mission stations are clustered, we may be overestimating or underestimating the impact of a single mission station. We assess robustness to spatial correlation by constructing spatial lags of mission stations and adding them to the regression model.

2.2 Data

2.2.1 Aid

Data on foreign aid is based on World Bank projects in the period 1995–2014, geocoded by AidData. Other geocoded datasets on aid exist, but this is the one which covers the whole African continent (and beyond) for the longest period. The World Bank dataset contains projects from both the International Bank for Reconstruction and Development (IBRD) and the International Development Association (IDA), in total 1,900 projects in Africa, split across 16,553 different locations. The IBRD provides low or zero interest rate loans to sufficiently credit-worthy countries, whereas the IDA gives loans to poorer and less credit-worthy countries. 12 % of IDA funds are given as grants not to be paid back. Both types of lending is accompanied by technical assistance from the Bank, and projects are monitored by Bank staff.[12]

The data contains information on all locations in which a given project has been implemented. Locations are categorized from 1 to 8 according to the level of geographical disaggregation of their coordinates, with categories named, somewhat misleadingly, "precision categories". Precision 1 locations correspond to a specific place, that is a populated place of some kind (e.g. village, town, city) in approximately 80 % of the cases, or to a third-order administrative division (ADM3, i.e. neighborhoods, suburbs) in approx-

[11] Nile, Niger, Senegal, Zambezi, Congo and its attributes Ubangi and Kasai.

[12] The information available at the project level includes the original World Bank identifier, project title, date of approval, expected date of completion, share in different sectors (Finance, Transportation, Energy, Health, Education, Agriculture, Water, Industry & Trade, Information Communication Technology, Public Administration.), lending instrument (development policy lending vs. investment), local implementing agency, total committed and disbursed amount, completion and supervision cost, independent evaluation rating.

imately 15 % of the cases.[13] Precision 2 locations are similar to precision 1, but their reported coordinates are not as accurate, with coordinates being within 25 kilometers from the exact location. Precision 3, 4 and 6 locations correspond to second-order administrative divisions (ADM2), first-order administrative divisions (ADM1), and countries, respectively. Note that precision 3, 4 and 6 categories refer to projects intended to serve the whole administrative division (e.g. training for the all the public employees in a province). They do *not* refer to imprecisely georeferenced point locations. Precision 5 locations are imprecisely geocoded, thus only approximate coordinates are reported; precision 7 locations are "unclear", in the sense that it was possible to identify only the country in which the project is located. Finally, precision 8 locations correspond to capital cities (both national and local), and also include projects aimed at government institutions (ministries, central bank, etc.). The distribution of location precision categories is reported in Table 1.

Table 1: Precision of aid locations

	World Bank (%)
Precision 1 – specific place	40.9
Precision 2 – within 25km of specific place	2.4
Precision 3 – municipality (ADM1)	25.7
Precision 4 – province (ADM2)	19.7
Precision 5 – imprecise	1.9
Precision 6 – country-wide projects	4.4
Precision 7 – unclear	0
Precision 8 – state or national capitals	5

Notes: The table reports the percentage of locations in each precision category. World Bank sample composed by 1,900 projects in Africa.

Most projects are implemented across several locations (around 40 on average), often of different precision categories. Consider for example the case of a project aimed at building a road to connect two towns located in two different provinces. In this case, at least four locations are assigned to the project: the two towns as precision 1 locations, and the two provinces as precision 4 locations, plus any province crossed by the road as precision 4, and any town as precision 1. For the purpose of our analysis, we retain only locations of precision 1 or 2: categories 3, 4 and 6 are too coarse to be uniquely assigned to a cell in our grid-level analysis, and we do not want results to be driven by capitals. This leaves us with 768 projects (40 %) and 12,318 project-locations (74 %).

[13] First-order administrative divisions (ADM1) are the largest administrative units (provinces, states, or governorates), second-order administrative divisions (ADM2) are the next level (district, municipality, or commune), third-order (ADM3) are sub-divisions of ADM2s (neighborhoods or suburbs), and so on.

These sample restrictions raise the question of whether the excluded projects are systematically different from the retained ones. To investigate this aspect, we check whether the included projects are also implemented in locations with different precision categories (see online appendix). Reassuringly, more than 60 % of the projects retained in our sample are also assigned to at least one location at the ADM1 (precision 4) and/or ADM2 level (precision 3). We also compare the projects in our sample to projects with at least one location at precision 3 or 4, but without any at precision 1 or 2, in terms of observable characteristics (see online appendix). In most respects, the projects in the two groups are broadly similar, although there are some differences in terms of sectoral composition. The projects in our sample have, on average, smaller shares in agriculture, health and education, and larger shares in energy, transport and water sanitation. This suggests that our sample of precise point locations has, not surprisingly, a disproportionate share of projects dedicated to building facilities and infrastructure.

2.2.2 Mission stations

We rely on two different historical sources to retrieve information on the location of Christian mission stations. Our preferred source is the *Geography and Atlas of Christian Missions* (Beach, 1903), digitized by Cagé and Rueda (2016). It includes the location of Protestant mission stations in Africa as of 1903, coupled with information on the investment of each mission (school, dispensary, hospital, etc.). Our alternative source is *Ethnographic Survey of Africa: Showing the Tribes and Languages* (Roome, 1924) digitized by Nunn (2010). This source reports locations of both Protestant and Catholic foreign mission stations in Africa as of 1924.

As apparent from Figure 1, the two sources do not perfectly overlap. The cell-level correlation between Protestant missions from Beach (1903) and Roome (1924) is 0.31. One reason for the low correlation might be that missionaries only started penetrating the African inland after first settling on the coast. Hence, the 1924 data has a higher concentration of mission stations further from the coast. We adopt a conservative approach, and thus we conduct our analysis using both sources separately. The results of virtually all the analyses are quantitatively similar, and qualitatively identical, using either one of the two atlases to construct $Mission_{ik}$. We report the baseline correlation (equation (1)) obtained using both sources, but, for ease of exposition, we only present further results obtained with data from Beach (1903).[14]

[14] Results using Roome (1924) data are available upon request.

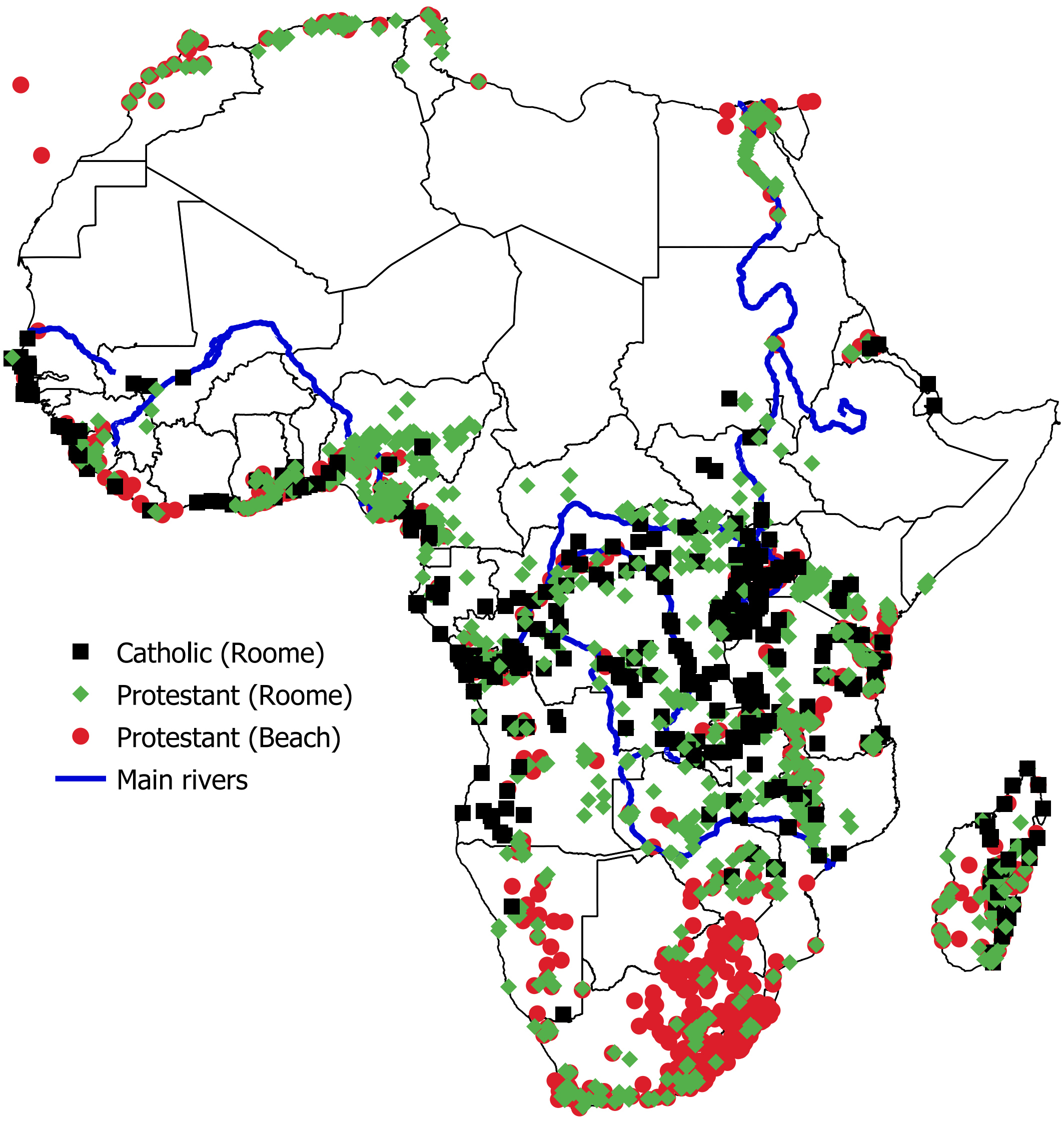

Figure 1: Location of mission stations and main rivers

These two atlases are standard sources for georeferenced mission stations in Africa in the economic literature. However, Jedwab, Meier zu Selhausen, and Moradi (2018) document that both sources are subject to measurement error in the exact location of missions due to geocoding mistakes. They also show that this issue of classical measurement error is greatly alleviated using large enough cells. In particular, they almost completely eliminate this issue by increasing cell size up to 0.3 x 0.3 degrees, while we use even larger cells at a resolution of 0.5 x 0.5 degrees. Jedwab et al. (2018) also collect more complete records of missions in Ghana using multiple sources. They are thus able to document that for this country the correlation between their location and those reported in Beach (1903) and Roome (1924) is very high only for the first established missions, but is less complete for those established later.[15] As early missions are located in better and more accessible areas, this might induce non-classical measurement error, which tends to magnify omitted variable bias induced by self-selection of missionaries. In the next section we explain how we cope with the selection issue.

2.2.3 Selection of missions and historical controls

Missionary activity in Africa was not randomly assigned across the continent, as illustrated by the case studies in Johnson (1967) and confirmed empirically in Jedwab et al. (2018). If the factors that determine the selection of mission station locations correlate with present-day aid allocation, coefficient β in equation (1) will be biased.

The first factor to consider is accessibility. Missionaries came by sea, and inland penetration was difficult, so missionaries followed the tracks of early European explorers, which partly correspond to the course of the main rivers. There is evidence that areas along coast and rivers have an advantage in development, and are more densely populated (Gallup, Sachs, & Mellinger, 1999), so we control for the (log) kilometer distance from the closest point on the coast and from the closest main river.[16] We also control for the (log) distance to the closest colonial railway, which had long-lasting effects on urbanization and growth in Africa (Jedwab, Kerby, & Moradi, 2017; Jedwab & Moradi, 2016), and distance to the closest explorer route. Finally, as a (inverse) measure of accessibility, we also include terrain ruggedness. Rugged landscape made it easier to hide from slave traders (Nunn & Puga, 2012), but it also enables rebel warfare (Fearon & Laitin, 2003).

The second factor to consider is the capacity to keep the settlement self-sustained for

[15]The correlation increases as cell size increases.

[16]We use distances in logs of because the marginal effect of a unit of proximity is likely to approach zero as distance increases. More specifically, we use $\log(1 + \text{distance})$, to avoid losing the cells at zero distance.

a long period of time. Self-sustainability crucially depended on access to water and land cultivation suitability, which are likely to be important for present-day outcomes as well. We thus control for both factors, proxied respectively by the *Caloric Suitability Index* (CSI) (Galor & Özak, 2016), and by the share of cell area that is within 10 kilometers of a water source. Missions were also more likely to establish in high altitudes, partly to avoid diseases like malaria, but also because of a more comfortable climate (Johnson, 1967). We therefore control for average elevation and for its interaction with a dummy for the tropics. As a further control for disease environment, we include a measure of malaria prevalence, the *Malaria Ecology Index* developed in Kiszewski et al. (2004).

In addition, the existence of different ethnic groups may have played an important role in the missionaries' settlement decisions. Unobserved variables at the ethnicity level may introduce biases, in light of research showing that pre-colonial ethnic institutions had long-lasting effects on development and public good provision in Africa (Gennaioli & Rainer, 2007; Michalopoulos & Papaioannou, 2013). We tackle this concern by including a separate dummy for each of the more than 800 pre-colonial ethnic homelands, whose boundaries are from Murdock's (1959) ethnolinguistic map. We assign cells to the ethnic polygon that covers the highest percentage of its surface. Regressions therefore only exploit within-ethnicity variation, and results cannot be driven by factors varying across ethnicities.

We also need to account for the main missionary purpose, namely conversion of Africans to Christianity. In particular, we are concerned that missionaries might have targeted more populated areas or cities. We thus control for the fourth polynomial in average population density in the 18th century from the History Database of the Global Environment (HYDE). Furthermore, we also include a dummy for the presence of cities at any time before 1800.

According to Robinson (1915), competition with Islam was a deterring factor, because spreading the Gospel in predominantly Muslim areas was complicated. Muslim populations may receive less development aid for political and religious reasons, so we control for the (log) distance from the closest Arab medieval trade route (which Michalopoulos, Naghavi, and Prarolo (2018) show had strong impact on adherence to Islam).[17] Table 2 presents summary statistics of the controls for cells with and without missions. More detailed information on data sources are available in the online appendix.

[17] Some missions were set up for the purpose of ending slave trade (Johnson, 1967), a practice that was especially prevalent along the coast of West Africa, and had long-lasting detrimental effects on development and social capital (Nunn, 2008; Nunn & Wantchekon, 2011). We are not aware of precisely georeferenced measures of slave trade; however, we are already controlling for many of its correlates, e.g. distance to the coast, terrain ruggedness, distance to Arab trade routes, and for ethnic-level dummies.

Table 2: Difference in means of control variables

	No mission	Mission	Difference
Log(Distance to coast)	5.76	4.01	1.76***
Log(Distance to main river)	5.29	5.89	−0.60***
% area within 10 km of water	0.07	0.10	−0.03***
Malaria Ecology Index	11.64	6.82	4.83***
Caloric mean index / 1000	1.33	1.57	−0.24***
Terrain Ruggedness Index	17.18	26.42	−9.25***
Mean elevation	714.05	750.26	−36.22
Tropical dummy	0.86	0.56	0.31***
Log(Distance to explorer route)	3.62	4.20	−0.58***
Log(Distance to colonial railway)	5.28	3.59	1.68***
18th cent. population	11.46	23.48	−12.02***
Precolonial city	0.01	0.04	−0.04***
Log(Distance to Arab trade)	5.44	6.09	−0.64***
No. observations	6512	380	

Notes: Missions from Beach (1903). * p<0.1; ** p<0.05; *** p<0.01.

2.3 Results

We estimate equation (1) by least squares, and the results are reported in Table 3. The dependent variable is an indicator equal to one if the cell ever received a World Bank project between 1995 and 2014.[18] All regressions include the full set of historical and geographical controls described above. We first experiment with different definitions of the dummy $Mission_{ik}$. In the first four columns, the mission data is from Roome (1924). In the first, we only include Catholic missions, in the second only Protestant, in the third both separately, and in the fourth both collapsed into a unique dummy. In the fifth column, we only include Protestant from Beach (1903), and in the sixth Protestant from Beach (1903) and Catholic from Roome (1924). Finally, in column 7, we collapse Protestant and Catholic missions using data from both sources together.

The correlation between historical mission presence and Word Bank aid location is positive and significant across the columns of Table 3. The estimated coefficients imply that cells with missions are approximately 45 % to 80 % more likely to host a World Bank project, compared to the sample mean. In order to assess the bias from unobservables, we draw on the procedure constructed by Oster (2019) to get a lower

[18]Here and in the rest of the paper we always estimate Linear Probability Models (LPM) when the dependent variable is binary, instead of using non-linear models. As we are interested in estimating differences in averages between groups, rather than predicting outcomes, the LPM performs just as well as non-linear models, but the coefficients are more straightforward to interpret, especially in case of interaction terms. Furthermore, we often include large sets of dummies in the regressions, which regularly causes the likelihood maximization algorithm to fail to converge in case of logit or probit models.

Table 3: Aid and missions from Beach (1903) and Roome (1924)

	World Bank aid 1995–2014						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Catholic mission (Roome)	0.20***		0.18***			0.19***	
	(0.03)		(0.03)			(0.03)	
Protestant mission (Roome)		0.15***	0.14***				
		(0.02)	(0.02)				
Any mission (Roome)				0.17***			
				(0.02)			
Protestant mission (Beach)					0.13***	0.11***	
					(0.03)	(0.03)	
Any mission							0.15***
							(0.02)
Ethnic dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.25	0.25	0.25	0.25	0.25	0.25	0.25
Oster bound	0.11	0.07		0.08	0.09		0.10
R-sq.	0.40	0.40	0.40	0.40	0.39	0.40	0.40
N	6876	6876	6876	6876	6876	6876	6876

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees ($\approxeq$ 220 km). The dependent variable is a dummy for at least one project commitment in sample period. Control variables include log distance to coast, explorer route, colonial railway, and Arab trade route; 3rd order polynomial in 18th century population; presence of a city as of 1800; country dummies; pre-colonial ethnic dummies; average altitude; terrain ruggedness index; percentage area within 10 km from water source; caloric suitability index; malaria ecology; tropics dummy (also interacted with mean altitude). Cells that are more than 90 % covered by barren land or more than 90 % covered by forest are excluded from the sample. The lower bound on the coefficient of interest is calculated as in Oster (2019): we set the R-squared from the hypothetical regression on $Mission_{ik}$ and both observed and unobserved controls equal to 1.3 times the R-squared from the actual regression on $Mission_{ik}$ and observed control. * p<0.1; ** p<0.05; *** p<0.01.

bound for β. This test formalizes the common practice of inspecting the stability of the coefficient of interest when controls are added. It is a refinement of Altonji, Elder, and Taber (2005), because it takes into account whether controls absorb residual variation. This is important for the credibility of the exercise, as we should not expect to observe coefficient instability when adding controls that are unrelated to the outcome variable. Under the assumption that selection on observables has the same direction as selection on unobservables, the test produces lower bound coefficients close to 0.1 (see lower panel of Table 3, row "Oster bound"), corresponding to mission cells having a 40 % higher likelihood of aid allocation. Importantly, the Oster bound is always significant at the 99 % level.[19]

[19] We set the R-squared from the hypothetical regression where unobserved controls are included equal to 1.3 times the R-squared from the actual regression on $Mission_{ik}$ and observed controls, as suggested by Oster (2019). The procedure is suited for models with only one treatment, so we do not calculate the bounds in column 3 and 6. Confidence intervals are calculated using standard errors from the regressions.

The estimated correlation is higher for Catholic missions, but the difference between denominations is significant at the 90 % level only in column 6, when using data from two different sources. Furthermore, we have no way to address differential selection of Protestant versus Catholic missionaries, so it is difficult to interpret the difference between the two coefficients. Finally, if we drop Congo DRC from the sample, the difference between the two disappears in column 3 and halves in column 6, becoming not significant. Congo DRC covers 7 % of the sample cells, making it the largest country in the sample, and has a high concentration of Catholic missions. Hence, the differential in the coefficient size seems to be driven by this heavyweight outlier. In the remaining parts of the paper we only report results on the mission data from Beach (1903) to simplify the exposition.

2.4 Robustness tests

Subsample analysis The first two robustness tests are aimed at restricting estimation of equation (1) to subsamples that are more likely to exhibit common support in the covariate distribution (see Table 4). First, we restrict the sample to cells on the coast or one of the main rivers (C/R column). As discussed in Section 2.1, this subsample is likely more homogeneous and enhances the credibility of our selection on observables strategy, that relies on mission and non-mission cells having the same covariate distributions (Imbens & Rubin, 2015). Then, we proceed in a more structured way by constructing three subsamples where observations are balanced on propensity scores, using three different strategies. The first strategy (PS1) has no geographical restrictions in estimation of propensity scores or matching between treatment and control groups. In the second (PS2), we estimate propensity scores separately within each country, and include treatment-control pairs that are statistical neighbors in the same country. In the third (PS3), we estimate propensity scores on the full sample, but include only treatment-control pairs that are neighbors within the same country.[20]

The subsample analysis yields results similar to those in the baseline Table 3. Across the different subsamples, the coefficient of interest is always positive, significant at least at the 95 % level, and sizable (35 % of the sample means).

[20]For PS1: we estimate a logit model on the full sample using $Mission_{ik}$ as the dependent variable, and $\mathbf{X}_{ik}$ (except ethnic dummies), its interactions, its squared terms, and country dummies as predictors. We then match mission cells with their nearest (statistical) neighbor without replacement using the predicted values as propensity scores. For PS2: we re-estimate propensity scores separately country by country (using the logit model but without interactions and squared terms, because the individual country samples are small), and restrict the pool of possible matches to cells that belong to the same country. For PS3: we re-estimate propensity scores with the same logit model as for the PS1, but without country dummies. We then perform the same matching procedure, but country by country.

Table 4: Aid and missions (Beach, 1903): Subsample analysis

	WB aid 1995–2014			
	C/R	PS1	PS2	PS3
Mission	0.11^{**}	0.12^{***}	0.07^{**}	0.10^{***}
	(0.05)	(0.03)	(0.03)	(0.03)
Ethnic dummies	Yes	Yes	Yes	Yes
Mean dep. var.	0.43	0.36	0.29	0.35
R-sq.	0.52	0.66	0.66	0.68
N	844	799	604	794

Notes: "C/R" stands for coast or river subsample. "PS" refers to different subsamples obtained by propensity score matching. Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 220 km). The dependent variable is a dummy for at least one project commitment in sample period. Control variables are the same as in table 3. Rivers include: Nile, Niger, Senegal, Zambezi, Congo and its attributes Ubangi and Kasai. * p<0.1; ** p<0.05; *** p<0.01.

Controlling for present-day population In all the regressions estimated so far, we have only included controls that are predetermined with respect to the variable of interest, $Mission_{ik}$. This approach is appropriate when the goal is causal interpretation of β. However, it forces us to rely heavily on historical controls, some of which are likely to be measured with error, in particular population density. If the measurement error in the historical variables is severe, we may fail to control credibly for the selection of missions. Furthermore, there is reason to believe that the presence of mission stations and the activities of missionaries have influenced settlement patterns. In that case, the coefficient on $Mission_{ik}$ partly captures a correlation between current population and aid allocation.

To tackle these concerns, we introduce different measures of present-day population in the control set. Under the assumption that population density is positively autocorrelated, the present-day measures may serve as proxy controls for historical population. Under the additional assumptions that selection on population is positive, and that the presence of missions stations increase population, we can interpret the coefficients on $Mission_{ik}$ from these regressions as a lower bound on the true causal effect.[21] The results from these regressions are reported in Table 5.

In the first column, we include fourth order polynomial terms of population in 1995.

[21] See Angrist and Pischke (2008) for a discussion of this point, and Michalopoulos and Papaioannou (2013, footnote 13) for an example.

Table 5: WB aid and missions (Beach, 1903): Present-day population controls

	WB aid 1995–2014		
	All	All	PrC
Mission	0.10***	0.07***	0.18***
	(0.03)	(0.03)	(0.05)
Pop. 1995 (4th polynomial)	Yes	Yes	Yes
Pop. place dummy	No	Yes	No
Ethnic dummies	Yes	Yes	Yes
Mean dep. var.	0.25	0.25	0.62
R-sq.	0.40	0.43	0.64
N	6876	6876	698

Notes: "PrC" stands for a sample of cells that include province capitals. Conley (1999) standard errors in parentheses, with cutoff at 2 degrees ($\approxeq$ 220 km). Estimation by ordinary least squares. The dependent variable is a dummy for at least one project commitment in sample period. Control variables are the same as in table 3. * p<0.1; ** p<0.05; *** p<0.01.

In the second, we add a set of dummies for the presence of populated places of different size.[22] In the third, we restrict estimation to a sample of cells which host province capitals, because these cities are likely to be both large in terms of population, and important from a political point of view. The regressions survive all three robustness tests. Estimates are still significant at least at the 99 % level, and effects are comparable to the previous ones (35 % of the sample means).

Spatial spillovers Our baseline equation (1) does not account for the possibility that benefits from hosting a mission station in one cell may spill over to the surrounding cells. On the one hand, this might induce an overestimation of the effect of missions on aid. In cases where a pair of neighboring cells both hosted missions, but where only one receives aid, the neighbor mission may be adding to the aid attraction. On the other hand, it might induce an underestimation of the effect of missions on aid, because missionary presence in one cell might increase the probability to attract aid in surrounding cells, even if those do not host any mission themselves. In both cases, failure to account for the presence of missions in surrounding cells may induce omitted variable bias, in the first case with a positive sign, in the second with a negative sign.

[22] Dummies for the presence of at least one: a) national capital; b) province capital; c) urban agglomeration of at least one million people, or city with at least 500,000 people; d) urban agglomeration of at least 250,000 people, or city with at least 100,000 people; d) urban agglomeration of at least 100,000 people, or city with at least 50,000 people; e) places with at least 10,000 people; f) places with at least 1,000 people.

To tackle concerns of spillover bias from neighboring cells, we run a regression where we add spatial lags of missions to our baseline regression. The lag variable is an indicator for presence of at least one mission in one of the (up to) 8 cells surrounding cell i (what we call inner ring, see Figure 2 for an example).[23] We also include the interaction of the lag variable with the mission dummy. The coefficient on the lag variable captures the effect of having a mission in the neighboring cell, apart from hosting a mission in the cell itself. The coefficient on the interaction term captures the additional effect due to the contemporaneous presence of missions both in the cell itself and in the inner ring.

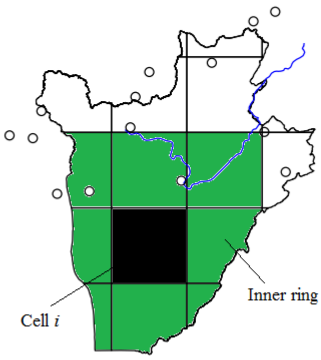

Figure 2: Example – spatial lags in Burundi

Results are reported in Table 6. The coefficients on the first spatial lag and its interaction with $Mission_{ik}$ are very close to zero and insignificant at conventional levels. Furthermore, their inclusion does not affect the estimate of our main coefficient of interest (columns 2 and 3) relative to the baseline (column 1). Inclusion of a second spatial lag (indicator for mission presence in one of the up to 16 cells surrounding the inner ring) yields similar results (reported in the online appendix). We take this as evidence that our estimated correlation holds mostly at the local level (the cell itself), with no role for spatial spillovers to surrounding cells.

Sensitivity tests The correlation between WB aid and historical missions are largely robust to various other sensitivity tests, available in the online appendix. The additional sensitivity tests are the following: First, we check if the estimated relationships are stable

[23]Since the location of World Bank-funded projects is likely determined by each country's government, they are probably not correlated across country borders. Allowing for cross-border correlation would bias the results, since zero correlations between border cells would pull down the overall estimate. Cross-border spatial correlation could be relevant if missions had persistent effects on surrounding areas, especially before current borders were put in place. The inclusion of cross border cells in the weighting matrix has virtually no impact on the results, and we present only our preferred specification here.

Table 6: WB aid and missions (Beach, 1903): Spatial lags

	WB aid 1995–2014		
	(1)	(2)	(3)
Mission	0.11***	0.11***	0.14***
	(0.03)	(0.03)	(0.05)
Mission Lag1		0.01	0.01
		(0.02)	(0.02)
Mission × Mission Lag1			-0.04
			(0.06)
Ethnic dummies	Yes	Yes	Yes
Mean dep. var.	0.26	0.26	0.26
R-sq.	0.43	0.43	0.44
N	5840	5840	5840

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 220 km). The dependent variable is a dummy for at least one project commitment in sample period. Mission Lag 1 refers to the (up to) 8 neighbors adjacent to each cell. Control variables are the same as in table 3. * p<0.1; ** p<0.05; *** p<0.01.

over time, splitting the sample in two, before and after the 2005 Paris Declaration.[24]

Next, we test whether the observed pattern holds for aid within all sectors (e.g. health, transport, etc.). If we detect differences across sectors, this might give some hints about the underlying mechanisms. Most projects span several of the ten sectors, and we have information on sector shares for each project. For each sector s, we construct two cell-level measures of aid: a binary indicator equal to one if at least one project with s as the largest sector has ever been implemented in the cell; a binary indicator equal to one if at least one project with a positive share in s has ever been implemented in the cell. Then, we estimate the baseline equation (1) for all these new sectoral outcome variables. Irrespective of the sector considered, coefficients on the mission dummies are always positive, statistically significant at conventional levels, and large relative to the mean of the dependent variables (estimates are presented in the online appendix). Magnitudes are highly comparable across different sectors.

Furthermore, we check if the use of binary treatment and outcome variables is important for the results. When using the number of missions and the log number of missions + 1 as treatment variables, we obtain results very similar to the baseline. We

[24]The Paris Declaration, signed at the "Second High Level Forum on Aid Effectiveness" organized by the OECD in 2005, was aimed at transferring more management and discretion to recipient countries. See http://www.oecd.org/dac/effectiveness/parisdeclarationandaccraagendaforaction.htm

also investigate whether the relationship between aid and missions is also at work at the intensive margin, replacing the dependent variable with the number of World Bank aid projects. Estimates from OLS, Poisson and Negative Binomial regressions confirm our baseline results. Finally, the correlation between WB aid and missions also survives the inclusion of subnational fixed effects at the ADM1 (state/governorates) or ADM2 (districts, municipalities or communes) level.

Taking stock, this section has documented a robust and sizable spatial relationship between World Bank aid and the historical presence of mission stations. Although it is hard to claim causality with observational data, the estimated relationship survives a vast array of robustness tests, including the most recent procedure suggested by the econometric literature to assess the bias from unobservable factors (Oster, 2019).

3 Implications for aid effectiveness

Having established that mission areas attract a disproportionate share of World Bank aid, it is natural to ask whether this has implications for aid effectiveness. Answering this question is interesting both from a policy perspective, and also to better understand the mechanism at play. For example, it is possible that missionary interventions have paved the ground for aid interventions later on, by providing suitable conditions for effective project implementation. These conditions might include cooperative behavior, trust in foreigners, and specific skills (e.g. language), all factors that the literature has found to be positively affected by missionary activity. This section aims at testing whether aid projects implemented in mission areas are more successful in achieving their goals. To do so we conduct two empirical tests, one using data on project ratings, and one using survey data on development outcomes from the Demographic and Health Survey (DHS).

3.1 Project ratings

Data and empirical strategy Each World Bank project is headed by a team leader who is also responsible for evaluating its success (relative to the stated goal) upon completion. After the initial evaluation, the World Bank's Independent Evaluation Group (IEG) performs a second assessment based on available project documentation. Furthermore, the IEG performs an additional in-depth evaluation of approximately 25 % of the projects, which includes on-site visits and additional analysis (Denizer, Kaufmann, & Kraay, 2013). Each layer of evaluation rates the projects on a six-point scale between "Highly satisfactory" and "Highly unsatisfactory".

We have rating information on 43 % of the projects included in our grid analysis, and we use these data to test whether projects implemented in the vicinity of missions display higher ratings. Since rating information is at the project level, rather than location level, we depart from our grid structure, and we construct a project-level dataset (recall that each project is implemented across several locations). Our baseline specification to be estimated by OLS is

$$Rating_{pk} = \beta \cdot MissionLocations_{pk} + \mathbf{X}_p\gamma + \mathbf{W}_k\delta + \varepsilon_{pk} \ , \tag{2}$$

where p and k are indexes for project and country, respectively.

$Rating_{pk}$ is a binary variable equal to one if the evaluation of project p is "Satisfactory" or better, and zero otherwise (we take the most recent available IEG evaluation, that is desk reviews in 87 % of the cases). $MissionLocations_{pk}$ is the project's fraction of precision 1 and 2 locations that are within 25 kilometers of a mission station (this radius implies observational units that roughly correspond to the size of cells in the grid structure). To select the relevant covariates, we follow Denizer et al. (2013). At the country level, we include average GDP per capita growth over the life of the project (from the World Bank), and the sum of the Freedom House scores of civil liberties and political rights.[25] At the project level, we include project length (in years), the log of total committed funds, a dummy for new projects (vs. follow-ups), a dummy for investment projects, sector dummies, share in largest sector, and the log of completion and preparation costs relative to total committed funds.

Results In Table 7 we present estimates from six different variants of equation (2). The regression reported in column 1 does not include any controls, the second one includes all controls expect cost variables (which are not available for many projects), and the third column includes the entire control set. Columns 4 to 6 replicate the same specifications but also include sector fixed effects.

The coefficient on the fractions of locations in the vicinity of a mission is small, and its sign is not consistent across specifications. The standard errors are at least three times larger than the coefficient. In short, Table 7 shows no evidence that mission presence is correlated with better (or worse) project performance, as measured by the IEG ratings.

The results in Table 7 should be interpreted with some caution. The sample size is relatively small, and the explanatory variable is subject to measurement error, due to the

[25] Denizer et al. (2013) also include CPIA ratings from the World Bank but we do not find these data for the period before 2005.

Table 7: World Bank's IEG project rating and missions (Beach, 1903)

	IEG rating: satisfactory (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of locations with missions	0.02	0.02	-0.02	0.01	0.02	-0.01
	(0.07)	(0.07)	(0.09)	(0.07)	(0.07)	(0.10)
Sector dummies	No	No	No	Yes	Yes	Yes
Mean dep. var.	0.67	0.67	0.66	0.67	0.67	0.66
R-sq.	0.00	0.05	0.16	0.06	0.11	0.20
N	324	324	188	324	324	188

Notes: Robust standard errors in parenthesis. The dependent variable is equal to one if the IEG rating is at least moderately satisfactory, and zero otherwise. Regressions without controls in column 1 and 4. * p<0.1; ** p<0.05; *** p<0.01.

need to keep the dataset at the project level. These factors imply low statistical power to reject the null hypothesis. Furthermore, the IEG rating is an imperfect measure of project performance. It measures performance relative to a goal, which is not the same across sectors, and it is at least partly based on documentation produced by the team leader. Furthermore, although formally independent, it is materially conducted by present and future World Bank employees (Denizer et al., 2013). Finally, the nature of our test is descriptive, because it does not account for non-random locations of projects to mission areas. This means that we are able to draw suggestive, but not definitive conclusions about project effectiveness.

3.2 Survey data on development outcomes

Our second empirical strategy attempts to overcome the limitations of the first, by relying on a quasi-experimental setup, and on direct measures of economic development. To identify effects of World Bank aid, we exploit the longitudinal dimension of the aid data and obtain a panel of cells-years spanning the period 1995–2014. Equipped with this data, we can compare development in cells that received aid at different points in time, exploiting information on the date of project approval and completion. The regression equation to be estimated by OLS has the form:

$$\begin{aligned} Y_{ikt} =& \kappa \cdot Mission_{ik} + \beta \cdot ActiveAid_{ikt} \cdot Mission_{ik} + \gamma \cdot ActiveAid_{ikt} \\ &+ \delta \cdot CompletedAid_{ikt} \cdot Mission_{ik} + \theta \cdot CompletedAid_{ikt} \\ &+ \mu \cdot EverAid_{ik} \cdot Mission_{ik} + \nu \cdot EverAid_{ik} + \lambda_{kt} + \varepsilon_{ikt} \ , \end{aligned} \tag{3}$$

where i, k and t are indices for cell, country and year, respectively.

The outcome variable Y_{ikt} is a measure of economic development (e.g. electrification or access to water). $ActiveAid_{ikt}$ is a binary indicator for the presence of at least one active project in year t.[26] As the effects of aid will not necessarily materialize immediately in the years of project implementation, we also include $CompletedAid_{ikt}$, which is a binary indicator for the past presence of a project. (The indicator is equal to one in every year after completion of the first project.) $EverAid_{ik}$ is a binary indicator equal to one if cell i has ever received at least one World Bank project in the sample period, and $Mission_{ik}$ an indicator equal to one if at least one historical mission was located in the cell. Fixed effects at the country-year level (λ_{kt}) are always included.

Specification (3) amounts to a triple differences setup ("difference-in-differences-in-differences"): $EverAid_{ik}$, $Mission_{ik}$ and their interaction control for time-invariant differences between different categories of cells. The coefficient on $ActiveAid_{ikt}$ captures whether Y_{ikt} is higher when a project is active (relative to periods before the arrival of aid), and β whether more so in cells that hosted a historical mission. The coefficient on $CompletedAid_{ikt}$ captures whether Y_{ikt} is higher after the completion of a project (relative to periods before the arrival of aid), and δ whether more so in cells that hosted a historical mission. We are interested in both β and δ because it is not *a priori* clear when the effects of aid should materialize.

It is important to stress that here we are mainly interested in testing whether mission cells cause higher or lower aid effectiveness, and not to test whether aid is effective per se, as this is outside the scope of our paper. As such, our identifying assumption is that conditional on $EverAid_{ik} = 1$, cells with and without missions have parallel trends in Y_{ikt}. Identification of β and δ (the coefficients of interests) does not require any assumption about parallel trends between cells that receive aid at different points in time, or between cells that ever/never received aid. These assumptions would be arguably very demanding, as they amount to saying that the timing of aid implementation is random. On the contrary, our identifying assumption is much less demanding, and it states that development trends (not levels) between cells with and without missions as of 1903 are the same in the period 1995-2014.

In order to implement this strategy, we need a measure of economic development at the cell-level observed at several points in time. We construct proxies of development from individual-level georeferenced survey data from the Demographic and Health Survey (DHS). Specifically, we use the individual recode of the DHS, which includes women of reproductive age (15–49), as it has the highest country coverage (in some countries, DHS only surveys women). We select four questions that cover different dimensions of

[26] A project is defined as active if the year t is between commitment and completion.

development and are asked consistently across countries and time. We collapse answers at the cell level to obtain four variables measuring the fraction of cell respondents with certain characteristics. The characteristics are the following: fraction with piped water as main source of drinking water;[27] fraction with electricity; fraction who own a television and/or a radio; fraction with floor made of modern material.[28] These measures are not necessarily representative at the cell-year level, but the number of respondents is high (cell mean is 190, median is 105), and the geographical and temporal coverage as well (1,900 cells, 4,200 cell-year observations, and on average 200 cells per year).[29]

Results In Table 8 we present estimates of equation (3) for each measure of Y_{ikt}. The odd-numbered columns serve as benchmarks, because they correspond to the simple difference-in-differences (no interaction with the mission dummy). Across different outcomes, the coefficient on $ActiveAid_{ikt}$ is positive and significant at the 99 % level, which means that when a World Bank project is active (between commitment and completion) the outcome is between 30 % (for electricity and piped water) and 9 % (for radio/TV owners) higher relative to years before the arrival of aid. The coefficient on $CompletedAid_{ikt}$ is also positive, but smaller, and significant only in the case of piped water. These estimates have a causal interpretation under the assumption that the timing of aid arrival is unrelated to trends in outcome. If this assumption is not met, the estimates conflate the effect of aid with the selection bias (e.g. cells growing faster being able to attract more aid). Finally, the coefficient on $EverAid_{ik}$ is positive and significant. This is consistent with the literature, which finds that aid does not go to the least developed areas. We take some comfort from these estimates because they suggest that our outcome variables are indeed decent proxies of development at the cell-year level.

The even-numbered columns correspond to the triple differences because they also include the mission dummy, and its interaction with the aid indicators. Relative to the odd-numbered columns, the inclusion of the interactions does not affect the estimated coefficients on the variables already included in the odd-numbered columns. Furthermore, the coefficients of interest on both interactions of interest do not have consistent signs across different outcomes and are almost always not statistically significant. In particular, the sign on the interaction between $ActiveAid_{ikt}$ and $Mission_{ik}$ is negative and insignificant for the outcome "Radio/TV", and positive and insignificant for the other outcomes. The size of the positive coefficients is (at least 50 %) smaller relative to the

[27] Other sources are worse: well water, surface water, rainwater.

[28] Non modern floors are for example leaves, sand, etc.

[29] We drop cell-year observations with less than 50 respondents. See the online appendix for a map of geographical coverage.

coefficient on $ActiveAid_{ikt}$ alone. The sign on the interaction between $CompletedAid_{ikt}$ and $Mission_{ik}$ is negative for the outcomes "Radio/TV" and "Proper floor" (significant in the former case), and positive and insignificant for the other two outcomes. As in the previous case, the size of the positive coefficients is (at least 60 %) smaller relative to the coefficient on $CompletedAid_{ikt}$ alone.

As a robustness test, we have also estimated a more parsimonious model where $ActiveAid_{ikt}$ and $CompletedAid_{ikt}$ are collapsed together in a unique treatment variable equal to one in the year of commitment of the first project, and in all the subsequent years. The estimates of the coefficient on the interaction of interest (not reported) are again small and not significant. To sum up, in this section we do not find any evidence that development outcomes are higher in cells with missions, relative to cells without missions, neither after nor during the implementation of aid projects.

There are a few caveats to this analysis that we want to highlight. First, the exercise does not estimate a precise "zero effect", thus we cannot definitively conclude that aid does not at all work better in mission areas. Second, using actual development as outcome variables (instead of project ratings) has some disadvantages: Although *access* to water and electricity may be direct products of specific aid projects, the outcomes themselves measure ownership of private goods. The time span of the analysis may not be long enough for the effects of development aid to materialize into private consumption or investment. Furthermore, proxies based on DHS data might not capture the dimensions of aid which are most affected by the projects. Finally, we note that if the "true" differences in aid success between cells with and without missions are small in magnitude, measurement error in several variables might result in too much attenuation bias to be able to estimate them.

3.3 Discussion

The analyses above are derived from a corollary of a potential mechanism, which is that aid is allocated to mission areas because it is thought to be more effective there. However, the results from two different empirical exercises do not lend support to this hypothesis. This conclusion rests on the assumption that we are actually able to measure effectiveness, as well as a number of assumptions of the validity of the test. There is also an implicit assumption that the allocating authorities have a reliable way to observe effectiveness, which may not be the case. If so, the allocation of aid to mission areas may be related to a prior belief that aid will be more effective there, a belief which is not updated because of a lack of evidence.

Table 8: Triple differences: aid effectiveness and missions Beach (1903).

	Electricity		Piped water		Radio/TV		Proper floor	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Active aid	0.11***	0.10***	0.13***	0.12***	0.06***	0.06***	0.11***	0.10***
	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)
Completed aid	0.02	0.03*	0.07***	0.07**	0.02	0.02	0.03	0.04*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
Ever had aid	0.06**	0.05***	0.06***	0.05**	0.04***	0.04***	0.08***	0.07***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
Mission		0.11*		0.11*		0.09***		0.08**
		(0.06)		(0.06)		(0.03)		(0.03)
Mission × Active aid		0.01		-0.03		0.02		0.02
		(0.05)		(0.05)		(0.02)		(0.04)
Mission × Completed aid		-0.04		0.02		0.00		-0.07*
		(0.06)		(0.06)		(0.03)		(0.04)
Mission × Ever had aid		0.02		0.04		-0.06**		0.06**
		(0.07)		(0.06)		(0.03)		(0.03)
Mean outcome	0.29	0.29	0.30	0.30	0.69	0.69	0.44	0.44
Cells	1970	1970	1971	1971	1970	1970	1971	1971
Observations	4201	4201	4202	4202	4201	4201	4202	4202

Notes: OLS estimates. Standard errors clustered at the country level. Country-year fixed effects always included. The dependent variable is the fraction of respondents whose dwelling include the feature reported above each regression. * p<0.1; ** p<0.05; *** p<0.01.

4 Potential mechanisms

In Section 2, we have established that areas close to historical missions tend to attract more World Bank aid. In the previous section, we tested whether this has implications for aid effectiveness, and were unable to find evidence for this in the data. Here we explore mechanisms (or mediators) through which the location of historical missions might have affected the present-day allocation of aid. Our point of origin is the literature on development aid allocation and on effects of historical missions, from which we identify factors that might potentially act as mediator.

There is widespread evidence that aid targeting depends on political considerations. Regions with ties to the party in power tend to receive more development aid, and more resources in general. There is also evidence that missionary activities had long-lasting positive effects on social capital, collaborative behavior, and literacy, all factors that likely contribute to higher *de jure* and *de facto* local political power (see Section 1). To investigate the role of political ties, we engineer an indirect test, probing for the existence of a political aid cycle in mission areas.

Next, we investigate the role of religious favoritism. Western and predominantly Christian countries stand out as the largest shareholders of the World Bank.[30] If shareholders for some reason prefer to allocate more aid to areas that are culturally similar to their own countries, we should expect to find disproportionately more World Bank projects in African regions with a large Christian population, and in areas that have strong ties with Western countries. In both cases, cells with a history of Christian missionary activity are likely to stand out, as the presence of missions increased both Christian and Western footprints.

Finally, we investigate the specific role of education levels. Among all development dimensions affected by missionaries, human capital stands out as the single outcome for which we have the largest body of convincing causal evidence.

4.1 Political aid cycle

Empirical strategy and data If mission areas attract more aid because of stronger political connections, we might expect these connections to break down in the event of government turnover. Turnover is a relatively rare phenomenon in African countries, both in autocratic and democratic societies (in our data, the median country has two

[30] The U.S. is the largest (holding respectively 10 % in the IDA, and 16 % in the IBRD), and, although Japan is second (with respectively 8 % and 7 %), Western European countries together form a sizable group: UK, France and Germany together account for 15 % and 12 % of IDA and IBRD funds, respectively. Figure available at https://www.worldbank.org/en/about/leadership/votingpowers/.

turnovers in 20 years). Against this backdrop, political connections are both durable and valuable. In order to test for the existence of a differential political aid cycle, we construct an annual balanced panel of cells covering the period 1995–2014 and compare data on national elections with the timing of cell-level aid commitments. We estimate the following equation by OLS:

$$Aid_{ikt} = \beta \cdot Election_{kt} \cdot Mission_{ik} + \gamma \cdot Turnover_{kt} \cdot Mission_{ik} + \lambda_{kt} + \mu_i + \varepsilon_{ikt} \, , \quad (4)$$

where i, k and t are indices for cell, country and year, respectively.

The variable Aid_{ikt} is an indicator equal to one if at least one aid project is committed in the corresponding calendar year, with at least one project location in cell i. $Election_{kt}$ is a binary indicator equal to one if there is a national election for the office of head of state, and $Turnover_{kt}$ is an indicator equal to one if the election results in a change in the person holding office. Turnover is defined as a change in the head of state, irrespective of party affiliation.[31] The data on elections is from the Varieties of Democracy (V-DEM) dataset, version 7.1 (Coppedge et al., 2017). Fixed effects at the country-year level (λ_{kt}) and at the cell level (μ_i) level are included in all regressions.

Note that the inclusion of country-year fixed effects means that we cannot identify a general political aid cycle at the country level, which is outside the scope of our paper. It does, however, allow to us credibly test for the presence of a political aid cycle specific to mission cells. The coefficient β captures whether mission cells are more likely to receive aid in election years when the incumbent is reelected, and γ the additional effect in case of turnover. The presence of cell fixed effects gives a difference-in-differences interpretation of the parameters of interests β and γ.[32]

Findings Table 9 reports estimates of several variants of equation (4) (always including cell and country-year fixed effects). In the first column, we only include the interaction between $Election_{kt}$ and $Mission_{ik}$; the coefficient is very small, and not significant at conventional levels, suggesting that election years are not different than other years for aid arrival in mission cells. In the second column, we only include the interaction between $Turnover_{kt}$ and $Mission_{ik}$; the coefficient is negative and significant at the 95 % level, implying that the probability of receiving a new World Bank aid project is 40 % lower in years where the election results in a change in the head of state. The same coefficient is again negative and significant in column 3, where we include both interactions together.

[31] Estimates are virtually identical if we define turnover as change in party. Results available on request.

[32] This difference-in-differences setup is admittedly somewhat unconventional, due to the inclusion of two treatments in the same equation. We do, however, also run separate regressions for both treatments.

In this case, the coefficient on $Election_{kt} \cdot Mission_{ik}$ gets larger but it is still insignificant, which is to say we do not find evidence of any drop or increase in years where the election results in a victory of the incumbent head of state.

One remaining threat to identification stems from the cross-sectional correlation of $Mission_{ik}$ with several covariates. To account for this issue, Columns 4 to 6 replicate the first three regressions, but also includes the same historical and geographical correlates of missions as the baseline regression (equation (1)), interacted with $Election_{kt}$ and/or $Turnover_{kt}$. The coefficient on $Turnover_{kt} \cdot Mission_{ik}$ is again negative and significant at least at the 95 % level. The coefficient on $Election_{kt} \cdot Mission_{ik}$ becomes bigger and significant in column 6. The estimates from the last regression imply that the probability of aid arrival in mission cells increase by 40 % in election years when the incumbent is reelected, and decrease by approximately the same amount when the election yields a turnover in the head of state. Both effects are relative to non-election years due to the inclusion of cell fixed effects.

Table 9: Political aid cycle in mission areas (Beach, 1903).

	At least one WB project committed in cell-year					
	(1)	(2)	(3)	(4)	(5)	(6)
Election × Mission	-0.00		0.01	0.01		0.03**
	(0.01)		(0.01)	(0.01)		(0.01)
Turnover × Mission		-0.03**	-0.04**		-0.03**	-0.05***
		(0.01)	(0.02)		(0.01)	(0.02)
Country-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	Yes	Yes	Yes
Mean dep. var. non-mission cells	0.03	0.03	0.03	0.03	0.03	0.03
Mean dep. var. mission cells	0.07	0.07	0.07	0.07	0.07	0.07
Cells	6819	6819	6819	6811	6811	6811
Observations	136380	136380	136380	136220	136220	136220

Notes: OLS estimates. Standard errors clustered at the country level. Balanced annual panel, twenty years. Controls included in columns 4-6 are the same as in Table 3, but interacted with the Election and/or Turnover dummy. * p<0.1; ** p<0.05; *** p<0.01.

The cyclical pattern exhibited by aid to mission areas suggests that political ties to the central government might be relevant in explaining the correlation uncovered in Section 2. One interpretation consistent with the evidence is the following: Areas close to historical missions are able to develop better ties with the ruling head of state (or with his party) over time. The political connection gives these areas an advantage in the competition to allocate aid projects. In case of political turnover, the connection breaks down and these areas experience a temporary drop in aid. The (less robust) increase in election years without turnover might constitute the reward by the incumbent in exchange for political support, but the instability of this estimate makes it difficult to draw strong conclusions.

The findings in Table 9 cannot be explained by a general slowdown of project commitments due to government officials being busy with the electoral campaign, because the regressions include country-year fixed effects. One could possibly argue that the effect is driven by turnover between heads of state who belong to different religions, which would imply religious favoritism as opposed to political favoritism. However, this interpretation is feasible only if all (or most) turnovers are from a Christian to a non-Christian head of state. A qualitative check of this explanation using multiple sources (e.g. Encyclopedia Britannica and Wikipedia) suggests that most turnovers happen between individuals of the same religion, making this explanation implausible. Yet another alternative explanation might have to do with heads of state coming disproportionately from mission cells (or having a missionary education). However, to be consistent with our findings this interpretation would also require (most) turnovers to happen between individuals with different missionary background, and in one specific direction, which seems unlikely. Furthermore, (Dreher et al., 2019) test and reject the hypothesis that African presidents fuel a disproportionate amount of World Bank aid to their birth regions using a dataset with approximately the temporal and geographical coverage used in the present paper.

4.2 The role of Christian religion and of human capital

Empirical strategy and data Human capital and religion are potential mediators in the relationship between aid and historical missions, although investigation of this is challenging, as is any mediation analysis. Simply including proxies for the candidate mechanism in the baseline equation (1) is problematic for causal inference; If these variables really are mediators, they are per definition not predetermined with respect to $Mission_{ik}$, which causes a bad control problem (Angrist & Pischke, 2008). In lack of a quasi-experimental strategy like the one in Section 4.1, we proceed with caution, as interpretation of results relies on several assumptions. First, we make an assumption on the direction of the selection bias that we are introducing by controlling for a "post-treatment" variable M_{ik} (education or religion). Then, we estimate the equation

$$EverAid_{ik} = \beta \cdot Mission_{ik} + \vartheta \cdot M_{ik} + \delta_k + \mathbf{X}_{ik}\gamma + \varepsilon_{ik} \ , \tag{5}$$

with or without M_{ik}. When M_{ik} is included, and assuming that $Mission_{ik}$ positively (negatively) affects M_{ik}, we interpret the coefficient β as a lower (upper) bound of the true effect of $Mission_{ik}$ on $EverAid_{ik}$.[33] If the estimates of β from the regressions with or without M_{ik} are not significantly different from each other, we conclude that M_{ik} is not the main mechanism of interest.

We construct measures of education and Christian religion using individual-level georeferenced data from the individual recode of the DHS (same data source used in Section 3). The DHS asks respondents (women between 15 and 49) about their highest level of education achieved: no education, primary, secondary, and tertiary. We construct three measures of education at the cell-level: the average level of education, the fraction of respondents with at least primary education, and the fraction of respondents with at least secondary education. As in Section 3 we drop cells with less than 50 respondents, and obtain a sample of 2,264 cells, each with more than 400 respondents on average (median 185). The DHS also includes a question about respondents' religious beliefs, which we use to construct the fraction of respondents of Christian religion (any denomination). The geographical coverage is slightly smaller than for education, with a sample of 2,083 cells.[34] Consistent with the literature discussed in Section 1, we assume that historical missions had positive effects on both education and Christianity.

Results The first column of Table 10 is an estimation of equation (1) on the subsample of cells for which we have a measure of education from the DHS. The coefficient on the mission dummy has a similar size and precision compared to the full-sample results. In relative terms, however, the correlation is smaller, due to larger incidence of aid projects in this subsample of cells (50 % compared to 25 % in Table 3).

Columns 2 to 4 present models with different measures of education: average level, fraction with at least primary education, and fraction with at least secondary education. The coefficients on all three measures of education are positive, significant at the 99 % level, and large. Inclusion of either measure reduces the size of the coefficient on $Mission_{ik}$. Secondary education seems to be more important here; it has the largest coefficient, and its inclusion almost halves the mission coefficient. Under the assumption that missionary activities have positive effects on education, the coefficient on $Mission_{ik}$ is a lower bound on the true effect. This lower bound ranges between 60 % and 90 % of the main effect in column 1. This suggests that a heritage of high human capital might be one reason for more aid going to mission areas. However, there seems to be room for

[33]This is the same logic used when controlling for present-day population.
[34]The question about religion is not included in all countries for all rounds.

Table 10: Education, Christian religion and mission areas (Beach, 1903).

	World Bank aid 1995–2014					
	(1)	(2)	(3)	(4)	(5)	(6)
Mission	0.10**	0.07*	0.09**	0.06	0.08*	0.08*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
Average education level		0.44***				
		(0.05)				
At least primary education (share)			0.70***			
			(0.09)			
At least secondary (share)				0.92***		
				(0.10)		
Christians (share)						0.14**
						(0.07)
Ethnic dummies	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.50	0.50	0.50	0.50	0.51	0.51
Mean M		0.76	0.54	0.20		0.52
R-squared	0.43	0.46	0.46	0.46	0.45	0.45
Observations	2264	2264	2264	2264	2083	2083

Notes: OLS estimates. Same control set and outcome variable as in Table 3. Conley (1999) standard errors in parentheses, with cutoff at 2 degrees ($\approxeq$ 220 km). * p<0.1; ** p<0.05; *** p<0.01.

other explanations as well, since the coefficient on $Mission_{ik}$ is not reduced to zero.

In column 5, we again replicate the baseline specification for the subsample of cells for which we have a DHS measure of religion. Although religion seems to play an independent role in aid allocation, the coefficient on $Mission_{ik}$ does not change when a measure of Christian religion is included in the regression. This means that there is no evidence in our data that Christian favoritism is the mechanism behind the correlation uncovered in Section 2.

4.3 Discussion

Secondary education seems to partly explain why mission areas get more aid, although the evidence is not conclusive. Even if we take these results at face value, it is not obvious why higher levels of education should make areas attractive targets for aid. It could be that some level of human capital is necessary for successful implementation of projects, in which case this would be indirect evidence of the "effectiveness" hypothesis that we examine in Section 3. On the other hand, if we interpret the evidence of a political aid cycle as favoritism (as we argue), human capital could play a role as a necessary condition for the formation of political connections. These are certainly questions that can be investigated empirically, which we leave for future research.

5 Conclusion

This paper has documented that 19th and 20th century Christian missionary activity in Africa predicts the location of present-day World Bank aid allocation. We find that the probability of receiving development projects is about 40 % higher in areas that contain a historical Christian mission station. This result is the main contribution of the paper, although we have made attempts to explain the source of the correlation, and to explore policy implications.

First, we tested whether aid effectiveness is higher in areas that hosted historical missions. As missionary activity had long-lasting positive effects on social and human capital, it might be possible that donors channel aid to these areas in the hope that these endowments facilitate successful implementation of the projects. Using data on project ratings and survey-based development indicators, we were unable to find evidence of superior aid effectiveness in areas exposed to early missionary activity. These results are not conclusive, however, due to data limitations.

Second, we have investigated three potential mechanisms: the role of political favoritism, religious favoritism and education. Education is positively correlated with aid allocation, and its inclusion among controls in the baseline regression reduces the coefficient on missions. There is no evidence of religious favoritism, but we do find evidence of a political aid cycle specific to mission areas: aid commitments are reduced whenever a presidential turnover occurs. We interpret this finding as evidence that mission areas have better connections with the central government, giving them an advantage in the competition to attract development projects. Electoral turnover breaks these ties temporarily, whereas incumbent reelection seems to strengthen them. We conclude that political favoritism is likely to play a role, but we are not able to rule out other mediating factors.

The correlations reported in this paper generates a number of questions, which leaves ample room for further research. Our attempts to test for effectiveness did not establish conclusive evidence, and we think that this deserves more attention. And although we provided evidence of a political aid cycle, we could only make conjectures about why it is there. We also do not have a clear understanding of how the aid cycle actually matters for long term patterns of aid allocation. Lastly, our list of potential mechanisms is not exhaustive, and there might be other explanations that we have not thought of (and therefore not tested).

Funding

This work was supported by the Centre of Equality, Social Organization and Performance (ESOP). ESOP is supported by the Research Council of Norway through its Centres of Excellence funding scheme [179552]. Matteo Alpino also acknowledges support by the SFB 884 "Political Economy of Reforms" hosted at the University of Mannheim and funded by the German Research Foundation (DFG).

Acknowledgments

The views expressed in this paper are those of the authors and do not necessarily reflect those of the Bank of Italy. We are grateful to Valeria Rueda and Julia Cagé for sharing their data with us. We also thank Axel Dreher, Martin Flatø, Nicola Gennaioli, Rune Jansen Hagen, Andreas Kotsadam, Eliana La Ferrara, Edwin Leuven, Jo Thori Lind, Silvia Marchesi, Halvor Mehlum, Anirban Mitra, Kalle Moene, Alexander Moradi, Andreas Müller, Ola Olsson, Johanna Rickne, Anna Tompsett, Gaute Torsvik, Nina Bruvik Westberg, participants at the 2016 Conference on Development Economics and Policy, the 2016 Nordic Conference on Development Economics, the 2016 Frisch-PRIO Workshop on Foreign Aid, the 2016 African Economic History Network Conference, the 2017 CSAE Conference, and the 2017 Royal Economic Society meeting for helpful comments and suggestions.

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ONLINE APPENDIX
NOT FOR PUBLICATION

Appendix A Data

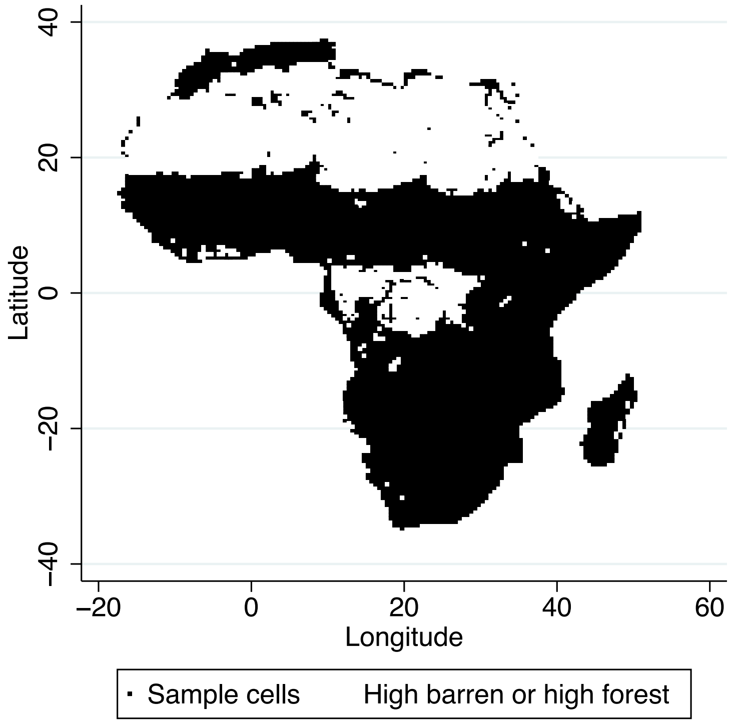

Figure A.1: Gridded sample. Blank spaces are cells covered by barren land or by forest for more than 90% of their surface in the XIX century (HYDE data).

Table A.1: Data sources

Data	Source	Link	Access date
World Bank projects	AidData	www.aiddata.org/	2016-06-22
World Bank project documents	World Bank	projects.worldbank.org/	2016-06-22
Missions in 1903	Beach (1903), Cagé and Rueda (2016)		
Missions in 1924	Nunn (2010), Roome (1924)	scholar.harvard.edu/nunn/	2016-06-15
Country borders	GADM	www.gadm.org/	2016-12-22
Coast line	Natural Earth	www.naturalearthdata.com/	2017-02-20
Rivers	Natural Earth	www.naturalearthdata.com/	2017-02-20
Explorer routes	Century Company, Nunn (2010)	scholar.harvard.edu/nunn/	2016-12-13
Colonial railways	Century Company, Nunn (2010)	scholar.harvard.edu/nunn/	2016-12-13
Gridded elevation data	United States Geological Survey	topotools.cr.usgs.gov/	2016-05-23
Caloric Suitability Index	Galor and Özak (2016)	www.omerozak.com/	2016-10-05
Water sources	WorldGeoDatasets (fee)	www.worldgeodatasets.com/	2016-05-20
Malaria Ecology Index	Kiszewski et al. (2004)	www.gordonmccord.com/	2016-12-13
18th century population	HYDE	www.pbl.nl/hyde/	2016-03-08
Historical cities	Chandler (1987)	www.worldcitypop.com/	2016-04-05
Ethnic groups	Murdock (1959), Nunn (2008)	scholar.harvard.edu/nunn/	2016-03-14
Arab trade routes	Kennedy (2001)	referenceworks.brillonline.com/	2016-09-03
Population in 1995	SEDAC	sedac.ciesin.columbia.edu	2016-02-06
Populated places	WorldGeoDatasets (fee)	www.worldgeodatasets.com/	2016-06-15
DHS variables	USAID DHS Program	www.dhsprogram.com/	2016-04-08
Elections	V-DEM	www.v-dem.net	2017-11-23
GDP growth	World Bank	data.worldbank.org/	2018-07-18
Rights and liberties	Freedom House	freedomhouse.org	2018-07-18

Table A.2: Frequency of other locations of the projects included in sample

	World Bank (%)
Projects with only precision 1 or 2 locations	15.7
Projects with also precision 3 and 4 locations	18.2
Projects with also precision 3 but not 4 locations	20.4
Projects with also precision 4 but not 3 locations	23.8
Projects with only precision 1,2 or 6 locations	11.2
Projects with only precision 1,2, or 8 locations	8.3
Residual category	2.2

Notes: The World Bank sample is composed by 768 projects, and the China sample by 800 projects.
The source of location information is World Bank project documents, and personal communication with project managers if additional detail is required. If locations cannot be retrieved from donor documents, AidData checks recipient country documents and aid management systems, or information from implementing agency websites. Locations may be towns, hills, farms, or other geographical features. The coders then search for coordinates in geographical databases like Geonames and Google Earth. If the name of a specific location cannot be matched with a set of coordinates, coders look for nearby towns or other identifiable features. The geocoding of the World Bank data is based on the same methodology as the UCDP Georeferenced Event Dataset. Described in detail in Strandow, Findley, Nielson, and Powell (2011).

Table A.3: Comparison: WB projects in sample vs. excluded WB project

	Not in sample	In sample	Difference
Committments (mil USD)	60.55	71.73	−11.18
Disbursments (mil USD)	23.25	29.83	−6.58
Start year	2,006.62	2,005.76	0.86*
End year	2,011.81	2,011.52	0.29
Length (in years)	5.95	6.56	−0.61***
Repeater (0/1)	0.27	0.27	0.00
Largest sector (%)	73.01	76.07	−3.06*
Completion cost (%)	1.43	1.21	0.22
Supervision cost (%)	2.57	2.52	0.05
IEG: satisfactory (0/1)	0.66	0.67	−0.01
Investment (0/1)	0.98	0.98	0.00
Agriculture (%)	16.56	7.33	9.24***
Public Admin (%)	20.33	20.52	−0.18
ICT (%)	0.19	1.95	−1.77**
Education (%)	11.96	6.16	5.80***
Finance (%)	2.41	1.95	0.47
Health (%)	28.46	11.16	17.30***
Energy (%)	4.01	13.76	−9.74***
Transport (%)	6.39	19.18	−12.80***
Water (%)	6.56	14.54	−7.98***
Industry & Trade (%)	3.12	3.45	−0.33
No. observations	300	768	

Notes: Projects in sample are those with at least one precision 1 or 2 location; projects "not in sample" have at least one precision 3 or 4 locations, but none at precision 1 and 2. * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

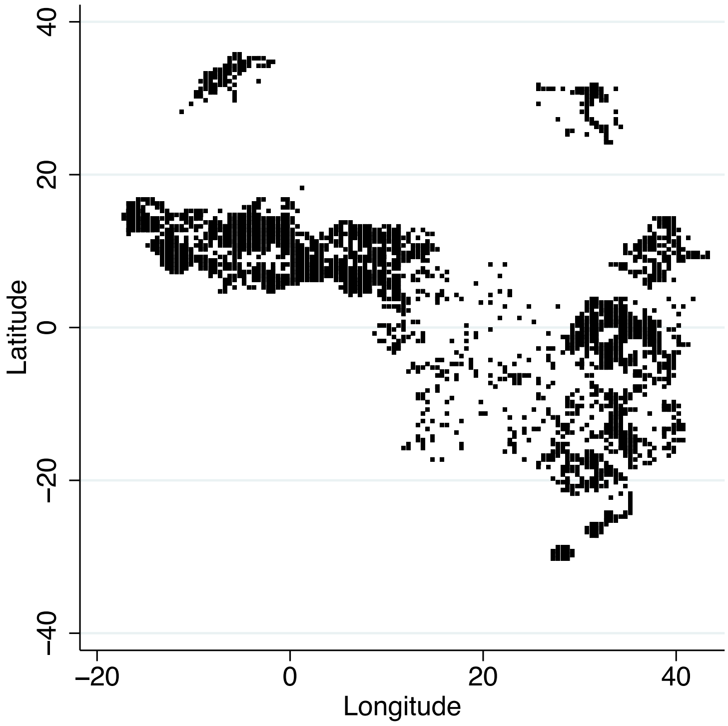

Figure A.2: DHS samples: black cells are those included in the analysis that rely on DHS data.

Appendix B   Supplementary material

Table B.1: WB aid and missions (Beach, 1903) in two decades

	Ever World Bank aid in period	
	1995-2004	2005-2014
Mission	0.09***	0.13***
	(0.02)	(0.03)
Ethnic dummies	Yes	Yes
Mean dep. var.	0.15	0.19
R-sq.	0.39	0.36
N	6876	6876

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 220 km). Estimation by ordinary least squares. The dependent variable is a dummy for at least one World Bank project commitment in each period. Control variables are the same as in table 3. * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table B.2: WB aid and missions (Beach, 1903): Non-binary treatment and outcome

	Ever WB aid		Number of WB projects			
	OLS	OLS	OLS	OLS	Poisson	NB
No. of missions	0.05***					
	(0.01)					
Ln(No. of missions)		0.13***				
		(0.03)				
Mission dummy			1.39***	2.34***	0.83***	0.73***
			(0.33)	(0.75)	(0.11)	(0.07)
Ethnic dummies	Yes	Yes	Yes	Yes	No	No
Mean dep. var.	0.25	0.25	0.92	3.63	0.92	0.92
R-sq.	0.39	0.39	0.48	0.51		
N	6876	6876	6876	1737	6884	6884

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 200 km) in columns 1–4. Robust standard errors in column 5 and 6. In columns 1 and 2 the dependent variable is a dummy for at least one World Bank project commitment in 1995–2014. In columns 3 to 6 the dependent variable is the number of aid commitments. In column 4 the sample is restricted to cells with at least one project commitment. Control variables are the same as in table 3, but without ethnic dummies in columns 5 and 6. * p<0.1; ** p<0.05; *** p<0.01.

Table B.3: WB aid and missions (Beach, 1903): Present-day population controls

	All cells	All cells	Pop. place		Prov. capital	
	(1)	(2)	(3)	(4)	(5)	(6)
Mission	0.10***	0.07***	0.07	0.10**	0.18***	0.21***
	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)
Population (1995)	0.01***	0.01***				
	(0.00)	(0.00)				
Population^2	-0.80***	-0.52***				
	(0.11)	(0.10)				
Population^3	0.00***	0.00***				
	(0.00)	(0.00)				
Population^4	-0.00***	-0.00***				
	(0.00)	(0.00)				
Pop. place dummy	No	Yes	No	No	No	No
Ethnic dummies	Yes	Yes	Yes	No	Yes	No
Mean dep. var.	0.25	0.25	0.49	0.49	0.62	0.62
R-sq.	0.40	0.43	0.59	0.34	0.64	0.32
N	6876	6876	1168	1168	698	698

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 220 km). Estimation by ordinary least squares. The dependent variable is a dummy for at least one project commitment in sample period. Control variables are the same as in table 3. The estimation samples in columns 3–4 and 5–6 are restricted by the presence of a populated place with at least 10,000 inhabitants, and a provincial capital, respectively. * p<0.1; ** p<0.05; *** p<0.01.

Table B.4: WB aid and missions from Beach (1903) and Roome (1924). Specifications without controls.

	World Bank aid 1995–2014			
	(1)	(2)	(3)	(4)
Catholic mission (Roome)	0.46***		0.56***	
	(0.04)		(0.04)	
Protestant mission (Roome)	0.45***			
	(0.03)			
Protestant mission (Beach)		0.44***	0.36***	
		(0.04)	(0.04)	
Any mission				0.46***
				(0.03)
Controls	No	No	No	No
Ethnic dummies	No	No	No	No
Country dummies	No	No	No	No
Mean dep. var.	0.25	0.25	0.25	0.25
R-sq.	0.05	0.01	0.03	0.04
N	6892	6892	6892	6892

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 220 km). The dependent variable is a dummy for at least one project commitment in sample period. * p<0.1; ** p<0.05; *** p<0.01.

Table B.5: WB aid and missions (Beach, 1903): Spatial lags

	World Bank aid 1995–2014				
	(1)	(2)	(3)	(4)	(5)
Mission	0.11***	0.11***	0.14***	0.11***	0.09
	(0.03)	(0.03)	(0.05)	(0.03)	(0.06)
Mission Lag1		0.01	0.01	0.00	0.01
		(0.02)	(0.02)	(0.02)	(0.03)
Mission × Mission Lag1			-0.04		0.09
			(0.06)		(0.10)
Mission Lag2				-0.02	-0.01
				(0.02)	(0.02)
Mission Lag1 × Mission Lag2					-0.01
					(0.04)
Mission × Mission Lag2					0.10
					(0.09)
Mission × Mission Lag1 × Mission Lag2					-0.19
					(0.12)
Ethnic dummies	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.26	0.26	0.26	0.26	0.26
R-sq.	0.43	0.43	0.44	0.44	0.44
N	5840	5840	5840	5840	5840

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≈ 200 Km). The dependent variable is a dummy for at least one project commitment in sample period. Mission Lag 1 refers to the (up to) 8 neighbors adjacent to each cell. Mission Lag 2 refers to the next (up to) 16 closest outer neighbors. Control variables are the same as in table 3. * p<0.1; ** p<0.05; *** p<0.01.

Table B.6: Aid and missions from Beach (1903) and Roome (1924): specifications with ADM1 fixed effects.

	World Bank aid 1995–2014						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Catholic mission (Roome)	0.22^{***}		0.20^{***}			0.21^{***}	
	(0.04)		(0.04)			(0.04)	
Protestant mission (Roome)		0.13^{***}	0.11^{***}				
		(0.02)	(0.02)				
Any mission (Roome)				0.16^{***}			
				(0.02)			
Protestant mission (Beach)					0.10^{***}	0.09^{***}	
					(0.03)	(0.03)	
Any mission							0.14^{***}
							(0.02)
Ethnic dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ADM1 dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.25	0.25	0.25	0.25	0.25	0.25	0.25
Oster bound	0.11	0.07		0.09	0.10		0.10
R-sq.	0.39	0.40	0.40	0.40	0.39	0.40	0.40
N	6796	6796	6796	6796	6796	6796	6796

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 220 km). The dependent variable is a dummy for at least one project commitment in sample period. Control variables include log distance to coast, explorer route, colonial railway, and Arab trade route; 3rd order polynomial in 18th century population; presence of a city as of 1800; country dummies; pre-colonial ethnic dummies; average altitude; terrain ruggedness index; percentage area within 10 km from water source; caloric suitability index; malaria ecology; tropics dummy (also interacted with mean altitude). Cells that are more than 90 % covered by barren land or more than 90 % covered by forest are excluded from the sample. The lower bound on the coefficient of interest is calculated as in Oster (2019): we set the R-squared from the hypothetical regression on $Mission_{ik}$ and both observed and unobserved controls equal to 1.3 times the R-squared from the actual regression on $Mission_{ik}$ and observed control. * p<0.1; ** p<0.05; *** p<0.01.

Table B.7: Aid and missions from Beach (1903) and Roome (1924): specifications with ADM2 fixed effects.

	World Bank aid 1995–2014						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Catholic mission (Roome)	0.27***		0.25***			0.26***	
	(0.05)		(0.05)			(0.05)	
Protestant mission (Roome)		0.14***	0.11***				
		(0.03)	(0.03)				
Any mission (Roome)				0.17***			
				(0.03)			
Protestant mission (Beach)					0.09***	0.08***	
					(0.03)	(0.03)	
Any mission							0.15***
							(0.02)
Ethnic dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ADM2 dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Oster bound	0.11	0.07		0.09	0.10		0.10
Mean dep. var.	0.25	0.25	0.25	0.25	0.25	0.25	0.25
R-sq.	0.39	0.40	0.40	0.40	0.39	0.40	0.40
N	6796	6796	6796	6796	6796	6796	6796

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 220 km). The dependent variable is a dummy for at least one project commitment in sample period. Control variables include log distance to coast, explorer route, colonial railway, and Arab trade route; 3rd order polynomial in 18th century population; presence of a city as of 1800; country dummies; pre-colonial ethnic dummies; average altitude; terrain ruggedness index; percentage area within 10 km from water source; caloric suitability index; malaria ecology; tropics dummy (also interacted with mean altitude). Cells that are more than 90 % covered by barren land or more than 90 % covered by forest are excluded from the sample. The lower bound on the coefficient of interest is calculated as in Oster (2019): we set the R-squared from the hypothetical regression on $Mission_{ik}$ and both observed and unobserved controls equal to 1.3 times the R-squared from the actual regression on $Mission_{ik}$ and observed control. * p<0.1; ** p<0.05; *** p<0.01.

Table B.8: Aid by sector and missions from Beach (1903).

	Ever received World Bank with major sector:				
	(1) Agric.	(2) Publ.adm.	(3) Infrastr.	(4) Education	(5) Finance
Protestant mission (Dennis et al.)	0.093***	0.121***	0.033**	0.053***	0.040***
	(0.023)	(0.025)	(0.013)	(0.016)	(0.014)
Mean dep.var.	0.103	0.221	0.030	0.050	0.024
Adj. R-sq.	0.282	0.318	0.212	0.210	0.299
No. of observations	6876	6876	6876	6876	6876

	Ever received World Bank with major sector:				
	(1) Health	(2) Energy	(3) Transport	(4) Water	(5) Industry
Protestant mission (Dennis et al.)	0.081***	0.057***	0.105***	0.075***	0.049***
	(0.019)	(0.021)	(0.024)	(0.021)	(0.019)
Mean dep.var.	0.133	0.091	0.154	0.105	0.067
Adj. R-sq.	0.288	0.251	0.276	0.249	0.283
No. of observations	6876	6876	6876	6876	6876

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 220 km). The dependent variable is a dummy for at least one project commitment in sample period. Only project in the above mentioned sector are considered in each column. Control variables include log distance to coast, explorer route, colonial railway, and Arab trade route; 3rd order polynomial in 18th century population; presence of a city as of 1800; country dummies; pre-colonial ethnic dummies; average altitude; terrain ruggedness index; percentage area within 10 km from water source; caloric suitability index; malaria ecology; tropics dummy (also interacted with mean altitude). Cells that are more than 90 % covered by barren land or more than 90 % covered by forest are excluded from the sample. * p<0.1; ** p<0.05; *** p<0.01.

Table B.9: Aid by main sector and missions from Beach (1903).

	Ever received World Bank with main major sector:				
	(1) Agric.	(2) Publ.adm.	(3) Infrastr.	(4) Education	(5) Finance
Protestant mission (Dennis et al.)	0.052***	0.035**	0.017**	0.029**	0.029***
	(0.018)	(0.016)	(0.008)	(0.011)	(0.011)
Mean dep.var.	0.058	0.055	0.014	0.017	0.012
Adj. R-sq.	0.232	0.240	0.149	0.224	0.396
No. of observations	6876	6876	6876	6876	6876

	Ever received World Bank with main major sector:				
	(1) Health	(2) Energy	(3) Transport	(4) Water	(5) Industry
Protestant mission (Dennis et al.)	0.050***	0.059***	0.065***	0.059***	0.024*
	(0.016)	(0.020)	(0.021)	(0.019)	(0.013)
Mean dep.var.	0.059	0.070	0.129	0.059	0.017
Adj. R-sq.	0.249	0.245	0.263	0.202	0.142
No. of observations	6876	6876	6876	6876	6876

Notes: Conley (1999) standard errors in parentheses, with cutoff at 2 degrees (≊ 220 km). The dependent variable is a dummy for at least one project commitment in sample period. Only project in the above mentioned sector are considered in each column. Control variables include log distance to coast, explorer route, colonial railway, and Arab trade route; 3rd order polynomial in 18th century population; presence of a city as of 1800; country dummies; pre-colonial ethnic dummies; average altitude; terrain ruggedness index; percentage area within 10 km from water source; caloric suitability index; malaria ecology; tropics dummy (also interacted with mean altitude). Cells that are more than 90 % covered by barren land or more than 90 % covered by forest are excluded from the sample. * p<0.1; ** p<0.05; *** p<0.01.