Lighting the path: The influence of historical Christian missions on modern-day development aid allocation in Africa*

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Abstract

In this paper, we document a positive correlation between the location of historical Christian missions and the within-country allocation of World Bank financed development projects in Africa. The correlation is conditioned on observable geographical and historical factors that have shaped missionaries' settlement decisions. We do not find any evidence that this correlation is explained by superior aid effectiveness in mission areas, as we should expect in light of the literature showing that missionaries increased permanently human capital of indigenous population. On the contrary, we document the existence of a political aid cycle specific to these areas, which suggests political connections as one likely explanation.

Keywords: development aid; christian missions; historical path dependence; 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 the 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 fairness, but several empirical studies seem to suggest that it is instead 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 distribution of development aid (Dreher et al., 2016; Jablonski, 2014; Masaki, forthcoming).

We add to this literature by studying the role of history and path dependence in shaping the present-day within-country allocation of aid. Although development aid in its present form is a relatively recent 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 proselytism 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 empirical analysis, we compare a georeferenced snapshot of all 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 km 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,

[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.

because the first step of aid allocation is at the country level. Second, 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 ocean 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 (forthcoming) 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.

Our findings speak to the recent literature that has investigated the long-lasting effects of early 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, forthcoming; Mantovanelli, 2014; Meier zu Selhausen, 2014; Nunn, 2014; Okoye & Pongou, 2017; Waldinger, 2017), health (Cagé & Rueda, 2017; Calvi & Mantovanelli, 2016) and income (Caicedo, 2018; Chen, Wang, & Yan, 2014).[4] The finding that areas close to missions are more developed, but they also receive more aid might appear puzzling, but it is actually consistent with the evidence of a positive correlation between wealth and aid (Briggs, 2017; Nunnenkamp, Öhler, & Andrés, 2017). The literature on missions has also documented that the channel through which early missionary interventions translate into better present-day outcomes does not have to do with persistence of infrastructures (e.g. schools or hospital). On the contrary, it is explained by the transmission of new values, by the introduction of better practices, and by the increase of non-cognitive skills such as collaborative behavior. All these factors might constitute necessary conditions for the successful implementations of aid projects today, hence explaining our findings. If this is the case, it is reasonable to expect that aid projects implemented in the vicinity of historical missions are more effective in achieving their goals.

In the second part of the paper, we test this hypothesis using two empirical strategies. First, we compare outcomes of projects implemented in the vicinity of missions to those

[4] 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) documents positive effects of monasteries in medieval England.

further away, using the ratings by the World Banks's Independent Evaluation Group (IEG) as a proxy for project performance. We do not find any significant difference.

Second, we exploit information on start and end dates of the aid projects to implement a triple difference-in-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 education, wealth, and access to services. 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 do not find any evidence that mission presence matters for aid effectiveness.

Having found no evidence that mission areas display higher project performances, the last section of the paper tests whether favoritism of some sort is the mechanism behind the spatial correlation between aid and missions. First, we investigate the role of political favoritism. There is consistent evidence from Africa that World Bank and African Development Bank money has been diverted to politically crucial areas: either competitive electoral districts (Masaki, forthcoming), strongholds of the incumbent regime (Briggs, 2014; Jablonski, 2014), or birth regions of the presidents (Dreher et al., 2016). In light of the fact that areas close to missions are richer and more productive, it is reasonable to think that they are also more important from a political point of view. We put this hypothesis to an empirical test by estimating the existence of 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 the areas in the proximity of mission stations experience a 40 % drop in the probability of receiving a new aid project in case 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 people 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 larger share of Christian population. To probe this mechanism, we simply add the latter variable to our baseline specifications. 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 can not rule out that human capital plays a relevant role.[5]

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. Our empirical strategy exploits spatial variation in both variables across Africa. Since the location of missions is predetermined and does not vary over time, we collapse the temporal dimension of aid allocation to get a cross-sectional dataset. The unit of observation is contiguous grid cells at a resolution of 0.5×0.5 degrees, which at the equator roughly corresponds to 55×55 kilometers. We intersect the grid with the African continent, and keep the part of the grid covering continental Africa and Madagascar. 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 sub-national administrative levels from the GADM database of Global Administrative Areas (ADM1, corresponding to states or governatores; and ADM2 corresponding to districts, municipalities or communes), by matching the centroid of each cell with the ADM in which it falls. 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.

The baseline estimation exploits within-country variation only. The inclusion of country dummies δ_k controls for all time-invariant country-level characteristics, many of whom are important determinants of foreign aid.[6] We also implement robustness tests using dummies at the sub-national administrative level (ADM 1 or 2), instead of at the country-level.

[5] We stress that this strategy is problematic because involves controlling for a covariate that is not pre-determinated with respect to mission presence, so we take these findings as suggestive.

[6] 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).

The vector $\mathbf{X}_{ik}$ contains control variables at the grid 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 from the centroid of each cell. We use the standard error estimator developed by Conley (1999).[7]

Coefficient β has a causal interpretation 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. In the following, we discuss possible sources of bias, and how we deal with them.

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 today, β will be biased, or the correlation may be entirely spurious. We have quite 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, forthcoming 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 treated observations) are very different in their covariates compared to cells without a historical mission (the control observations), and if the control function is misspecified (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 experiment with restricting the sample to coastal cells and to those that intersect one of the main African rivers.[8] 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

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

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

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. This is the geocoded dataset 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 agents.[9]

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 geo-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 approximately 15% of the cases.[10] Precision 2 locations are similar to precision 1, but their reported coordinates are not as accurate, with coordinates being within 25km from the exact location. Precision 3, 4 and 6 locations correspond respectively to second-order (ADM2, i.e. district, municipality, or commune), first-order administrative divisions (ADM1, i.e. , states, or governorates), and countries. Note that precision 3, 4 and 6 categories do not refer to projects precisely located in one point, but for which it

[9]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.

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

was impossible to pinpoint precise coordinates; on the contrary, they refer to projects intended to serve the whole administrative division (e.g. a training for the all the public employees in a province). Precision 5 locations are imprecisely geo-coded, 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 our results to be driven by capitals.[11] This leaves us with 768 projects (40%) and 12,318 location-project coordinate pairs (74%).

Our sample restriction raises 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 location of different precision categories (see online appendix). It is comforting to see that more than 60% of the projects retained in our sample are also assigned to at least one location at the ADM1 (precision 4)

[11] We also drop South Sudan.

and/or ADM2 level (precision 3). Furthermore, we compare the projects in our sample to projects with at least one location at precision 3 or 4, but without any at precisions 1 or 2 in terms of observables 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 compositions. The projects in our sample have, on average, smaller shares in Agriculture, Health and Education, while larger ones 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.). We replicate the analysis using mission stations identified in *Ethnographic Survey of Africa: Showing the Tribes and Languages* by Roome (1924), and digitized by Nunn (2010). This source reports locations of both Protestant and Catholic foreign mission stations in Africa as of 1924. The results of virtually all the analyses are quantitatively similar, and qualitatively identical, using either one of these sources to construct $Mission_{ik}$.[12]

The reason to rely on two different sources is twofold. First, historical sources are likely to be subject to measurement error in the precise location of missions, and we cannot rule out that some stations are not reported. The two sources are independent from each other, which means that any non-random measurement error affecting one is not likely to affect the other as well. Second, both sources report locations at a specific point in time; however, new missions are likely to be founded afterwards, and some of the early stations abandoned. It is less likely that our results are driven by the location of missions in one specific year if we can replicate the analysis using sources recorded twenty years apart. Figure 1 shows the location of mission stations in the two sources, along with main rivers. The period between the two sources saw a massive expansion of missionary activity in Africa, due to the colonization of the continent. Therefore, the cell-level correlation between protestant missions from Beach (1903) and Roome (1924) is only 0.31. One specific reason for the low correlation is that missionaries started

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

penetrating the African inland after first settling on the coast. Hence, the 1924 data has a higher concentration further from the coast, which is evident in Figure 1.

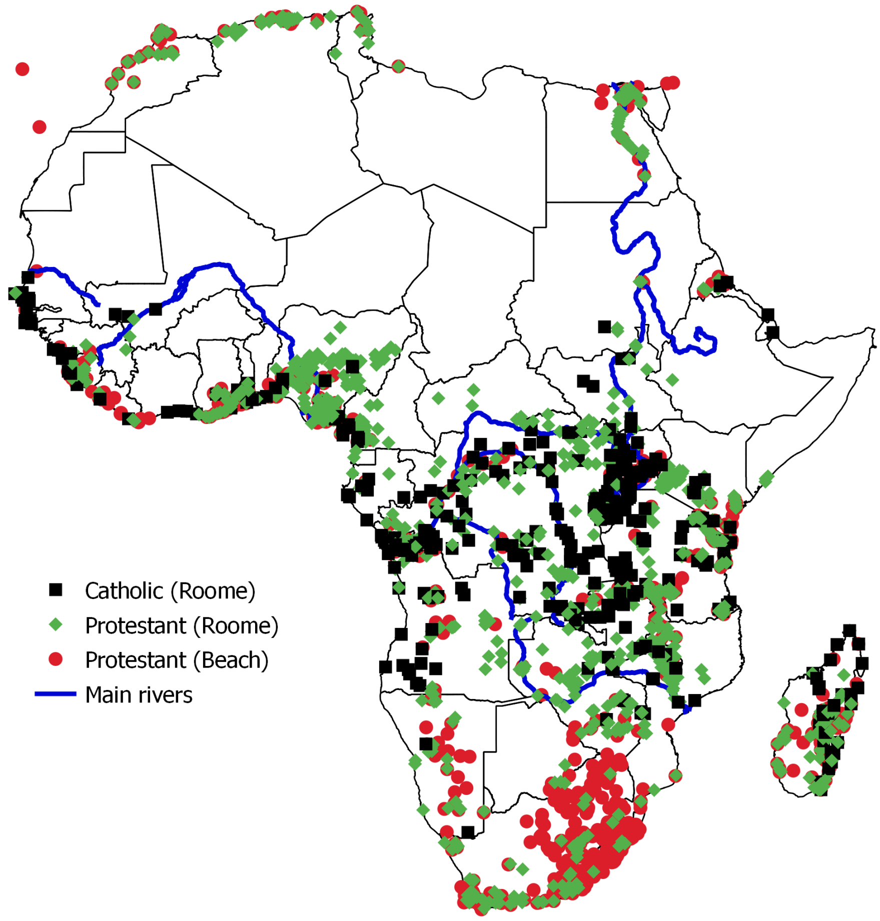

Figure 1: Location of mission stations and main rivers

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). If the factors that determine the selection of mission station location 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. As there is evidence that areas along coast and rivers have an advantage in development, and are more densely populated

(Gallup, Sachs, & Mellinger, 1999), we control for the (log) kilometer distance from the closest point on the coast and from the closest main river.[13] We also control for the (log) distance to the closest explorer route, and to the closest colonial railways, which had long-lasting effects on urbanization and growth in Africa (Jedwab, Kerby, & Moradi, 2017; Jedwab & Moradi, 2016). Finally, as a (inverse) measure of accessibility, we also include terrain ruggedness. Rugged landscape makes it easier to hide, which provided protection 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 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* from Kiszewski et al. (2004).

In addition, the existence of different ethnic groups may have played an important role in the missionaries' settlement decisions. As such, we are concerned that 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

[13] We use distances in logs of because the marginal effect of a unit of proximity is likely to approach zero as distance increases. More precisely, we use the log of 1 + the distance measured in kilometers, to avoid losing the cells at zero distance. The average distance is large, so the addition of 1 is analytically inconsequential.

at any time before 1800.

According to Robinson (1915), competition with Islam was a deterring factor, in that spreading the Gospel in predominantly Muslim areas was more complicated. Muslim populations may receive less development aid today for political and religious reasons, thus we control for the (log) distance from the closest Arab medieval trade route (which Michalopoulos, Naghavi, and Prarolo (forthcoming) show had strong impact on adherence to Islam).[14]

Table 2 present summary statistics of the controls for cells with and without missions.[15] More detailed information on data sources are available in the Appendix.

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: Mission data from Beach (1903). Each row represents an unconditional mean. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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. All regressions include the full set of historical and

[14]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). Although we are not aware of georeferenced measures of slave trade that are precise enough for our application, 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.

[15]The difference in means of distance to main river and to explorer route do not have the expected sign. Note, however, that these are unconditional correlations, and the distance variables are all correlated. The conditional correlations have the correct sign.

geographical controls described above. We 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 only data from both sources together.

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 ($\approxeq$ 200 km). The dependent variable is a dummy for at least one project commitment in sample period. Control variables: log distance to: coast, explorer route, colonial railway, 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 (forthcoming): 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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 (forthcoming) to get a lower bound for β. This test formalizes the common practice of inspecting the stability of the coefficient of interest after including controls. It is a refinement of Altonji, Elder, and Taber (2005), because it takes into account whether the additional controls are useful in absorbing residual variation. This is important for the credibility of the exercise, as we should not expect to induce coefficients instability when adding controls which are unrelated with the outcome variable. Under the assumption that selection on observables has the same direction than selection on unobservables, the test produces lower bound coefficients close to 0.1, corresponding to mission cells having a 40 % higher likelihood of aid allocation.[16] Importantly, the Oster bound is always significant at the 99% level.[17]

The estimated correlation is higher for Catholic missions, but the difference between confessions 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 the two confessions, 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). The reason is partly to simplify the exposition, and partly because the base map from which the missions were georeferenced is of much higher resolution than the one in Roome (1924), so there is likely less measurement error coming from inaccurate georeferencing.

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

[16] 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 controls, as suggested by Oster (forthcoming). The procedure is suited for models with only one treatment, so we do not calculate the bounds in column 3 and 6.

[17] Confidence intervals calculated using the standard errors from the regressions.

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.[18]

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 (≊ 200 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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

[18] 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.

at the 95% level, and sizable (35 % of the sample means).

Controlling for present-day population In all the regressions estimated so far, we have only included controls that are pre-determined with respect to the variable of interest, $Mission_{ik}$. This approach is the most appropriate to attempt a 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.[19] Furthermore, there is reason to believe that the presence of mission stations and the activities of missionaries has 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 in the control set different measures of present-day population. Under the assumption that population density is positively auto correlated, 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.[20] The results from these regressions are reported in Table 5. In the first column,

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 ($\approxeq$ 200 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

[19]In the most extreme scenario, we are just controlling for random variables.

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

we include fourth order polynomial terms in population in 1995. In the second, we add a set of dummies for the presence of populated places of different size.[21] 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 on WB aid survive all the three robustness tests: the 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 Missions are highly clustered in our data, as apparent from Figure 1. This leaves open the possibility that we are overestimating the effect of missions on aid. For two neighboring cells that both have missions, but where only one gets aid, it may be argued that both missions are contributing to attract aid. At the same time, disregarding the presence of missions in neighboring cells leaves open the possibility that we are underestimating the true effect, if cells without missions receive the benefit of having missions in a cell nearby. In both cases, failure to account for the presence of missions in surrounding cells may induce omitted variable bias.

To alleviate concerns of spillover bias from neighboring cells, we include spatial lags of missions in our baseline regression, and its interaction with the main treatment variable. 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).[22] The coefficients on the first spatial lag, and on its interaction with $Mission_{ik}$, are very close to zero and insignificant at conventional level. 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 (see online appendix). We take this as evidence that our estimated correlation holds mostly at the local level, with limited role for spatial spillovers.

[21] 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.

[22] 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 correlations 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.

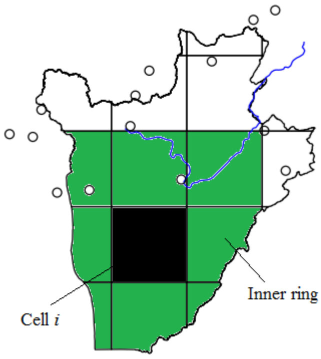

Figure 2: Example: spatial lags in Burundi

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 (≊ 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. Control variables are the same as in table 3. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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 over time, splitting the sample before/after the 2005 Paris Declaration.[23] Next, we check if our use of binary treatment and outcomes is important for the results. When using the

[23]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 (although casual observation suggests that it has not had much effect. See http://www.oecd.org/dac/effectiveness/parisdeclarationandaccraagendaforaction.htm

number of missions and the log number of missions + 1 as treatment variables, we obtain results very similar to the baseline. We 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/governatores) or ADM2 (districts, municipalities or communes) level.

Taking stock, this section has documented a very robust and sizable spatial relationship between World Bank aid and historical presence of mission stations. It is hard to claim causality only based on a selection on observables strategy, but the estimated relationship survives a vast array of robustness tests, including the most recent procedure suggested by the econometric literature to asses the bias from unobservable factors (Oster, forthcoming).

3 Implications for aid effectiveness

Having established that mission areas attract a disproportionate share of World Bank aid, a natural question is 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. It is possible that missionary interventions have paved the ground for aid interventions, by providing suitable conditions for effective project implementation. These conditions might include cooperative behavior, trust in foreigners, and specific skills (e.g. language).

This section aims at testing whether aid projects implemented in mission areas are more successful in achieving their goals. We do this with two different empirical strategies: one using data on project ratings; and one using survey data on development outcomes.

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 cell-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-cell structure, and we construct a project-level dataset (recall that each project is implemented across several locations). Our baseline specifications 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 stations (this distance roughly corresponds to the extent 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.[24] At the project level, we include: project length (in years), the natural log of total committed funds, a dummy for new vs. follow-up projects, a dummy for investment projects, sector dummies, share in largest sector, and the natural log of completion and preparation costs relative to total committed funds.

Results In Table 7 we present estimates from six different specifications of equation (2). The regression reported in column 1 does not include any controls, the second one include all controls expect cost variables which are not available for many projects, and the third one 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 very small across specifications, and standard errors at least three times larger. The sign is not consistent across specifications. All in all, we find no evidence that mission presence is correlated with better (or worse) project performance, as measured by the IEG ratings.

[24] Deniyer also includes CPIA ratings from the World Bank but we do not find these data for the period before 2005.

The lack of correlation is striking, but this test is purely descriptive because it fails to

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 (0.07)	0.02 (0.07)	-0.02 (0.09)	0.01 (0.07)	0.02 (0.07)	-0.01 (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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

control for non-random locations of projects to mission areas. 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; it is at least partly based on documentation produced by the team leader; although formally independent, it is materially conducted by present and future World Bank employees (Denizer et al., 2013).

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 strategy, and on direct measures of economic development. To identify the effects of World Bank aid we exploit the longitudinal dimension of the data: we add the temporal dimension to our gridded dataset to 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. Our baseline equation to be estimated by OLS has the form:

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

where i, k and t are indexes for cell, country and year, respectively. The outcome variable Y_{ikt} is a measure of economic development (e.g. electrification, education). $ActiveAid_{ikt}$ is a binary indicator for the presence of at least one active (that is between commitment and completion) project in year t. $EverAid_{ik}$ 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 λ_{kt} level are always included.

This specification amounts to a triple difference-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, and β whether the more so in cells that hosted a historical mission. In other words, the coefficient of interest β captures the differential effects of having an active aid projects in cells that hosted a mission, relative to cells that did not.

It is important to stress that here we are mainly interested in testing whether mission cells cause higher/lower aid effectiveness, and not to test whether aid is effective per se. 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 does not require any assumption about parallel trends between cells which 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 say that the timing of aid implementation is random. On the contrary, our assumption is much less demanding, and it simply 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 our strategy, we need a measure of economic development at the cell-level observed at several points in time. We construct these measures from individual-level georeferenced data from the Demographic and Health Survey (DHS); in particular 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 six questions which cover different dimensions of development and are asked consistently across countries and time. We collapse answers at the cell level to obtain six variables measuring the fraction of cell respondents with certain characteristics. The characteristics are the following: fraction with secondary education or higher; fraction with piped water as main source of drinking water;[25] fraction with electricity; fraction who owns a television and/or a radio; fraction who owns a motor vehicle; fraction with floor made of modern material.[26] The main weakness of our measures is that they are not necessarily representative at the cell-year level. However, the number of respondents is high (mean 190 respondents, median 105), and the geograph-

[25]Other sources are worse: well water, surface water, rainwater.
[26]Non modern floors are for example leaves, sand, etc.

ical and temporal coverage as well (1900 unique cells, 4200 cell-year observations, and on average 200 cells per year).[27]

Results In Table 8 we present estimates from four different specifications of equation (3) for each Y_{ikt}. For each outcome variable, the first regression serves as benchmark because it corresponds to the simple difference-in-differences without interaction with the mission dummy. Across different outcomes, the coefficient on $ActiveAid_{ikt}$ is positive and significant at least at the 90% level, which means that when a World Bank project is active, the outcome is between 30% (for electricity and piped water) and 7% (for motor vehicles) higher than in other periods. These estimates have a causal interpretation under the assumption that the timing of aid arrival is unrelated with trends in outcome. If this assumption is not met, the estimates conflates together the effect of aid with the selection bias (e.g. cells growing faster able to attract more aid). Difference in outcomes between cells which ever or never had aid have a similar magnitude and precision. The sign of the coefficient on both variables is consistent with the literature, which finds that aid does not go to the least developed areas. We take comfort from these estimates because they suggest that our outcome variables are indeed decent proxies of development at the cell-year level.

The second column in each panel correspond to the triple difference-in-differences because they also include the mission dummy, and its interaction with the aid indicators. The coefficient of interest on the interaction between $ActiveAid_{ikt}$ and $Mission_{ik}$ is never significantly different from zero, and the sign is not consistent across different outcomes. In other words, we do not find any evidence that aid is more effective when implemented in areas in the vicinity of mission stations.

It might be that the effect of development aid materializes only or partly after the intervention is concluded. If this is the case, or if the effects of aid are long lasting, it is more appropriate to not include in the control group those cells that have received aid in the past. To deal with this issue, we include in the third and fourth columns an indicator equal to one for cells currently without aid, but which received at least one project in the past. We also include its interaction with the mission indicator. In these specifications, the coefficient on the interaction between $ActiveAid_{ikt}$ and $Mission_{ik}$ is interpreted as relative to the pre-aid period only. Nevertheless, estimates from these more robust specifications are very similar to the baseline results. All together, we fail to find any evidence pointing at a role of historical missions in improving aid effectiveness.

[27] We drop cell-year observations with less than 50 respondents.

Table 8: Triple difference-in-difference: aid effectiveness and missions Beach, 1903.

	Secondary education				Electricity in dwelling				Piped water in dwelling			
Active aid	0.05***	0.04***	0.06***	0.05***	0.09***	0.09***	0.13***	0.12***	0.10***	0.09***	0.11***	0.10***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Ever had aid	0.06***	0.06***	0.06***	0.05***	0.09***	0.08***	0.06***	0.05**	0.07***	0.07***	0.06**	0.05***
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Mission		0.09***		0.09***		0.11*		0.11*		0.11*		0.11*
		(0.03)		(0.03)		(0.06)		(0.06)		(0.06)		(0.06)
Mission × Active aid		0.02		-0.01		-0.04		-0.03		0.03		0.01
		(0.02)		(0.03)		(0.04)		(0.05)		(0.04)		(0.05)
Mission × Ever had aid		0.01		0.04		0.06		0.04		-0.00		0.02
		(0.02)		(0.03)		(0.05)		(0.06)		(0.05)		(0.07)
Aid was active			0.01	0.02			0.07***	0.07**			0.02	0.03*
			(0.02)	(0.01)			(0.02)	(0.02)			(0.02)	(0.02)
Mission × aid was active				-0.07				0.02				-0.04
				(0.05)				(0.06)				(0.06)
Mean outcome	0.25	0.25	0.25	0.25	0.30	0.30	0.30	0.30	0.29	0.29	0.29	0.29
Cells	1998	1998	1998	1998	1970	1970	1970	1970	1971	1971	1971	1971
Observations	4280	4280	4280	4280	4202	4202	4202	4202	4201	4201	4201	4201

	Radio/TV owners				Car/motorbike owners				Proper floor in dwelling			
Active aid	0.05***	0.05***	0.06***	0.06***	0.02**	0.01	0.02*	0.02	0.10***	0.08***	0.11***	0.10***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Ever had aid	0.05***	0.05***	0.04***	0.04***	0.01	0.01	0.01	0.01	0.09***	0.09***	0.08***	0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Mission		0.09***		0.09***		0.01		0.01		0.08**		0.08**
		(0.03)		(0.03)		(0.01)		(0.01)		(0.03)		(0.03)
Mission × Active aid		0.02		0.02		0.02		0.01		0.05		0.02
		(0.02)		(0.02)		(0.01)		(0.02)		(0.04)		(0.04)
Mission × Ever had aid		-0.05**		-0.06**		-0.02		-0.01		0.03		0.06**
		(0.02)		(0.03)		(0.01)		(0.02)		(0.03)		(0.03)
Aid was active			0.02	0.02			0.00	0.01			0.03	0.04*
			(0.01)	(0.01)			(0.01)	(0.01)			(0.02)	(0.02)
Mission × aid was active				0.00				-0.01				-0.07*
				(0.03)				(0.02)				(0.04)
Mean outcome	0.69	0.69	0.69	0.69	0.18	0.18	0.18	0.18	0.44	0.44	0.44	0.44
Cells	1970	1970	1970	1970	1966	1966	1966	1966	1971	1971	1971	1971
Observations	4201	4201	4201	4201	4156	4156	4156	4156	4202	4202	4202	4202

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

4 Potential mechanisms

In the previous sections, we have established that areas close to historical missions tend to attract more World Bank aid. However, we did not find any evidence that this has implications for aid effectiveness, which suggests that efficiency considerations are likely not the underlying reason for the correlation. If not for efficiency reasons, what other factors might be responsible for the spatial pattern that we observe in Africa? Here we provide some suggestive answers to this question.

First, we test whether the observed pattern holds for aid to all kind of 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, suggesting that the mechanisms at play are not sector specific.

Next, we specifically investigate the role of a few factors, which we consider potential mechanisms in light of the literature on development aid allocation, and on historical missions. There is widespread evidence that aid targeting depends on political considerations. Regions with special ties with the political 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: we test for the existence of a political aid cycle in mission areas.

Next, we investigate the role of a different sort of favoritism. Western and predominantly Christian countries stand out as the largest shareholders of the World Bank.[28] If shareholders for some reason prefer to allocate more aid to areas that are culturally

[28]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/.

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 respects, 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 because it is arguably the single factor for which we have the largest body of convincing causal evidence.

4.1 Political aid cycle

Empirical strategy and data 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 we estimate by OLS the following equation:

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

where i, k and t are indexes 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. The variables $Election_{kt}$ and $Turnover_{kt}$ are indicators equal to one if there is a national election for the office of head of state, and if it results in a turnover in the person holding office, respectively. The data is from the Varieties of Democracy (V-DEM) tdataset, version 7.1 (Coppedge et al., 2017). Fixed effects at the country-year λ_{kt} and at the cell μ_i level are included.

Note that the inclusion of fixed effects at the country-year level does not allow for identifying a political aid cycle, which is outside the scope of our paper, but it allows to 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 contemporaneous presence of cell fixed effects gives a difference-in-differences interpretation of the parameters of interests β and γ. The inclusion of cell fixed effect is possible due to the within cell variation in political events over time.

Findings Table 9 reports estimates of several specifications 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 election years which result in a change in the person holding the office of head of state. The same coefficient is again negative and significant in column 3, where we include both interactions together. In this case, the coefficient on $Election_{kt} \cdot Mission_{ik}$ gets larger but it is still insignificant, that is we do not find evidence of any drop or increase in those election years which result in a victory of the incumbent head of state.

Our strategy controls for annual shocks at the country level. However, 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 historical and geographical correlates of missions (see section 2) 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.

The cyclical pattern exhibited by aid to mission areas suggests that political ties to the national 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). The political connection favors these areas 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 those election years without turnover might constitute the reward by the incumbent in exchange for political support.

The findings in Table 9 can not be explained by a general slowdown of aid motivated by the fact that government officials are busy with the electoral campaign, due to the presence of country-year fixed effects. One alternative explanation would be that turnover happens between heads of state who belong to different religions. However, this interpretation is feasible only if all (or most) turnovers are from a Christian to a non-Christian head of states. A qualitative check of this explanation using multiple sources (Encyclopedia Britannica, Wikipedia) suggests that most turnovers happen between individuals of the same religion.

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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

4.2 The role of Christian religion and of human capital

Empirical strategy and data Testing for the role of religion and education in explaining the correlation between aid and historical missions is not an easy econometric task. The simple inclusion of proxies for the candidate mechanisms in the baseline equation 1 is problematic, because these variables (e.g. education) are not pre-determined with respect to $Mission_{ik}$. In lack of a quasi-experimental strategy like the one in the previous section, we proceed with caution in this way. 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 $EverAid_{ik}$.[29] 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 our 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

[29] This is the same logic used when controlling for present-day population.

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. We drop cells with less than 50 respondents, and we obtain a sample of approximately 2,200 cells, with more than 400 respondents on average (median 185). The DHS also surveys respondents about their religion beliefs, thus we construct the fraction of respondents of Christian religion (any confession). The geographical coverage is only slightly smaller than for education. Consistent with the literature discussed in section 1, we assume that historical missions had positive effects on all our DHS variables.

Results The first column of Table 10 reports estimates from the baseline equation (1) estimated in 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.

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 these different measures of educations are positive, significant at the 99% level, and large. When a proxy for education is included in the regression, the coefficient on $Mission_{ik}$ becomes smaller, and even insignificant in column 4. Under the assumption that missionary activities have positive effects on education, the coefficient on mission is a lower bound on the true one. Therefore we can not exclude that a heritage of high human capital might be one mechanism behind the spatial correlation between historical missions and present-day aid.

In column 5, we again replicate the baseline specification for the subsample of cells for which we have a DHS measure of religion. In this case, the coefficient on $Mission_{ik}$ does not change when a measure of Christian religion is included in the regression. This finding suggests that Christian favoritism is not the mechanism behind the correlation uncovered in Section 2.

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 (≊ 200 km). * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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.

However, the presence of mission stations does not appear to increase the effectiveness of development projects, as we should expect in light of the literature showing that missionaries activities had long-lasting effects on characteristics that facilitate aid implementation (e.g. cognitive and non-cognitive skills). This implies that higher levels of development is not a likely explanation behind the correlation between mission stations and aid allocation.

On the contrary, we find evidence of a political aid cycle specific to mission areas: aid arrival is less likely in years of presidential turnovers. We interpret this finding as evidence that mission areas are better connected with the central political power, and thus receive more aid. Electoral turnover (temporarily) breaks these ties, whereas incumbent reelection seems to strengthen them. However, we can not rule out the possibility that also other mechanisms play a role, most notably via human capital.

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ONLINE APPENDIX
NOT FOR PUBLICATION

Appendix A Data

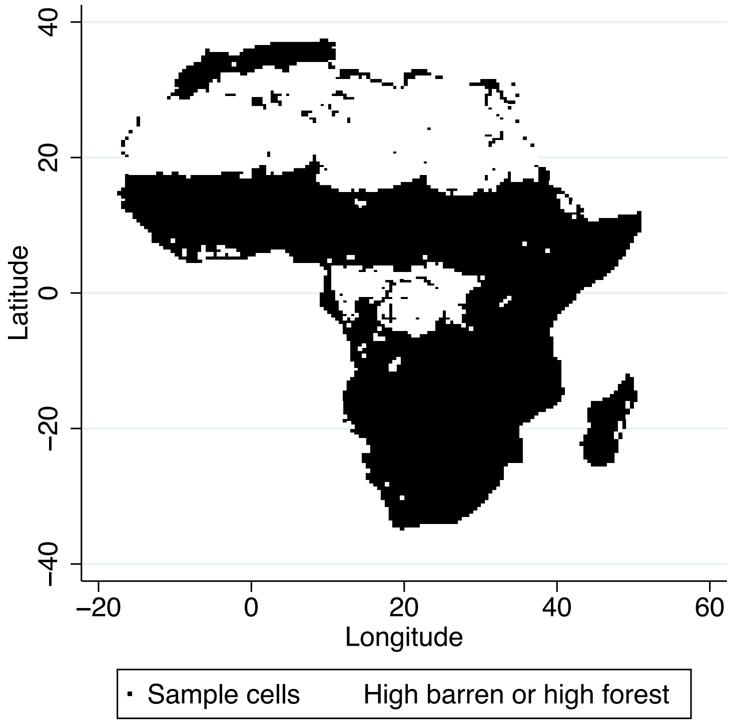

Figure A.1: Gridded sample. Blank spaces are cells covered by barren land or by forest for more than 90% of their surface in the XIX century (HYDE data).

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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

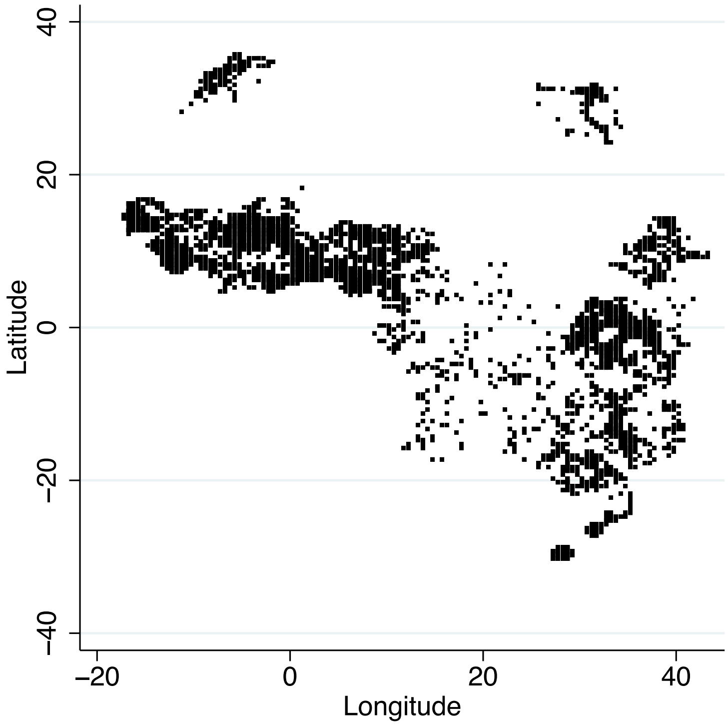

Figure A.2: DHS samples: black cells are those included in the analysis that rely on DHS data.

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 ($\approxeq$ 200 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 (≊ 200 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 ($\approx$ 200 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 (≊ 200 km). The dependent variable is a dummy for at least one project commitment in sample period. Control variables: log distance to: coast, explorer route, colonial railway, 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 (forthcoming): 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 ($\approxeq$ 200 km). The dependent variable is a dummy for at least one project commitment in sample period. Control variables: log distance to: coast, explorer route, colonial railway, 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 (forthcoming): 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)	(2)	(3)	(4)	(5)
	Agric.	Publ.adm.	Infrastr.	Education	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)	(2)	(3)	(4)	(5)
	Health	Energy	Transport	Water	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 (≊ 200 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: log distance to: coast, explorer route, colonial railway, 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 ($\approxeq$ 200 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: log distance to: coast, explorer route, colonial railway, 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.