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Abstract

Banks enter locations where local demand and infrastructure make financial intermediation profitable. We argue that Christian missions affected both margins, and thus the present-day geography of formal banking in Africa reflects the historical footprint left by Christian missionary expansion. We test this claim by matching 2,278 historical mission stations to about 26,600 enumeration area (EA)-round observations across Africa from Afrobarometer surveys. Comparing otherwise similar EAs within and outside a 25 km radius, we find that those within 25 km of a historical mission are about 10 percentage points more likely to host a bank facility today. The result survives extensive historical and geographic controls, tests for selection on unobservables, and the exclusion of EAs near pre-colonial urban centres. We find evidence consistent with both demand- and supply-side mechanisms: Mission proximity predicts higher educational attainment and household assets, lower reliance on subsistence agriculture, and greater local infrastructure, including post offices – the institutional precursor to formal banking. We find little evidence of Protestant superiority; both denominations appear to have generated lasting effects. Our findings suggest that Africa's present-day geography of formal financial access reflects centuries-old institutional footprints of European Christian missions.

Keywords: Christian missions, financial inclusion, banking, Africa, colonial history

1. Introduction

Efforts to explain and reduce gaps in financial inclusion have largely focused on the demand for financial services. Differences in account ownership are typically attributed to present-day characteristics of individuals and firms – income, education, gender, digital technology, and the regulatory environment (Demirgüç-Kunt, Klapper, Singer, Ansar, & Hess, 2020). Implicit in this approach is the assumption that the supply of formal financial infrastructure is exogenous: Banks are already there, and the challenge is to increase households' ability or willingness to use them. Yet this assumption is difficult to reconcile with the stark spatial inequality in bank presence across much of Africa, where many communities remain physically distant from any formal financial institution.

We argue that this geography is itself a historical equilibrium. Banks do not locate wherever demand exists. Because operating a branch requires substantial fixed investments, financial institutions enter only where local conditions make intermediation profitable. These extend beyond market size to include complementary institutional and socioeconomic conditions – human capital, administrative capacity, market integration, and trust – that lower transaction costs and support exchange (North, 1990; Levine, 2005; Guiso, Sapienza, & Zingales, 2004). In colonial Africa, Christian missions were among the first organisations to invest persistently in these local complements. Through schools, clinics, training, and connections to colonial administrative and commercial networks, missions transformed isolated settlements into places better suited for formal economic activity (Nunn, 2010; Frankema, 2012; Cagé & Rueda, 2016). In doing so, they plausibly affected both sides of the financial market: They increased the demand for financial services by fostering literacy and participation in the cash economy while simultaneously reducing the costs of supplying those services by creating the institutional environment in which banks could operate. If these local complementarities persist – as a large literature on historical persistence suggests (Acemoglu, Johnson, & Robinson, 2001; Nunn, 2014) – then present-day bank locations should continue to reflect the historical geography of missionary expansion. We show that they do.

We geo-match 2,278 historical Catholic and Protestant mission stations to 26,606 enumeration area (EA)-round observations surveyed by Afrobarometer across 42 African countries between 2014 and 2023. Villages[1] within 25 km of a historical mission are approximately 10 percentage points more likely to host a bank today than otherwise comparable villages farther away – more than a quarter of the sample mean. This relationship survives an extensive set of historical and geographic controls constructed specifically to absorb the observable determinants of non-random mission placement – log distances to colonial railways, pre-colonial explorer routes, and historical cities in AD 1400 and AD 1800, terrain ruggedness, malaria suitability, and precipitation – as well as country and survey-round fixed effects.

We subject this result to several checks designed to address the central threat to causal interpretation: that missions, like banks, were placed in locations already favourable to development (Jedwab, Meier zu Selhausen, & Moradi, 2022). First, the estimated effect is positive and statistically significant at every distance threshold from 10 km to 40 km for both Protestant and Catholic missions, indicating that the finding is not an artefact of a particular threshold choice. Second, using the coefficient-stability approach of Oster (2019), we show that unobserved selection would need to be at least 3.9 times as important as the observed selection we already control for to explain the result away. Third, the coefficient is unchanged when we drop all villages within 50 km of a historical city recorded in AD 1400 or AD 1800, directly addressing the concern that missions and banks simply co-located near pre-existing centres of development.

We then distinguish a demand-side channel – missions raising individual capacity and need for formal financial services – from a supply-side channel specific to our setting: missions

[1] For readability, we also refer to EAs as "villages," but the empirical unit is always the EA-round, and some EAs are urban.

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supplying the local preconditions banks required to operate profitably at all. The two channels find distinct empirical support. On the demand side, mission proximity predicts higher educational attainment, higher asset ownership, lower reliance on subsistence agriculture, and lower lived poverty, consistent with the human-capital mechanism long documented in this literature (Michalopoulos & Papaioannou, 2020; Nunn, 2010). On the supply side, mission proximity predicts a denser network of local infrastructure, including schools, markets, and – of particular interest given our setting – post offices. For most of the 20th century, the post office was the primary point of formal financial contact for ordinary Africans: Colonial administrations grafted post office savings banks onto existing postal networks from the 1880s onward (World Bank, 2006), but the upgrade from postal counter to functioning savings bank required literate staff and a monetised, trusting clientele that colonial administrations often struggled to establish (Velasco, 2023). Mission stations plausibly supplied exactly this population. Our finding that mission proximity predicts post-office presence is consistent with this specific historical channel, though a formal mediation analysis – estimating what share of the banking effect runs through each intermediate outcome – would require instruments for each mechanism and is beyond the scope of this paper.

Examining results separately by denomination, we find that both Catholic and Protestant missions are associated with economically similar and statistically indistinguishable improvements in bank presence across all distance thresholds. This null result complements Becker and Meier zu Selhausen (2025), who find no systematic differences between Protestant and Catholic missions in long-run human-capital outcomes using a comprehensive mission census. Our findings, on a different outcome and a more complete mapping of mission locations, point in the same direction: Whatever organisational differences distinguished Catholic and Protestant missionary strategies in Africa, they do not translate into detectably different footprints in the present-day geography of formal finance.

The paper makes two substantive contributions beyond its direct findings. First, the missions literature has documented that mission proximity predicts higher educational attainment, literacy, and social capital (Michalopoulos & Papaioannou, 2020; Nunn, 2010; Wantchekon, Klašnja, & Novta, 2015; Okoye, 2021; Fors, Isaksson, & Lindskog, 2024), emphasising almost uniformly a demand-side channel in which missions changed people and that change persisted. The closest exception is Alpino and Hammersmark (2021), who show that mission locations predict the allocation of present-day World Bank aid projects and interpret political connections as a likely explanation. We extend this literature to an outcome that is not an individual-level legacy – bank-facility location is a firm's decision, contingent on the profitability of a place – and provide evidence consistent with a supply-side channel operating alongside the standard demand-side one.

Second, the literature on the historical origins of African financial development has traced the persistence of colonial-era financial infrastructure (World Bank, 2006; Velasco, 2023). We push the origin point further back, into the missionary period that in many places predates formal colonial administration, and identify a specific historical mechanism – the institutional upgrade from postal network to savings bank – through which that persistence plausibly operated.

These findings carry a direct implication for financial-inclusion policy. Present-day access to banking reflects not only current income, regulation, or technology but also historically inherited variation in local institutional capacity laid down by missionary activity decades before the financial institutions we study existed. Whether digital finance, mobile money, or regulatory reform can durably close inclusion gaps may therefore depend on whether they can substitute for, or build on, precisely the local complements – literate populations, monetised exchange, administrative presence – that missions historically provided and that many underserved communities still lack.

The rest of the paper is organised as follows. Section 2 presents the theoretical framework. Section 3 describes the data and construction of key variables. Section 4 presents the empirical strategy. Section 5 presents the main results. Section 6 concludes.

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2. Theoretical arguments

A bank opens a branch when the expected return from serving a local market exceeds the fixed and variable costs of operating there. Expected revenues increase with the size of the potential customer base and its demand for formal financial services, while operating costs depend on banks' ability to screen and monitor clients, enforce contracts, hire qualified workers, and conduct transactions (Levine, 2005; Rajan & Zingales, 1998; North, 1990). Branch entry should therefore be more likely where economic activity generates sufficient demand and where local human capital and infrastructure reduce the cost of financial intermediation.

Two sets of local conditions shape this profitability calculus. On the demand side, branch viability requires a sufficiently large pool of households and firms that are both willing and able to use formal financial services. This is more likely where incomes extend beyond subsistence, economic activity is conducted through monetary rather than predominantly in-kind exchange, and individuals possess the literacy and administrative skills needed to navigate formal financial contracts. On the supply side, profitability depends on the costs of delivering those services. Banks benefit from access to educated workers, reliable systems of record-keeping and identification, greater trust in formal institutions, and pre-existing administrative and commercial infrastructure that lowers the fixed and recurrent costs of local operations (Guiso et al., 2004; Burgess & Pande, 2005). Where these complementary conditions are weak, formal financial institutions may find branch expansion unprofitable even when households face substantial unmet demand for credit, savings, or payment services (Dupas, Green, Keats, & Robinson, 2016; Allen et al., 2014).

Christian missions may have affected both margins. Mission schools expanded literacy and numeracy, while missionary activity contributed to wage employment, monetised exchange, and the development of local administrative and commercial networks (Nunn, 2010; Valencia Caicedo, 2019; Fourie & von Fintel, 2014; Wantchekon et al., 2015; Cagé & Rueda, 2016; Gallego & Woodberry, 2010; Woodberry, 2004; Frankema, 2012). If these effects persisted (Acemoglu et al., 2001; Nunn, 2014; Dell, 2010), mission-exposed locations should have developed both a larger potential market for formal finance and lower costs of supplying it. We therefore expect present-day bank presence to be higher near historical mission stations, even long after missions ceased to play their original institutional role.

From the late 19th century onward, colonial administrations increasingly used existing postal networks to extend savings services into areas beyond the reach of conventional banks, exploiting the geographic coverage of post offices to provide basic financial services (Madimu & Msindo, 2019). Yet the existence of a postal outlet alone did not guarantee the viability of postal banking: Its operation also required literate personnel and a clientele sufficiently familiar with monetary transactions, record-keeping, and formal institutions (Velasco, 2023). These were some of the capabilities that missionary activity may have helped foster. Historical mission proximity should therefore be associated not only with present-day bank presence, but also with the persistence of local institutional infrastructure – such as post offices – through which formal financial services historically expanded into rural areas.

Mission placement was not random. Missionaries selected locations partly on the basis of accessibility, population concentration, political organisation, and other characteristics that may also predict subsequent financial development (Jedwab et al., 2022; Nunn, 2010). We therefore do not interpret the cross-sectional association between mission proximity and bank presence as causal. Our empirical specifications control for observable historical and geographic correlates of mission placement, and we assess sensitivity to remaining unobserved selection using the procedure of Oster (2019).

The data also do not separately identify the proposed demand- and supply-side mechanisms. Education, household assets, occupational structure, local infrastructure, and bank presence are observed at the time of the surveys. We therefore use these outcomes to

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assess whether the cross-sectional patterns are consistent with the proposed mechanisms, rather than to estimate a causal mediation effect.

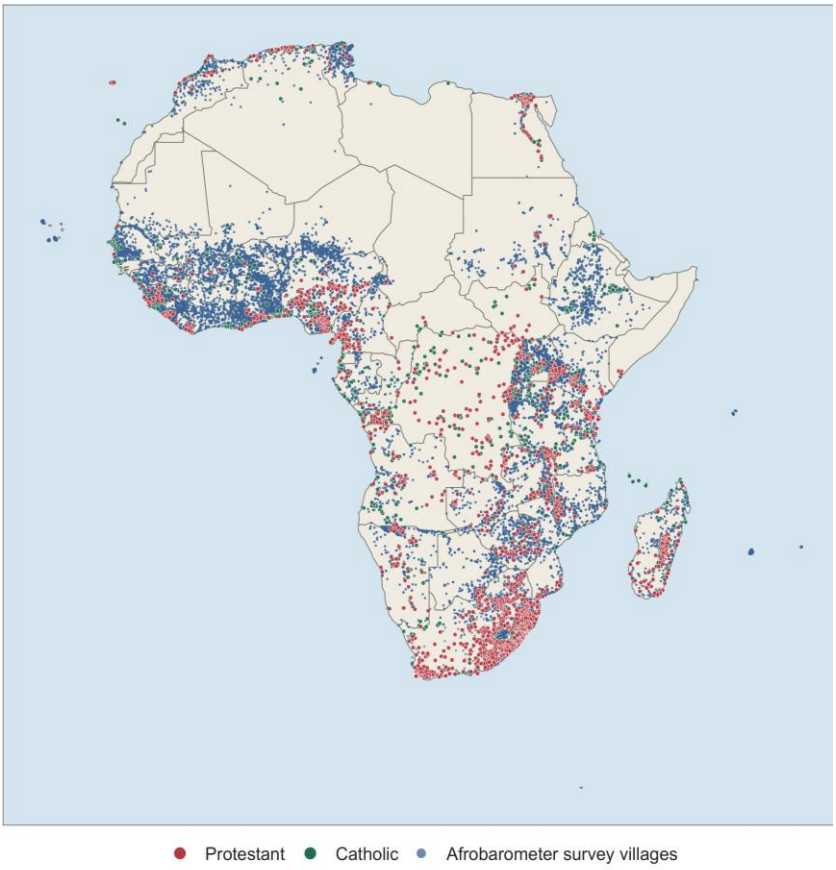
3. Data

We construct the data set from several sources, described in turn below.

Mission data

Our key explanatory variable requires geo-referenced locations of historical Christian mission stations across colonial Africa. The standard source in this literature is Roome's 1924 *Ethnographic Survey of Africa Showing the Tribes and Languages*, as digitised by Nunn (2010). We instead draw on the more recent and more comprehensive geospatial data set of Catholic and Protestant mission stations assembled by Hedde-von Westernhagen and Becker (2022), who document that existing sources – including Roome's atlas – substantially undercount historical mission locations, and who provide a correspondingly more complete geo-referenced data set for use in sub-national studies of colonial legacies. In total, the Hedde-von Westernhagen and Becker (2022) data record 2,278 mission stations across colonial Africa in the mid-1920s, of which 756 are Catholic and 1,522 are Protestant. Figure 1 maps their spatial distribution across our sample of African villages.

Figure 1: Distribution of Christian mission stations in Africa



Christian mission stations digitised by Hedde-von Westernhagen and Becker (2022) are indicated by red (Protestant) and green (Catholic) points. Small blue points show the locations of Afrobarometer survey EAs.

Survey data

Our key measure of bank location is from the Afrobarometer survey, a well-established series of public opinion surveys conducted across multiple African countries since 1999. Afrobarometer employs a clustered, stratified, multi-stage, area probability sampling design. The sampling universe includes all citizens aged 18 years or older. Clustering divides the population into smaller geographic units, allowing for efficient fieldwork management while

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maintaining broad representation; stratification further divides the population into subgroups based on characteristics such as region and urban-rural classification. The survey then proceeds through a multi-stage selection process, moving from larger to progressively smaller geographic units before reaching individual households and respondents. Area probability sampling ensures that every eligible individual has a known, non-zero probability of selection, producing estimates that are representative of the national population in each surveyed country.

Afrobarometer has completed 10 survey rounds since 1999 (Afrobarometer, 2025). Data on local bank presence are available from Round 6 onward. We use rounds 6-9, covering fieldwork conducted between 2014 and 2023; the geocoded data provided to us by Afrobarometer cover these four rounds, so Round 10 is not included. The unit of observation is the EA-round, where the EA is Afrobarometer's primary sampling unit. We identify EAs by the GPS coordinates recorded at the time of fieldwork, aggregate individual responses within each EA, and measure distances from each EA's coordinates to mission stations.[2] The pooled sample covers 42 African countries, although participation varies by round: 36 countries in Round 6, 34 in Round 7, 32 in Round 8, and 39 in Round 9.[3] Appendix Table A.3 reports the full breakdown by country and round. The pooled data set contains 26,781 EA-round observations, of which 26,606 have non-missing information on bank presence.

Local bank presence

Geocoded data on the spatial distribution of banking facilities are generally unavailable at fine spatial scales across Africa. Afrobarometer field enumerators record a standard set of observable characteristics once for each EA, including whether a bank is present in the EA or within easy walking distance (item EA-FAC-F). This is a community-level observation, not a response collected from each sampled individual. The wording of the item changes during our sample period. In rounds 6 and 7, it refers to a "bank"; from Round 8 onward, it was broadened to "bank, money-transfer point, mobile banking services, or ATM." We use the item, under the wording that applied in each round, to construct *Banked*, an indicator equal to 1 if EA v is recorded as having the facility in survey Round t and 0 otherwise. For brevity, we refer to the facilities covered by either wording as bank facilities and to the outcome as bank presence; survey-round fixed effects absorb the resulting level shift, and Section 5.4 reports results separately for the two wordings.

Present-day controls

We include cluster-level demographic characteristics constructed from individual responses: mean respondent age, mean age squared, and the share of female respondents. These are measured at the EA-round level and vary across waves, absorbing compositional differences in the respondent pool. We exclude individual-level outcomes that could plausibly reflect mission influence – such as education or employment – to avoid bad controls in the sense of Angrist and Pischke (2009); the main results are robust to omitting these controls entirely (see Table 1, columns 1 and 2).

[2] In rounds 8 and 9, these coordinate-based clusters coincide with Afrobarometer's EA identifiers. Rounds 6 and 7 carry no EA identifier in our data, and in some country-rounds the number of distinct coordinate points departs from the number of sampled EAs. Several Round 6 surveys are geocoded at a coarser level, so that one point groups several EAs; in that case the EA-level observations, including bank presence, are taken from one of the grouped EAs. Conversely, in Round 7 the coordinates for Ghana, Nigeria, and Zambia are recorded at a finer level than the EA, yielding several points per EA. Section 5.4 shows that the main result holds when the affected country-rounds are excluded or reweighted, and when the analysis is restricted to rounds 8-9.

[3] Algeria, Burundi, and Egypt appear only in Round 6, whereas Congo-Brazzaville, Mauritania, and Seychelles appear only in Round 9.

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Historical and geographic controls

We control for pre-existing differences in accessibility and historical settlement that predict both mission placement and long-run development. Historical controls are the log distances to the nearest colonial railway line (Jedwab et al., 2022; Nunn & Wantchekon, 2011), the nearest 19th-century European explorer route (Nunn & Wantchekon, 2011), and the nearest city with a recorded population in AD 1400 and AD 1800 (Chandler, 1987). Geographic controls include elevation, terrain ruggedness,[4] annual precipitation from WorldClim 2.1 (Fick & Hijmans, 2017), and the *plasmodium falciparum* temperature suitability index (Gething et al., 2011) – a climate-driven proxy for disease environment that is determined by geography and unaffected by economic activity. Table A.1 in the Appendix reports descriptive statistics for all variables. Among the 26,781 EA-round cells in the full sample, 43% lie within 25 km of at least one mission station – 33% near a Protestant station and 22% near a Catholic one.

4. Estimation strategy

We estimate a specification of the form:

$$Banked_{vt} = \beta\, Mission_v + \boldsymbol{X}'_{vt}\boldsymbol{\gamma} + \boldsymbol{Z}'_v\boldsymbol{\lambda} + \delta_c + \tau_t + \varepsilon_{vt}$$

where $Banked_{vt}$ is an indicator equal to 1 if EA v is recorded as having a bank in Afrobarometer survey Round t; $Mission_v$ equals 1 if the centroid of EA v lies within 25 km of a historical Christian mission station;[5] X_{vt} is a vector of present-day EA-level demographic controls, including the mean age of respondents, the mean of age squared, and the share of female respondents, thereby avoiding "bad controls" in the sense of Angrist and Pischke (2009); Z_v is a vector of time-invariant historical and geographic controls; δ_c and τ_t denote country and survey-round fixed effects, respectively; and ε_{vt} is the error term, with standard errors clustered at the EA level.

Our object of interest is β, which captures the association between historical mission proximity and present-day bank presence, conditional on a rich set of observable confounders. Ordinary least squares (OLS) estimation of Equation 1 is unlikely to recover a causal effect because mission placement was non-random. Historically, missionaries selected sites based on factors such as local population density, the disease environment, accessibility, and pre-existing political structures (Nunn, 2010; Jedwab et al., 2022), many of which may independently predict present-day financial outcomes. The ideal experiment would compare EAs that did and did not receive a mission while holding fixed all factors that jointly determined mission placement and long-run financial development – the counterfactual required to identify a causal effect (Heckman, Ichimura, & Todd, 1997; Rosenbaum & Rubin, 1983). In the absence of a plausible instrument or a sharp discontinuity in mission placement, we cannot construct this counterfactual directly.

We therefore pursue two complementary strategies to assess how much of β plausibly reflects a causal relationship rather than selection on unobservables. First, we saturate our specification with an extensive set of historical and geographic controls – including distance to colonial railways, distance to 19th-century explorer routes, distance to cities recorded in AD 1400 and AD 1800, terrain ruggedness, malaria suitability, elevation, and precipitation – intended to absorb the observable determinants of both mission placement and long-run development. Second, following Cagé and Rueda (2020), Fors et al. (2024), Okoye (2021), and Alpino and Hammersmark (2021), we apply the coefficient-stability approach of Oster (2019), which formalises how strong selection on unobserved confounders would need to be, relative to selection on our observed controls, to fully explain away the estimated effect of mission proximity on bank presence. We view this as a transparent bound on the plausibility

[4] The terrain ruggedness index measures the mean absolute elevation difference between a cell and its eight neighbours (Riley, DeGloria, & Elliot, 1999).

[5] We use a 25 km threshold at baseline, following Jedwab et al. (2022) and Becker and Meier zu Selhausen (2025), and evaluate robustness to alternative cutoffs in Section 5.

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of a causal interpretation, rather than as a substitute for a design-based identification strategy, and we report results under both approaches in Section 5.

5. Results

5.1. Main results

Table 1 reports our baseline estimates of Equation 1. The dependent variable is a binary indicator of whether a village hosts a bank facility. The estimates are from a linear probability model.

Column 1 reports the unconditional relationship between mission proximity and bank presence: Villages within 25 km of a historical mission station are 11.5 percentage points more likely to host a bank facility than villages farther away. Column 2 adds present-day demographic controls (mean respondent age, age squared, and the share of female respondents in the village); the coefficient is essentially unchanged at 11.5 points, indicating that observable demographic composition does not explain the raw relationship.

When Column 3 adds historical controls, the coefficient falls modestly to 11.2 points and is unchanged when geographic controls (terrain ruggedness, malaria suitability, elevation, and precipitation) are added in Column 4. Column 5 additionally includes country and survey-round fixed effects, absorbing all cross-country and cross-wave variation; the coefficient falls to 9.6 points.

The preferred estimate in Column 5 is economically sizeable, corresponding to 27% of the 36% sample mean. The coefficient changes little when historical and geographic controls are added, remaining between 11.2 and 11.5 points in columns 1-4. The limited sensitivity of the coefficient to observable controls motivates the Oster analysis reported below.

Table 1: Mission proximity and bank presence: Baseline

	Dependent variable: Banked EA				
	(1)	(2)	(3)	(4)	(5)
Any mission (within 25 km)	0.1149***	0.1146***	0.1124***	0.1119***	0.0960***
	(0.0060)	(0.0060)	(0.0064)	(0.0064)	(0.0065)
Observations	26,606	26,605	26,605	26,605	26,605
R^2	0.0140	0.0155	0.0306	0.0373	0.1525
Mean of dep. variable	0.363	0.363	0.363	0.363	0.363
Std. dev. of dep. variable	0.481	0.481	0.481	0.481	0.481
Present-day controls	No	Yes	Yes	Yes	Yes
Historical controls	No	No	Yes	Yes	Yes
Geographic controls	No	No	No	Yes	Yes
Survey-round F.E.	No	No	No	No	Yes
Country F.E.	No	No	No	No	Yes

Notes: *The dependent variable equals 1 if the EA is recorded as having a bank (rounds 6-7) or a bank, money-transfer point, mobile banking service, or ATM (rounds 8-9), and 0 otherwise. Any mission equals 1 if the EA centroid lies within 25 km of a Christian mission station. Present-day controls include cluster-level mean age, mean age squared, and the share of female respondents. Historical and geographic controls include distance to the nearest colonial railway, pre-colonial explorer route, and city with a recorded population in AD 1400 or AD 1800, as well as terrain ruggedness, the* plasmodium falciparum *temperature suitability index, elevation, and annual precipitation. Standard errors, clustered at the EA level, are reported in parentheses. ***p<0.01, **p<0.05, *p<0.10.*

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5.2. Selection on unobservables

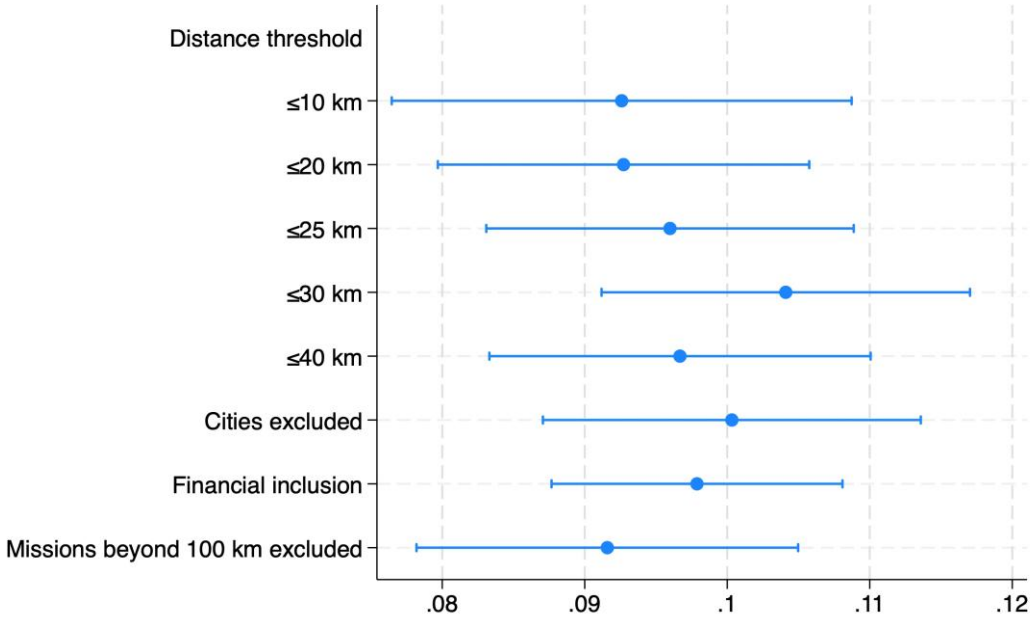
The non-random placement of Christian missions is a central threat to a causal interpretation of the results in Table 1: Missionaries selected sites based on population density, disease environment, and accessibility (Jedwab et al., 2022), any of which could independently shape long-run financial development. Absent a plausible instrument or discontinuity in historical mission placement, we cannot rule out selection on unobservables directly. We instead apply the coefficient-stability bounding procedure of Oster (2019), which formalises how strong selection on unobserved confounders would need to be, relative to selection on our observed controls, to fully account for the estimated effect of mission proximity on bank presence. The logic follows directly from the pattern in Table 1: If β moves little as controls are added, selection on the unobservables that remain is plausibly limited as well.

Appendix Table A.4 reports the resulting δ statistics, computed using the Stata module *psacalc*, with R_{max} set to 1.3 times the R^2 of each corresponding regression. With present-day and historical controls alone, δ = 4.86; adding geographic controls raises this to δ = 4.97; adding country and survey-round fixed effects yields δ = 3.92. Across all three specifications, unobserved confounders would need to be at least 3.9 times as strongly correlated with mission placement and bank presence as our observed controls to drive the estimated effect to zero – well above the threshold of $\delta \geq 1$ conventionally treated as reassuring evidence against omitted-variable bias.

5.3. Robustness

For the baseline, we use a 25 km treatment radius. Figure 2 checks the robustness of the results to alternative distance cutoffs. The figure shows that the baseline result is not an artifact of the 25 km treatment radius: The coefficient ranges from 9.3 percentage points at 10 km and 20 km to 10.4 points at 30 km, and is 9.7 points at 40 km.

Figure 2: Mission proximity and bank-facility presence: Robustness checks



Notes: *Each row reports the OLS coefficient with 95% confidence interval. "Cities excluded" drops all EAs whose centroid falls within 50 km of a city with a recorded population in AD 1400 or AD 1800, addressing the concern that mission stations and banking infrastructure co-locate near pre-colonial urban centres. All specifications include the full set of present-day, historical, and geographic controls and absorb country and survey-round fixed effects. See Appendix Table A.6 for coefficient estimates and standard errors.*

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We next exclude all EAs within 50 km of a city recorded in AD 1400 or AD 1800. This removes locations most likely to reflect persistent advantages associated with pre-colonial urban centres rather than missionary activity. The estimated coefficient is 10.0 percentage points, close to the 9.6-point baseline estimate.

In our baseline specification, the treatment group comprises EAs within 25 km of a mission station, while the control group includes those beyond 25 km. However, including far-off, unexposed areas in the control group may introduce bias if distant regions differ systematically from treated ones. We therefore restrict the comparison group to EAs located 25-100 km from the nearest mission, making treated and comparison locations more geographically similar. As shown in Figure 2, our estimates remain robust under this restriction.

Because nearby EAs may be subject to correlated shocks, we also report Conley standard errors that allow for spatial correlation within 50 km, 75 km, and 100 km (Conley, 1999). Appendix Table A.8 reports results using Conley standard errors. Although the standard error increases from 0.007 to 0.015, the coefficient of 0.096 remains statistically significant at the 1% level across all three cutoffs.

Finally, Column 7 of Appendix Table A.6 replaces bank presence with an individual-level measure of financial inclusion: the share of respondents in the EA who report owning a bank account. This variable is available only in rounds 8 and 9. EAs within 25 km of a mission have a 9.8-percentage-point higher account-ownership share, consistent with the bank-presence result.

5.4. Survey rounds, question wording, and EA identification

Two features of the data change between rounds 7 and 8. First, the bank item broadens from "bank" to "bank, money-transfer point, mobile banking services, or ATM" (Section 3), and the share of EAs recorded as having the facility rises from 25% in rounds 6-7 to 48% in rounds 8-9. Second, EA identification is exact in rounds 8-9 but approximate in rounds 6-7, where the number of coordinate points departs from the number of sampled EAs in several country-rounds.

In Appendix Table A.7, which examines the sensitivity of the baseline estimate to both, Column 1 reproduces the baseline. Restricting the sample to rounds 8-9 raises the coefficient to 13.9 percentage points (Column 2), whereas rounds 6-7 yield 5.8 points (Column 3). Relative to the corresponding sample means of 48% and 25%, however, the two estimates are similar, at 29% and 23% of the mean, so much of the difference in levels reflects the broader outcome definition in the later rounds. Column 4 drops the 25 country-rounds in which the number of coordinate points differs by more than 30% from the number of EAs implied by the sampling design (respondents divided by eight), and Column 5 instead weights each country-round equally, so that country-rounds with many coordinate points do not receive disproportionate weight. The coefficients are 12.2 and 11.6 percentage points, respectively.

Because the wording change and the improvement in EA identification occur in the same round, we cannot separate their contributions to the larger coefficient in rounds 8-9. Neither, however, accounts for the baseline association: Mission proximity predicts bank presence under each wording on its own, and the estimate rises rather than falls when the country-rounds with imprecise EA identification are excluded or down-weighted. To the extent that coarse coordinates in Round 6 add measurement error to mission proximity, the pooled baseline is, if anything, conservative. The Conley standard errors in Appendix Table A.8 also allow for correlation among nearby coordinate points, including points that belong to the same EA.

5.5. Heterogeneity by mission denominations

Historical evidence suggests that missionary denominations differed in their institutional strategies and, consequently, in their long-run developmental impacts. A substantial

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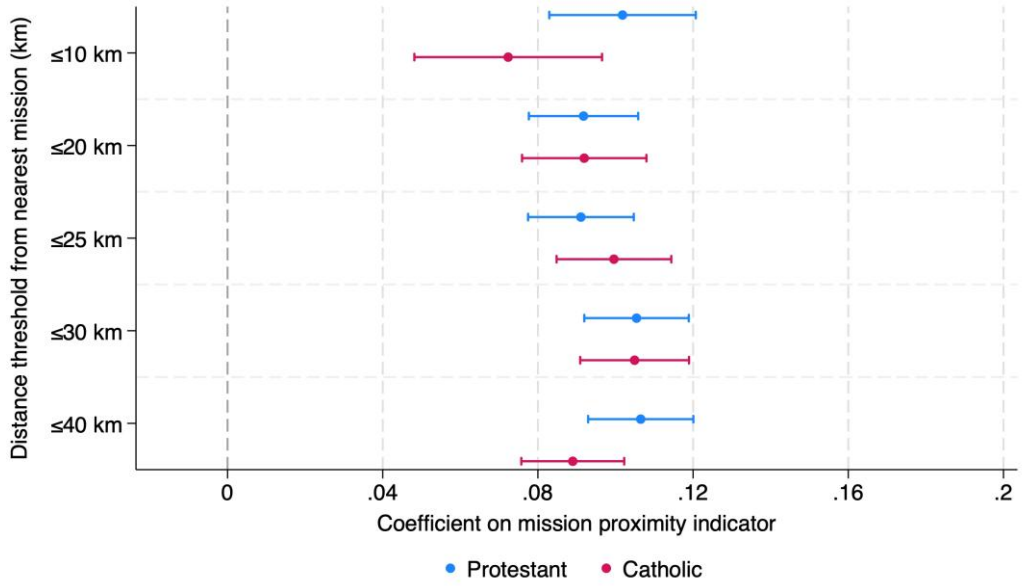
literature argues that Protestant missions generated stronger educational outcomes in sub-Saharan Africa through their greater emphasis on literacy, mass schooling, and Bible translation into local languages (Gallego & Woodberry, 2010; Woodberry, 2004; Cagé & Rueda, 2016). More recent evidence, however, challenges this view. Using newly assembled historical data, Becker and Meier zu Selhausen (2025) find no systematic differences in the long-run effects of Protestant and Catholic missions across a range of development outcomes.

Motivated by this debate, we examine whether the relationship between historical mission presence and present-day banking access varies by denomination. Figure 3 plots separate estimates for Catholic and Protestant missions across alternative distance thresholds, and Appendix Table A.5 reports the coefficients. Across all distance thresholds, proximity to both Catholic and Protestant missions is positively and significantly associated with the presence of a bank facility. The estimated effects range from 7.2 to 10.7 percentage points, indicating that the baseline relationship is not driven by either denomination alone.

Figure 3: Mission and bank presence: Protestant vs. Catholic stations



Notes: *Each point reports the OLS coefficient with a 95% confidence interval for the binary indicator equal to 1 if the EA centroid lies within the stated distance of a Protestant or Catholic mission station. All specifications include the full set of present-day, historical, and geographic controls and absorb country and survey-round fixed effects. Standard errors are clustered at the EA level. See Appendix Table A.5 for coefficient estimates and standard errors.*

The spatial pattern, however, differs modestly across denominations. Protestant-mission proximity exhibits a relatively stable association across distance thresholds, ranging between 9.1 and 10.7 percentage points. Catholic-mission proximity displays a more pronounced increase over intermediate distances, rising from 7.2 points within 10 km to a peak of 10.5 points at 30 km, before declining to 8.9 points at 40 km. At the baseline 25 km threshold, the estimated effects are similar across denominations, amounting to 10.0 points for Catholic missions and 9.1 points for Protestant missions.

Comparing the two denominations across thresholds reveals generally modest differences. Within 10 km, Protestant-mission proximity is associated with a larger effect than Catholic-mission proximity (10.2 vs. 7.2 points). At 20 km, the estimates are identical (9.2 points). The Catholic estimate is slightly larger at 25 km (10.0 vs. 9.1 points), while the estimates coincide again at 30 km (10.5 points each). By 40 km, the Protestant estimate again exceeds the Catholic estimate (10.7 vs. 8.9 points). Formal tests of coefficient equality fail to reject the null

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at the 5% level at every threshold,[6] although the differences at 10 km and 40 km are marginally significant at the 10% level.

The somewhat larger Protestant coefficient at 40 km would be consistent with Protestant missions' heavier investment in schools, printing presses, and vernacular literacy, which may have extended their influence beyond the immediate mission station (Woodberry, 2004; Cagé & Rueda, 2016). Given that none of the differences is significant at the 5% level, however, we do not interpret them further. The consistently positive and statistically significant coefficients for both denominations – combined with the absence of any statistically significant gap between them – indicate that the main relationship between historical mission presence and present-day banking access is not driven by a single missionary tradition. This finding aligns with Becker and Meier zu Selhausen (2025), who document no differences between Protestant and Catholic missions in either overall or gender-specific human-capital legacies.

5.6. Potential mechanisms

Table 2 examines whether mission proximity predicts the demand- and supply-side conditions discussed in Section 2. Panel A considers education, agricultural employment, household assets, and lived poverty, while Panel B examines local post offices, schools, markets, and urban status.

Demand-side channel

Panel A shows that mission proximity is associated with stronger demand-side fundamentals. EAs within 25 km of a mission have a 7.3-percentage-point higher share of respondents with secondary education, an 8.8-point lower share employed in agriculture, a 0.18-standard-deviation higher asset index, and a 0.12-standard-deviation lower Lived Poverty Index score.

The education result is consistent with previous evidence on the persistent human-capital effects of missionary activity (Nunn, 2010; Cagé & Rueda, 2020). In our setting, these persistent differences may also matter for bank location because education, wealth, and participation in non-agricultural activity increase the potential demand for formal financial services.

Supply-side channel

Panel B shows that mission proximity is also associated with local infrastructure. EAs within 25 km of a mission are 2.9 percentage points more likely to have a post office, 2.8 points more likely to have a school, and 5.4 points more likely to have a market. They are also 18.7 points more likely to be classified as urban (Column 8), the largest association in the table relative to its mean, consistent with missions having contributed to local agglomeration of population and economic activity. The post-office association is relevant to the historical expansion of formal savings services: Colonial administrations used postal networks to provide savings facilities from the late 19th century onward (Madimu & Msindo, 2019), while their operation required literate personnel, reliable record-keeping, and customers familiar with monetary transactions (Velasco, 2023). Missionary activity may have contributed to these local conditions.

[6] We test the null of equal Protestant and Catholic coefficients at each distance threshold by re-estimating the specification with both proximity indicators included simultaneously, country and survey-round fixed effects absorbed, and standard errors clustered at the EA level. The Protestant-minus-Catholic difference is 2.8 percentage points at 10 km (p=0.069), 0.3 points at 20 km (p=0.802), -0.8 points at 25 km (p=0.486), 0.2 points at 30 km (p=0.861), and 2.1 points at 40 km (p=0.060).

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Table 2: Potential mechanisms linking historical missions to present-day banking

	Panel A: Demand-side channels				Panel B: Supply-side channels			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Education	Agriculture	Asset index	Lived poverty	Post office	School	Market	Urbanity
Within 25 km of mission	0.0732***	-0.0881***	0.1748***	-0.0803***	0.0290***	0.0276***	0.0542***	0.1867***
	(0.0040)	(0.0038)	(0.0113)	(0.0071)	(0.0061)	(0.0048)	(0.0065)	(0.0067)
Observations	26,774	26,780	25,905	26,760	21,600	26,688	26,643	26,780
R^2	0.3541	0.3440	0.4349	0.4874	0.1293	0.0631	0.1118	0.2172
Mean dep. variable	0.346	0.235	1.952	0.986	0.190	0.852	0.668	0.457
Std. dev. dep. variable	0.323	0.299	0.965	0.653	0.392	0.355	0.471	0.496
Present-day controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-round F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *Each column reports a separate OLS regression. Panel A examines demand-side channels by considering whether historical mission stations are associated with persistent differences in human capital, structural transformation, household wealth, and poverty. The dependent variables are EA means of individual-level outcomes: the share of respondents with secondary education or above in Column 1, the share employed in agriculture in Column 2, the mean asset index in Column 3, and the mean Lived Poverty Index in Column 4. Panel B examines local infrastructure and broader economic development. The dependent variables in columns 5-7 are binary indicators equal to 1 if a post office, school, or market, respectively, is present in the EA. Column 8 uses an indicator equal to 1 for urban EAs. The main explanatory variable,* Within 25 km of mission*, equals 1 if the EA centroid lies within 25 km of a Christian mission station and 0 otherwise (Hedde-von Westernhagen & Becker, 2022). All specifications include the full set of present-day, historical, and geographic controls, together with country and survey-round fixed effects. Standard errors, clustered at the EA level, are reported in parentheses. ***p<0.01, **p<0.05, *p<0.10.*

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Limits of the evidence

Table 2 should not be interpreted as a causal mediation analysis. The mechanism outcomes are measured contemporaneously with bank presence and may themselves respond to bank entry. For example, local economic activity or infrastructure could increase after a branch opens. Reverse causality cannot explain our main association, since historical mission locations predate present-day bank presence, but it prevents us from establishing whether the outcomes in Table 2 are causes or consequences of bank location. Identifying these channels separately would require information on the timing of branch entry and the intermediate outcomes, or independent variation in each mechanism. We therefore interpret Table 2 as evidence consistent with the proposed channels rather than as a decomposition of the mission-bank relationship.

6. Conclusion

We document a persistent association between historical mission locations and present-day bank presence in Africa. EAs within 25 km of a historical mission are about 10 percentage points more likely to host a bank today than EAs located farther than 25 km from the nearest mission. The estimate changes little after we control for historical settlement, colonial transport access, geography, and present-day demographics, and remains similar when we exclude areas near pre-colonial cities. Although the cross-sectional design prevents a causal interpretation, the Oster bounds indicate that substantial selection on unobservables would be required to explain the association.

Mission proximity also predicts higher education and household assets, lower agricultural dependence, and greater local infrastructure. These correlations are consistent with missions affecting both the demand for financial services and the cost of supplying them, although their present-day measurement prevents us from identifying the mechanisms separately. The results extend the literature on missionary persistence from individual outcomes to the location decisions of financial institutions.

The findings suggest that present-day banking geography partly reflects historically accumulated differences in local human capital and institutional capacity. Policies aimed at expanding financial access may therefore interact with these local conditions. In places where complementary infrastructure is weak, expanding formal finance may require investments beyond the provision of financial services itself.

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Appendix

Table A.1: Summary statistics

	N	Mean	Std. dev.	Min.	Max.
Panel A: Outcome and treatment variables					
EA has a bank	26,606	0.363	0.481	0.00	1.00
Within 25 km of any mission	26,781	0.429	0.495	0.00	1.00
Protestant mission	26,781	0.334	0.472	0.00	1.00
Catholic mission	26,781	0.219	0.413	0.00	1.00
Panel B: Mechanism outcomes					
Secondary education or above (share)	26,775	0.346	0.323	0.00	1.00
Agricultural employment (share)	26,781	0.235	0.299	0.00	1.00
Asset index	25,906	1.952	0.965	0.00	5.00
Lived Poverty Index	26,761	0.986	0.653	0.00	4.00
Post office present	21,601	0.190	0.392	0.00	1.00
School present	26,689	0.852	0.355	0.00	1.00
Market present	26,644	0.668	0.471	0.00	1.00
Urban population (share)	26,781	0.457	0.496	0.00	1.00
Panel C: Present-day controls					
Mean age	26,780	37.2	8.109	18.00	98.00
Female (share)	26,781	0.501	0.189	0.00	1.00
Panel D: Historical controls					
Log distance to colonial railway	26,781	4.843	1.552	0.00	7.98
Log distance to explorer route	26,781	4.795	1.418	0.00	7.92
Log distance to city in 1400	26,781	6.270	0.899	0.00	7.94
Log distance to city in 1800	26,781	6.585	1.449	0.00	8.50
Panel E: Geographic controls					
Elevation (m)	26,781	589.273	597.788	-26.00	3,507.00
Precipitation (mm)	26,781	1,118.121	708.728	0.00	4,566.00
Malaria suitability	26,781	0.431	0.229	0.00	0.81
Terrain ruggedness	26,781	16.156	21.547	0.00	298.62

Notes: *The unit of observation is the EA-round cell. The sample pools Afrobarometer rounds 6-9 across 42 African countries, with N=26,781 EA-round observations before listwise deletion. Cluster-level outcomes and controls are constructed by aggregating individual-level responses to the EA level. Treatment indicators equal 1 when the EA centroid lies within 25 km of the nearest mission station of the relevant denomination. Historical controls are log distances in kilometres to the nearest colonial railway, explorer route, and city recorded for 1400 and 1800 in Chandler (1987).*

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Table A.2: Definition of the main variables

Variable	Definition	Source
Outcome variables		
Bank presence	Indicator equal to 1 if the EA is recorded as having a bank (rounds 6-7) or a bank, money-transfer point, mobile banking service, or ATM (rounds 8-9) in the EA or within easy walking distance, and 0 otherwise	Afrobarometer, rounds 6-9
Financial inclusion	Share of respondents in the EA who report owning an account at a bank or another formal financial institution	Afrobarometer, rounds 8-9
Treatment variables		
Mission proximity	Indicator equal to 1 if the EA centroid lies within 10, 20, 25, 30, or 40 km of any Christian mission station, and 0 otherwise	Hedde-von Westernhagen and Becker (2022)
Protestant-mission proximity	Indicator equal to 1 if the EA centroid lies within 10, 20, 25, 30, or 40 km of a Protestant mission station	Hedde-von Westernhagen and Becker (2022)
Catholic-mission proximity	Indicator equal to 1 if the EA centroid lies within 10, 20, 25, 30, or 40 km of a Catholic mission station	Hedde-von Westernhagen and Becker (2022)
Distance to nearest mission	Natural logarithm of 1 plus the distance, in kilometres, to the nearest mission station. Separate measures are constructed for all, Protestant, and Catholic missions.	Authors' calculations using Hedde-von Westernhagen and Becker (2022)
Present-day controls		
Mean age	Mean age of respondents in the EA	Afrobarometer, rounds 6-9
Mean age squared	Square of the mean age of respondents in the EA	Authors' calculations using Afrobarometer
Female share	Share of respondents in the EA who are female	Afrobarometer, rounds 6-9
Historical controls		
Pre-colonial city, AD 1400	Log distance to nearest city in AD 1400	Chandler (1987)
Pre-colonial city, AD 1800	Log distance to nearest city in AD 1800	Chandler (1987)
Colonial railway proximity	Log distance to a colonial railway line	Nunn and Wantchekon (2011)
Explorer-route proximity	Log distance to 19th-century European explorer route	Nunn and Wantchekon (2011)
Geographic and pre-treatment controls		
Elevation (m)	Elevation of the EA centroid in metres above sea level	WorldClim 2.1/SRTM
Terrain ruggedness	Terrain ruggedness index measured around the EA centroid	Authors' calculations using SRTM
Precipitation (mm)	Average annual precipitation at the EA centroid, measured in millimetres	WorldClim 2.1 (Fick & Hijmans, 2017)
Malaria suitability	Temperature suitability index for the transmission of *plasmodium falciparum*	Gething et al. (2011)

Notes: *All distance variables are calculated using the Africa Albers Equal Area Conic projection (ESRI:102022). Raster values are extracted at EA centroid coordinates using point-in-pixel assignment. For coastal and island EAs whose centroids do not intersect a valid raster cell, values are calculated as the mean of valid cells within a local buffer.*

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Table A.3: Number of EAs by country and Afrobarometer round

Country	R6	R7	R8	R9	Total
Algeria	134	—	—	—	134
Angola	—	—	298	150	448
Benin	141	150	150	150	591
Botswana	79	150	148	150	527
Burkina Faso	117	150	150	150	567
Burundi	142	—	—	—	142
Cabo Verde	54	154	150	150	508
Cameroon	102	150	148	149	549
Congo-Brazzaville	—	—	—	150	150
Côte d'Ivoire	90	144	143	149	526
Egypt	103	—	—	—	103
Eswatini	71	150	150	149	520
Ethiopia	—	—	300	300	600
Gabon	73	150	150	150	523
The Gambia	—	150	150	150	450
Ghana	258	1,401	297	300	2,256
Guinea	131	150	150	150	581
Kenya	228	193	339	300	1,060
Lesotho	94	147	150	150	541
Liberia	96	150	149	150	545
Madagascar	132	150	—	147	429
Malawi	28	150	150	150	478
Mali	135	150	149	150	584
Mauritania	—	—	—	148	148
Mauritius	110	150	150	150	560
Morocco	129	148	—*	150	427
Mozambique	200	287	140	140	767
Namibia	94	231	150	150	625
Niger	129	150	150	150	579
Nigeria	222	1,559	200	200	2,181
Senegal	127	199	150	150	626
Seychelles	—	—	—	149	149
Sierra Leone	122	149	150	150	571
South Africa	495	449	400	397	1,741
Sudan	51	111	223	149	534
São Tomé and Príncipe	56	150	—	150	356
Tanzania	211	300	300	299	1,110
Togo	119	190	150	150	609
Tunisia	143	148	—*	150	441
Uganda	263	145	150	300	858
Zambia	93	666	150	150	1,059
Zimbabwe	182	146	150	150	628
Total	**4,954**	**9,017**	**5,934**	**6,876**	**26,781**

Notes: *The table reports the number of EA clusters by country and Afrobarometer survey round. R6-R9 refers to survey rounds 6-9, fielded in 2014/2015, 2016/2018, 2019/2021, and 2021/2023, respectively. "—" indicates that the country was not included in the corresponding round. The final row reports the total number of clusters in each round, while the final column reports the cumulative number of clusters across all rounds. Clusters are identified by GPS coordinates. In rounds 8-9, they coincide with Afrobarometer EA identifiers; in rounds 6-7, where no identifier is available, coarser geocoding in several Round 6 surveys groups several EAs into one point, and finer geocoding in Round 7 for Ghana, Nigeria, and Zambia splits EAs into several points (see Section 5.4).*

**While Morocco and Tunisia were surveyed in Round 8, their geocoded data for that round were not available for analysis.*

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Table A.4: Robustness to omitted variable bias: Oster (2019) bounds

	(1)	(2)	(3)
$\hat{\beta}$ (controlled)	0.112	0.112	0.096
R^2 (uncontrolled)	0.014	0.014	0.014
R^2 (controlled)	0.031	0.037	0.152
$R_{\max}$	0.040	0.048	0.198
Oster's δ	**4.86**	**4.97**	**3.92**
Present-day controls	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes
Geographic controls	No	Yes	Yes
Survey-round F.E.	No	No	Yes
Country F.E.	No	No	Yes

Notes: *The table reports Oster (2019) omitted-variable bias bounds for the coefficient on mission proximity, computed with psacalc (Oster 2019). $\hat{\beta}$ (controlled) is the OLS coefficient on Within 25 km of a mission from the corresponding specification in Table 1. R^2 (uncontrolled) is the R2 from the bivariate regression of the outcome on the treatment alone, estimated on the same sample. $R_{max} = 1.3 \times R^2$ (controlled) follows Oster's (2019) recommended upper bound for the maximum attainable R^2. Oster's δ is the ratio of selection on unobservables to selection on observables that would be required to drive the estimated coefficient to zero. Values above 1 indicate that unobservables would need to be more important than observables to explain away the result.*

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Table A.5: Mission and bank presence: Heterogeneity by denomination

	Dependent variable: Banked EA									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Within 10 km of Catholic mission	0.072***									
	(0.012)									
Within 10 km of Protestant mission		0.102***								
		(0.010)								
Within 20 km of Catholic mission			0.092***							
			(0.008)							
Within 20 km of Protestant mission				0.092***						
				(0.007)						
Within 25 km of Catholic mission					0.100***					
					(0.008)					
Within 25 km of Protestant mission						0.091***				
						(0.007)				
Within 30 km of Catholic mission							0.105***			
							(0.007)			
Within 30 km of Protestant mission								0.105***		
								(0.007)		
Within 40 km of Catholic mission									0.089***	
									(0.007)	
Within 40 km of Protestant mission										0.107***
										(0.007)
Observations	26,605	26,605	26,605	26,605	26,605	26,605	26,605	26,605	26,605	26,605
R^2	0.147	0.150	0.150	0.151	0.151	0.151	0.153	0.153	0.151	0.153
Present-day controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-round F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *Each column reports a separate OLS regression. The dependent variable is a binary indicator equal to one if a bank facility is present in the enumeration area. The explanatory variable in each column is a binary indicator equal to one if the enumeration-area centroid lies within the indicated distance of a Catholic or Protestant mission station, and zero otherwise. Columns (5) and (6) use the 25-kilometer threshold employed in the baseline specifications. All regressions include present-day, historical, and geographic controls, as well as country and survey-round fixed effects. Standard errors, clustered at the EA level, are reported in parentheses. ***p<0.01, **p<0.05, *p<0.10.*

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Table A.6: Mission proximity and bank presence: Robustness

	Panel A: Alternative distance thresholds					Panel B: Other checks		
	Dependent variable: Banked EA					Banked EA, historical cities excluded	Financial inclusion, alternative outcome	Banked EA, nearest mission ≤ 100 km
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any mission within 10 km	0.0926*** (0.0082)							
Any mission within 20 km		0.0927*** (0.0066)						
Any mission within 25 km			0.0960*** (0.0066)			0.1003*** (0.0068)	0.0979*** (0.0052)	0.0916*** (0.0068)
Any mission within 30 km				0.1041*** (0.0066)				
Any mission within 40 km					0.0967*** (0.0068)			
Observations	26,605	26,605	26,605	26,605	26,605	25,505	12,810	20,888
R^2	0.1499	0.1520	0.1525	0.1535	0.1519	0.1498	0.5410	0.1740
Present-day controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-round F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *Panel A reports estimates obtained using alternative distance thresholds to define proximity to a Christian mission station. Each treatment indicator equals 1 if the Afrobarometer EA centroid lies within the stated distance of a mission station. Panel B reports robustness checks based on the 25-km threshold. Column 6 excludes EAs located within 50 km of a city recorded in either AD 1400 or AD 1800. Column 7 uses the cluster-level financial-inclusion measure as the dependent variable. Column 8 restricts the sample to EAs whose nearest mission station is located within 100 km.*

All specifications include present-day, historical, and geographic controls, as well as country and survey-round fixed effects. Present-day controls include cluster-level mean age, mean age squared, and the share of female respondents. Historical controls include distance to the nearest colonial railway, pre-colonial explorer route, and city with a recorded population in AD 1400 or AD 1800. Geographic controls include elevation, annual precipitation, terrain ruggedness, and the plasmodium falciparum *temperature suitability index. Standard errors, clustered at the EA level, are reported in parentheses. ***p<0.01, **p<0.05, *p<0.10.*

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Table A.7: Robustness to survey round and EA identification

	Dep. variable: Banked EA				
	Baseline	Rounds 8-9	Rounds 6-7	Excludes flagged country-rounds	Equal country-round weights
	(1)	(2)	(3)	(4)	(5)
Any mission (within 25 km)	0.0960***	0.1391***	0.0584***	0.1218***	0.1157***
	(0.0066)	(0.0101)	(0.0084)	(0.0079)	(0.0074)
Observations	26,605	12,717	13,888	19,382	26,605
R^2	0.1525	0.1596	0.0979	0.1724	0.1546
Mean of dep. variable	0.363	0.483	0.254	0.390	0.363
Item wording	Both	Broad	"Bank"	Both	Both

Notes: *All columns estimate the Column 5 specification of Table 1, with present-day, historical, and geographic controls and country and survey-round fixed effects. Column 2 restricts the sample to rounds 8-9, in which EA-FAC-F reads "Bank, money-transfer point, mobile banking services, or ATM" and coordinate-based clusters coincide with Afrobarometer EA identifiers. Column 3 restricts the sample to rounds 6-7, in which the item reads "Bank." Column 4 excludes the 25 country-rounds in which the number of distinct coordinate points differs by more than 30% from the number of EAs implied by the sampling design (respondents divided by eight): 17 in Round 6; Ghana, Namibia, Nigeria, Senegal, South Africa, and Zambia in Round 7; and South Africa in rounds 8 and 9. Column 5 weights each observation by the inverse of the number of observations in its country-round. Standard errors, clustered at the EA level, are reported in parentheses. ***p<0.01, **p<0.05, *p<0.10.*

Table A.8: Robustness to spatial standard errors

		Conley standard errors		
	Baseline	50 km	75 km	100 km
	(1)	(2)	(3)	(4)
Within 25 km (all missions)	0.096***	0.096***	0.096***	0.096***
	(0.007)	(0.015)	(0.016)	(0.015)
Observations	26,605	26,605	26,605	26,605
R^2	0.152	0.152	0.152	0.152
Controls	Yes	Yes	Yes	Yes
Country & round F.E.	Yes	Yes	Yes	Yes
SE type	Clustered	Conley (cutoff in column header)		

Notes: *The dependent variable is a binary indicator equal to 1 if the EA has a bank facility. All columns estimate the same OLS specification; the point estimate is invariant to the standard-error correction. The baseline clusters standard errors at the EA level. Conley standard errors allow for arbitrary spatial correlation within the stated radius around each observation (Conley, 1999). All specifications include the full set of present-day, geographic, and historical controls and absorb country and survey-round fixed effects. ***p<0.01, **p<0.05, *p<0.10.*

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