The Arab Slave Trade and the Diffusion of Islam in Africa*

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Abstract

This paper investigates the role of the Arab slave trade in the diffusion of Islam in Africa. Three of the four major slave-trading routes operating in Africa between 1400 and 1900 were run by Muslim raiders. Because Islamic law prohibited the enslavement of free Muslims, raided populations had an incentive to convert preventively to protect against capture. We show that groups more exposed to the Arab raiding routes exhibit a higher historical and contemporary prevalence of Islam. Results are robust to rich geographic and ethnographic controls, country fixed effects, the exclusion of areas in historical Muslim kingdoms, and an instrumental variable based on distance to the nearest Arab slave port. We find no effect for the Atlantic route, run by Europeans, where conversion granted no protection. Two patterns support the mechanism of preventive conversion: spillovers in never-raided neighbors of raided groups, which attenuate where mobility or mounted raiding was costly, and weaker effects in rugged terrain that helped groups defend against raiders. We then ask what form of Islam preventive conversion produced. Muslims in exposed groups report stronger religiosity, more conservative gender norms, and less syncretism, consistent with a stricter adoption of Islam required to signal Muslim identity to raiders. These attitudes shape women's socio-economic behaviors. Muslim women from raided groups work less and bear more children, an effect absent for Muslim men and for non-Muslim women from the same groups. Overall, this paper documents a key channel through which Islam spread in Africa, and shows how the way a religion is spread impacts the form of beliefs and practices it takes.

Keywords: Slave Trade, Islam, Institutions, Persistence, Africa
JEL Codes: J15, N47, Z1, Z12, Z13

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1 Introduction

> *"Whoever is captured in a condition of non-belief, it is legal to own him [...] but not he who was converted to Islam voluntarily [...] These are free Muslims, whose enslavement is not allowed in any way."* — Aḥmad Bābā, jurist, in 1614, quoted in Lovejoy (2011, pp. 31–32).

Religion is a first-order determinant of economic behavior and development (Becker et al., 2024; Iyer, 2016; Lowes, 2024; McCleary and Barro, 2006), particularly in low- and middle-income countries, where religiosity remains high and religious institutions play a central socio-economic role (Lowes et al., 2025). A growing body of economic literature analyzes the historical determinants of religious affiliation and practice. Yet even within the same religion, beliefs and practices vary considerably, and what drives this variation and what it implies for economic outcomes remain largely unexplored. One candidate explanation is the channel through which a religion spreads: historically, diffusion can be top-down, through conquest and the conversion of ruling elites, or bottom-up, through the independent choices of ordinary households and communities, or through influence from religious entrepreneurs. Religious diffusion can also be peaceful, reflecting sincere belief or the pursuit of material benefit, or coerced, by fear or pressure. Each dimension of diffusion plausibly shapes not only whether a religion spreads, but the form it takes.

In this paper, we fill this gap by studying the role of the Arab slave trade in the diffusion of Islam in Africa. Between the fifteenth and nineteenth centuries, four major slave-trading routes operated on the African continent: the Red Sea, trans-Saharan, and Indian Ocean routes (the "Arab routes"), largely run by Muslim raiders serving Arab and Ottoman markets, and the trans-Atlantic route, operated by European merchants (Lovejoy, 2011; Saleh and Wahby, 2022; Wright, 2007). Because Islamic law prohibited the enslavement of free Muslims while permitting the enslavement of non-Muslims outside Muslim-ruled territories, historians have long argued that the Arab slave trade created incentives for preventive conversion in raided areas, a case of bottom-up, coerced diffusion. However, these accounts remain primarily qualitative (Lovejoy, 2011; Manning, 1990): whether preventive conversion had persistent effects on religious adherence and what form of Islamic practice it produced remain untested.

To test this hypothesis, we link historical estimates of the number of slaves exported from each ethnic group along the four African slave-trade routes between 1400 and 1900 (Nunn, 2008; Nunn and Wantchekon, 2011) to measures of Islamic presence at the ethnic-territory level. First, we measure Muslim adherence at the ethnic-territory level using the World Religion Database (2005). Second, we digitize a continent-wide scholarly inventory and maps of historical mosques (9th–early 20th centuries) from Pradines (2022) that we geolocalize to

create a measure of historical presence of Islam. Third, we assemble contemporary individual-level data from all available Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) in Africa that elicit religion and self-reported ethnicity. We use respondents' ethnicity to link the DHS/MICS surveys to the historical ethnicity-level slave-export data. This yields a database covering 585,097 women across 66 survey waves in 28 countries.[1]

Our main empirical strategy exploits within-country variation across ethnic groups in historical exposure to the Arab slave trade, controlling for geographic and ancestral covariates that the literature links to the diffusion of Islam in Africa, such as land-suitability inequality and proximity to historical trade routes (Michalopoulos et al., 2016, 2018). We focus on the Red Sea, trans-Saharan, and Indian Ocean routes (the "Arab routes"), and use the Atlantic route as a placebo.

We find that greater exposure to the Arab routes is strongly associated with more historical mosques and higher contemporary Muslim shares at the ethnic-group level. A one standard deviation increase in Arab-route captives raises the probability that an ethnic territory hosts at least one historical mosque by 4 percentage points — a roughly 40 percent increase over the sample frequency — and is associated with a 4.3 percentage point higher Muslim share in 2005, about 15 percent of the sample mean. The same increase raises the probability that an ethnic group is majority Muslim by 4.8 percentage points, an 18 percent increase relative to a baseline of 26 percent. To put these magnitudes in perspective, the Muslim-share effect is roughly one third of the effect of distance from the historical trade routes emphasized by Michalopoulos et al. (2016, 2018). Results are qualitatively similar at the individual level. Exposure to the Arab route is also negatively associated with Christian and animist shares, consistent with reallocation across faiths rather than differential population growth.

There are two concerns in interpreting these results causally. One is that Islam also spread through Muslim trade networks: merchants brought the faith to commercial towns, where local traders and, later, rulers converted before it diffused into the broader population (Levtzion and Pouwels, 2000; Michalopoulos et al., 2018). This trade-based diffusion was one of the primary channels for the diffusion of Islam in Africa and may have occurred alongside, and be correlated with, raiding-based preventive conversion. A second concern is that ethnic groups historically more likely to adopt Islam may also have been differentially targeted by Arab raiders. We address these concerns in four ways. First, we compare ethnic groups within countries by including country fixed effects, which absorb country-level characteristics common to all ethnic groups within a country. Second, in the individual-level specification we replace country with district fixed effects, a more demanding test that compares respondents

[1] 87% of respondents can be matched to an ethnic group. See Section 3 for details on sample construction and matching.

from different ethnic groups within the same district. Third, we condition on a rich set of geographic and historical covariates that the literature links to Islam's diffusion, including the trade-route proximity and land-inequality channels of Michalopoulos et al. (2018). The estimated effect changes little as this specification is saturated with additional covariates, and formal tests drawing on Oster (2019) and Cao et al. (2024) indicate that omitted variable bias is unlikely to explain our key findings. Finally, we instrument each group's Arab-route exposure with the distance from its homeland to the nearest Arab slave port, following Nunn (2008) and Corno et al. (2020). The resulting estimates are consistent with the OLS findings. Results are further robust to excluding ethnic territories historically inside (or near) Muslim-ruled polities, to comparing adjacent ethnic territories with dyad fixed effects and matching on observables, and to an alternative exposure measure that excludes the Indian Ocean route, where European participation increased in the eighteenth–nineteenth centuries.

We then test whether preventive conversion, the adoption of Islam to protect against slave raiding identified in the historical literature, drives this pattern. Because our treatment measure counts individuals *actually captured*, not the underlying *threat of capture*, recorded exports understate the true threat where conversion succeeded in deterring raiders.[2] First, raids on a neighboring ethnic group should raise perceived risk even for groups never themselves raided. We find exactly this: neighbors' Arab exposure predicts higher Muslim adherence among never-raided groups, and the spillover is attenuated where difficult geography constrained preindustrial human mobility between a group and its neighbors (Özak, 2018). This spillover could still reflect ordinary contact-based diffusion between neighboring populations rather than risk. To distinguish these two channels, we draw on Alsan (2015) and show that the spillover weakens where a group's tsetse-fly suitability relative to its neighbors offered protection from mounted raiding (Law, 1976; Webb, 1993), controlling for preindustrial human mobility. The tsetse fly carries *trypanosomiasis*, an illness lethal to horses and camels, which made mounted raiding prohibitively costly in tsetse-prone regions. Consistent with this channel, the attenuation is concentrated on the trans-Saharan and Red Sea routes, which relied on mounted raiders, and not on the Indian Ocean route, which did not, though the latter comparison draws on limited within-country variation. Second, among groups that did experience slave raiding, if conversion provided protection, the incentive to convert should be weaker where rugged geography helped groups escape from and fend off slave raiders. Consistent with this, the association between Arab exposure and Muslim adherence weakens significantly in territories with severely rugged terrain that helped groups defend from slave raids (Nunn and Puga, 2012).

We then move to investigating the downstream implications of such preventive conversion for the *form* of Islam practiced. *Ex ante*, conversion to Islam to minimize the risk of being

[2]Notably, any bias introduced by this is likely downward, making our estimates conservative. Indeed, the IV results point in the direction of an attenuation bias in our coefficients.

raided could have led to two forms of Islam. On the one hand, converts under coercion may have had little desire to genuinely adopt the aggressor's culture, producing a *nominal form* of Islam with persistent traditional beliefs and practices. On the other hand, because conversion was self-initiated to secure protection, communities had reason to coordinate around a credible, publicly observable Muslim identity, producing a *substantive and stricter form of Islam*, characterized by more conservative gender norms, less syncretism with traditional religions, and greater religiosity. Indeed, historians record contemporaries judging which groups could lawfully be enslaved by how faithfully they were seen to practice Islam (García Novo, 2011; Oualdi, 2024, discussed further in Section 6.2.1). We find that the empirical evidence aligns closer with the second view. Using individual-level data from Pew's "Tolerance and Tension: Islam and Christianity in Sub-Saharan Africa" (Pew Research Center's Forum on Religion & Public Life, 2010), we find that individuals in more-exposed ethnic homelands are more likely to hold conservative gender attitudes and less likely to retain traditional African beliefs that Islam explicitly proscribes. Evidence from the folklore catalogue of Michalopoulos and Xue (2021) corroborates these patterns, showing that conservative and Islamic-practice motifs are more frequent in the oral traditions of exposed groups. A third source, newly digitized ethnographic narratives from the Joshua Project, a missionary-authored repository of group-level data that we newly compile and match to our sample, shows the same pattern.

Preventive conversion also produced a second, independent byproduct: an interiorized racial hierarchy within Islam. Although Islamic doctrine treats all believers as equal regardless of race, a racial-status hierarchy emerged in practice across the Muslim world, associating Blackness with slave status and Arab identity with freedom and religious prestige (Lewis, 1990). This left Black African converts in an ambiguous position: Muslim by religion, but racially associated with the enslaved, and therefore more likely to have their claim to genuine Muslim status, and the legal protection it carried, questioned by others. Using folklore motifs from Michalopoulos and Xue (2021), we find suggestive evidence that Arab slave trade exposure predicts more white-favoring motifs in local oral traditions, consistent with raided groups interiorizing Arab identity and whiteness as markers of prestige.

We then ask whether this more conservative form of Islam produced by preventive conversion shapes, in turn, economic behavior. We test this using individual-level data on women's labor-market outcomes from all IPUMS-DHS surveys in Africa that record ethnicity. We find that Muslim women descended from more heavily raided groups participate less in the labor force, bear more children, and become mothers earlier: a one log-point increase in Arab-route exposure lowers female labor force participation by 2.8 percentage points against a sample mean of 59 percent. The penalty is absent for Muslim men, and the results survive controls for the ancestral determinants of female labor supply emphasized in prior work, namely pastoralism (Becker, 2025) and plough agriculture (Alesina et al., 2013). We confirm that this is unlikely to be a demographic legacy of the slave trade itself: while we replicate

Teso (2019)'s finding that Atlantic exposure raised female participation through the demographic shock to sex ratios, the Arab-route penalty loads entirely on Muslim women, while non-Muslim women from the same raided groups remain unaffected.

Together, these results provide the first empirical evidence for the preventive conversion mechanism long hypothesized in the historical literature to explain the diffusion of Islam in Africa. We also show that it generated a stricter form of Islam, with lasting effects on beliefs, practices, and ultimately economic behavior. More broadly, our findings suggest that the mechanism by which a population comes to adopt a religion, not merely whether it does so, shapes the form that religion subsequently takes, cautioning against treating a religion and its diffusion as a monolith.

This paper contributes to three literatures. First, it contributes to the literature on the long-run impact of historical institutions on economic development, which studies how historical events and subsequent institutional paths, shape contemporary outcomes (Acemoglu et al., 2001, 2002; Sokoloff and Engerman, 2000).[3] Our paper relates to a subset of this literature that studies the long-run impact of coercive and extractive institutions (Banerjee and Iyer, 2005; Beigelman, 2024; Dell, 2010; Dell and Olken, 2019; Lowes and Montero, 2021; Méndez and Van Patten, 2022; Naidu and Yuchtman, 2013). This empirical literature has, in particular, analyzed the long-run legacies of Africa's slave trades on economic performance and financial development (Derenoncourt, 2025; Levine et al., 2017; Nunn, 2008; Pierce and Snyder, 2018), culture, family structure and gender norms (Corno et al., 2020; Edlund and Ku, 2011; Lowes and Nunn, 2024; Nunn and Wantchekon, 2011; Teso, 2019), political structure, conflict, and ethnic fragmentation (Fenske and Kala, 2017; Green, 2013; Whatley and Gillezeau, 2011). Relative to prior work, we make two contributions. First, whereas most empirical studies analyze the trans-Atlantic trade, except for Corno et al. (2020), which studies the Red Sea route, we provide, to our knowledge, the first systematic analysis of the legacies of the Arab slave routes. Second, this literature has not, to our knowledge, examined religious adherence as an outcome of the slave trade. We do so by assembling original measures of historical Islamic presence, complementing a related literature that has primarily examined missionary legacies and Christianity.

This paper also contributes to the economics of culture and, in particular, of religion.[4] In addition to work showing how religious beliefs shape economic, social, and political outcomes, a growing empirical literature investigates the determinants of religious affiliation and conversion. Empirically, several drivers have been identified (Becker et al., 2025, 2024).[5]

[3] See Nunn (2014) for a comprehensive review of this literature.

[4] For detailed reviews, see Guiso et al. (2006); Iyer (2016); McCleary and Barro (2006, 2019); Nunn (2012).

[5] A large theoretical literature also studies the formation and persistence of religious preferences, and has analyzed the role of religious capital (Iannaccone, 1992), socially adaptive beliefs and psychological motives (Bénabou and Tirole, 2002; Henrich, 2004), and cultural transmission (Bisin and Verdier, 2000) as classified by Bisin et al. (2023).

The first is state power, through coercive instruments such as war and conquest, and the legal statuses and protections they entail, as well as soft power via the colonial expansion of Christian missions (Bergeron, 2025; Cantoni, 2012; Iyigun, 2008; Nunn, 2010) or the role of media (Buccione and Mello, 2020; Grosfeld et al., 2024). The second driver highlights psychological channels: religion serves as a coping mechanism—a "psychological immune system"—that mitigates distress and provides meaning when adverse shocks occur (Bentzen, 2019; Binzel and Carvalho, 2017; Chen, 2010; Fruehwirth et al., 2019). Finally, a growing literature models religion as a marketplace with competing platforms (Seabright, 2024). In this framework, individuals choose among denominations given the total "price of belonging" (time, compliance with prohibitions) and the bundle of benefits (services, insurance, status). For example, Saleh and Tirole (2021) show that discriminatory taxes on non-Muslims in early Islamic Egypt increased conversion rates to Islam. Montero et al. (2025) study the Seventh-day Adventist Church in Sub-Saharan Africa, which bans tobacco, coffee, and tea production, increasing opportunity costs for potential members in areas suitable for these crops. These channels vary along two dimensions: whether conversion is initiated top-down, by elites, as with war and conquest, or bottom-up, by ordinary individuals, as in the marketplace mechanisms above, and whether it reflects coercion or free choice. We study the case these examples leave out, bottom-up and coerced: converting to Islam reduced the risk of being enslaved by Arab-Muslim raiders, giving ordinary individuals a direct incentive to convert out of fear rather than choice, with consequences that extend beyond religious practice to economic behavior such as female labor supply. To our knowledge, this case has not been studied in this literature.

Finally, we contribute to the quantitative economics of Islam. Much of this literature studies the role of Islam in explaining the "long divergence" in economic development between the Middle East versus Western Europe (Chaney, 2016; Kuran, 2018; Platteau, 2008, 2017; Rubin, 2011, 2017). A complementary strand quantifies the effects of specific practices and rituals on beliefs, social capital, and economic performance (Campante and Yanagizawa-Drott, 2015; Carvalho, 2013; Clingingsmith et al., 2009). There is, however, little work on the determinants of the spread of Islam, with the exception of Michalopoulos et al. (2016, 2018) which analyze the role of trade networks, agro-ecology and land inequality in Islam's historical diffusion. Our paper introduces the Arab slave trade and the conversion it incentivized as another channel through which Islam spread.

The rest of the paper proceeds as follows. Section 2 provides historical context on the Arab slave routes and the Islamic law on enslavement. Section 3 describes the data and variable construction. Section 4 details the empirical strategy. Section 5 presents the main results on historical Islamic presence and contemporary Muslim adherence. Section 6 provides further evidence on the mechanism of preventive conversion and explores its downstream implications, exploiting data on religiosity, traditional belief displacement, folklore,

and contemporary female labor force outcomes. Section 7 concludes.

2 Historical background

2.1 The Four African Slave Trades, 1400–1900

Between 1400 and 1900, Africa was integrated into four major slave-trading systems that differed in destination markets and in the identity of raiders and traders: the Atlantic, trans-Saharan, Red Sea, and Indian Ocean routes (Lovejoy, 2011; Saleh and Wahby, 2022; Wright, 2007). The Atlantic trade, organized by European merchants, removed an estimated 8.7 million captives between 1500 and 1800, and a further 4.0 million in the nineteenth century, mainly for New World plantations.[6] The other three routes are older by several centuries and linked sub-Saharan Africa to North Africa and the Middle East (Lewis, 1990; Lovejoy, 2011; Saleh and Wahby, 2022). For 1600–1800, the combined trans-Saharan, Red Sea, and Indian Ocean flows amounted to roughly 2.3 million people (about 0.9 million in 1600–1700 and 1.3 million in 1700–1800).[7] In the nineteenth century, departures via these three routes totaled about 2.1 million (approximately 1.2 million across the Sahara, 0.5 million via the Red Sea, and 0.4 million via the Indian Ocean).[8]

Following the historical literature, we refer to the trans-Saharan, Red Sea, and Indian Ocean routes collectively as the "Arab routes." This is a label for trading systems that operated within Muslim commercial and political networks, not a claim about the ethnicity of raiders (Lovejoy, 2011). Capture was carried out mainly by the armies of African Muslim states, conducting state-organized expeditions known as *ghazwa* or *salatiya* against non-Muslim populations at their frontiers, though both Arab and non-Muslim raiders also supplied these networks (Lovejoy, 2011). Arab and Ottoman merchants and officials, however, were most prominent as financiers and transporters, and as intermediaries in destination markets, rather than as raiders themselves.

This leaves two categories of Muslim polity in our setting. The first are the historic Muslim empires and sultanates where captives were sold: the Ottoman Empire, the sultanates of Morocco, Egypt under Mamluk and then Ottoman rule, and the polities of the Arabian Peninsula and Persian Gulf. These were the destination markets for the trans-Saharan, Red Sea, and Indian Ocean routes (Lewis, 1990; Lovejoy, 2011). The second are the African Muslim states that organized frontier raiding, for example Bornu, Sennar, Dar Fur, Wadai,

[6] Lovejoy (2011) Tables 3.1 and 7.1.

[7] Lovejoy (2011), Table 3.7.

[8] The composition of exported captives also differed by route: Red Sea and trans-Saharan shipments contained relatively more women and eunuchs, destined for domestic service and court households, whereas Indian Ocean plantation demand and, especially, the Atlantic trade prioritized males.

and the Sokoto Caliphate.[9]

2.2 Slavery Under Islamic Law

Islamic jurisprudence accepted slavery as a social institution but restricted who could be lawfully enslaved. Two rules matter for our mechanism. First, free Muslims could not be enslaved. Second, conversion *after* lawful enslavement did not itself confer freedom: a convert remained a slave unless emancipated through recognized procedures.

Non-Muslim subjects of Muslim polities, primarily Christians and Jews, the "People of the Book" or *dhimmīs*, held a protected legal status: as free persons already incorporated under Muslim rule, they could not be enslaved, in exchange for paying the poll tax (*jizya*) and meeting certain civic obligations (e.g., Lewis, 1990; Lovejoy, 2011). This protection, however, did not hold outside Muslim empires, so converting to Christianity or Judaism did not offer protection on the frontier. These provisions left two lawful paths into slave status: being born into it, or being taken as a non-Muslim captive, Christian, Jewish, or otherwise, at the frontier of Muslim polities. Contemporary jurists codified these rules. Writing in 1614, Aḥmad Bābā stated: "*Whoever is captured in a condition of non-belief, it is legal to own him . . . but not he who was converted to Islam voluntarily . . . These are free Muslims, whose enslavement is not allowed in any way.*"[10] As Islamic societies relied heavily on slave labor, a direct consequence of these legal limits was large-scale importation from beyond the Islamic frontier. As Lewis (1990) puts it, "*In one of the sad paradoxes of human history, it was the humanitarian reforms brought by Islam [notably, the prohibition on enslaving free Muslims] that resulted in a vast development of the slave trade inside, and still more outside, the Islamic empire.*" Non-Muslim frontier populations became the main lawful source of new captives, who were brought into Muslim lands and socialized into Islam.

Enforcement was imperfect and uneven across time and place but nevertheless real.[11] Arab and Ottoman markets inspected captives at the point of sale, including customs checks, prayer tests, diplomatic petitions from origin polities, and a ransom market for free-born Muslims wrongly sold into bondage (Lovejoy, 2011).[12] Because enforcement operated at destination markets and not only among raiders, our mechanism does not require that every

[9] How these states came to adopt Islam, well before organizing raids of their own, is addressed in Section 2.3.

[10] Quoted in Lovejoy (2011, pp. 31–32).

[11] Enforcement was strongest during the early phases of conquest and expansion. As frontiers stabilized, it became more difficult to seize non-Muslims, and policing of Qur'anic rules weakened (Lewis, 1990).

[12] In 1391–92, the king of Bornu petitioned the Mamluk court to investigate captives and free those able to establish Muslim status. Lovejoy also documents checks at northern customs posts, including prayer tests, and kin-based identification: in one case, a captive from Bornu was freed in North Africa after his uncle identified him (Lovejoy, 2011).

raider supplying these routes was Muslim, only that the markets receiving captives were. Because recognition as a lawfully protected Muslim depended on an assessment of religious practice rather than on self-identification alone, that assessment could be, and was, contested. Historians of slavery in Muslim societies document cases in which captives who identified as Muslim were nonetheless enslaved on the grounds that their observance was judged insufficient (Oualdi, 2024).

This legal rule had a direct spatial consequence: because free Muslims could not be enslaved, neither the destination empires nor the African Muslim states that organized raiding could lawfully draw captives from within their own territory. Both depended instead on the frontier of non-Muslim territory beyond Muslim rule, well beyond the established Muslim trade corridors that connected these same polities to the wider commercial world.[13] As Lovejoy notes, "*the source of slaves for export tended to be south of the desert edge and inland from the Red Sea,*" with the slaving frontier moving steadily inland over time as nearer populations converted or were exhausted as sources (Lovejoy, 2011). It is important to note that this channel of conversion did not operate in the Atlantic trade, and conversion offered no protective value against capture along this route, which is why we use it as a placebo in our empirical analysis.

Taken together, these features motivate our central hypothesis and its testable prediction. Recognition as Muslim created credible legal protection against enslavement, raising *ex ante* incentives to convert and contributing to the diffusion of Islam along slaving frontiers. Because the credibility of protection depended on publicly observable, group-level recognition (shared rituals, mosque presence, leadership) and converts faced social and political costs when few peers converted, adoption exhibits complementarities: the return to conversion rises with the local Muslim share.

2.3 Other Channels of Diffusion of Islam in Africa

The Arab slave trade was not the only channel through which Islam reached Africa, nor the first. The historiography of Islam's diffusion in Africa identifies three broad channels: long-distance trade, military conquest, and, to a lesser extent, peaceful proselytization by Sufi orders and itinerant scholars (Levtzion and Pouwels, 2000).[14] Trade was the dominant channel of diffusion: Islam reached West African and Sahelian ruling elites primarily through commerce linking these regions to North Africa, and conversion was concentrated

[13] For example, Sokoto and Bornu pushed into the Middle Belt, Zubayr Pasha raided the Bahr al-Ghazal, and Tippu Tip's expeditions from Zanzibar reached deep into the Congo basin and Lake Tanganyika (Lovejoy, 2011).

[14] Conquest-driven Islamization includes the eleventh-century Almoravid movement and, centuries later, the Fulani jihads that founded the Sokoto Caliphate itself.

among rulers and merchant communities in commercial towns rather than imposed on the broader population (Levtzion and Pouwels, 2000; Michalopoulos et al., 2018). These commercial networks are distinct from the slave-raiding routes we study in this paper, described in Section 2.1: trade routes carried goods and people under commercial and diplomatic relationships linking Muslim merchants to African elites, whereas the raiding routes we analyze carried captives taken by force at the frontier. Military conquest, by contrast, imposed Muslim rule directly on conquered populations, though the conversion of the general population often followed gradually rather than by force.

Across these three channels, Islamization in the region was largely top-down and gradual: elites converted first, often for commercial or diplomatic advantage, and conversion spread to the broader population only slowly, in some places not until the colonial period (Levtzion and Pouwels, 2000; Michalopoulos et al., 2018).[15] This elite-driven channel of Islamization is conceptually distinct from the bottom-up channel we study in this paper: the Arab slave trade gave ordinary people, not just rulers, a direct incentive to convert for the legal protection described in Section 2.2. Trade and conquest are the two channels most likely to confound our analysis. We therefore control for distance to historical trade routes in every specification, and report results robust to excluding ethnic territories historically under direct Muslim rule.

3 Data

3.1 Exposure to the Arab Slave Trade

Historical data on slave exports by ethnic group come from Nunn and Wantchekon (2011). The dataset reports the number of slaves taken from each of the 842 ethnic groups in Murdock (1967)'s classification along the four main slave routes—the Atlantic, Red Sea, Indian Ocean, and trans-Saharan—between 1400 and 1900.

Our main measure of exposure to the Arab slave trade, *Arab*, aggregates the Red Sea, trans-Saharan, and Indian Ocean routes, which historically involved Muslim raiders. Because the Indian Ocean route also saw partial European participation, especially from the eighteenth century onward, we construct a narrower measure, *Old Arab*, that excludes the Indian Ocean route. *Atlantic*, based on the Atlantic route alone, serves as our placebo.

Following Nunn and Wantchekon (2011), and in the absence of reliable pre–slave-trade population denominators, we take the natural log of one plus the number of slaves taken

[15] The African Muslim states that later organized frontier raiding, described in Section 2.1—Bornu, Sennar, Dar Fur, Wadai, and the Sokoto Caliphate, for example—were themselves Islamized earlier through this same elite, trade-based channel, well before becoming the raiding states whose frontiers we study.

from ethnic group e divided by the historical area of its homeland. As a robustness check, we also show that results are similar when (i) using inverse-hyperbolic-sine transformations of (normalized or raw) exports as in Corno et al. (2020); (ii) trimming or winsorizing the upper tail following Teso (2019); and (iii) normalizing by ethnic population data from Murdock (1967) for the colonial-era (early–twentieth-century), compiled after the slave trade ended and available for approximately 85% of groups.

3.2 Religious Adherence, Beliefs, and Practices

We draw on six complementary sources at the individual and ethnic levels to measure religious adherence and norms.

Historical mosques To measure historical Islamic presence, we digitize a scholarly inventory of historical mosques compiled by Pradines (2022). The corpus covers sites from the 9th–10th centuries CE up to the early 20th century. The volume synthesizes archaeological, architectural, and historical evidence, and provides country-by-country mosque site lists and regional maps, organized by three macro–regions (West Africa, the Horn/Red Sea–Nile–Lake Chad corridor, and the Swahili coast). We digitize the entire volume and maps to geolocate sites and construct ethnic-territory measures of historical Islamic presence (any mosque and mosque count in a given ethnic-group territory). We are able to match 239 mosques with a century of construction to an ethnic-territory in our dataset.[16]

Contemporary share of Muslims at the ethnic-territory level We also measure ethnic-level Muslim adherence using the World Religion Database (WRD), which provides estimates of the fraction Muslim by ethnic group within country in 2005 (Johnson and Grim, 2008). We overlay the WRD ethnic units with Murdock (1967)'s homelands to obtain ethnic–homeland measures that are comparable to the slave-trade exposure data, following Michalopoulos et al. (2018).[17] We then construct the variable "*Muslim Share*", reporting the fraction of Muslim believers within a given ethnic group in 2005. Additionally, we use cross–country estimates of Muslim population shares in 1900 from Barrett et al. (2001). Country–level Muslim shares obtained by aggregating WRD to the national level are highly correlated with the 1900 series (corr. = 0.93). Figure 1 maps slave-trade exposure along the Arab routes and contemporary Muslim adherence by ethnic homeland.

Individual-level data on religiosity. Data on religious affiliations come from the Demographic and Health Surveys (DHS) and the Multiple Indicator Cluster Surveys (MICS). DHS

[16] See Appendix Figure A1 for a map of these digitized mosque locations.

[17] We perform this mapping using the 2006 version of the World Language Mapping System.

and MICS are nationally representative, individual-level surveys conducted across African countries. We assembled all DHS and MICS waves publicly available for Africa between 1994 and 2017 that record respondents' religion and self-reported ethnicity. The latter allows us to link contemporaneous outcomes to historical exposure to the slave trade at the ethnic–homeland level. We were able to match 87.3% of respondents to ethnic groups in the slave–trade dataset, yielding a final micro sample of 585,097 women aged 15–49 across 28 countries and 66 survey waves. From these sources, we construct an individual-level indicator $Muslim_i$ equal to one if the respondent i reports Islam as their religion. For falsification exercises, we similarly define $Christian_i$ and $Animist_i$ equal to one when the respondent reports Christianity or an animist/traditional faith, respectively.

Contemporary Attitudes For contemporary attitudinal outcomes, we use individual-level data from Pew's "*Tolerance and Tension: Islam and Christianity in Sub-Saharan Africa,*" a large-scale survey of religious beliefs and practices conducted across 19 sub-Saharan African countries in 2008 and 2009. The survey elicits self-reported ethnicity, enabling us to link respondents to historical ethnic-territory slave-trade exposure using the same matching procedure as the main DHS/MICS analysis. We restrict the attitudinal analysis to Muslim respondents and construct indicators spanning religious practice (women's autonomy over the veil, religious education, consumption of religious media, interfaith marriage, support for the death penalty for apostasy, Islamic knowledge, self-reported religiosity, and trust in people of other religions). We also use the survey's questions on Muslim respondents' stated beliefs in miracles, witchcraft, evil spirits, juju, and sacrifices to test whether Arab-route exposure predicts lower adherence to these beliefs, consistent with a more complete adoption of Islam and less syncretism.

Folklore motifs and concepts We draw on the folklore catalogue assembled by the Russian anthropologist Yuri Berezkin and analyzed by Michalopoulos and Xue (2021). The catalogue records the presence or absence of 2,564 mythological motifs across 958 ethnic groups worldwide, offering a window into deep-rooted cultural values encoded in oral traditions transmitted across generations. We match these data to Murdock ethnic territories and construct two classes of outcomes. First, we aggregate specific recurring motifs (themes or images) into composite indices capturing gender conservatism in folk narratives, using a large language model to flag relevant motifs and avoid authorship bias in index construction.[18] Second, using *ConceptNet*-based semantic concept frequencies identified by Michalopoulos and Xue (2021), we compute for each ethnic territory the frequency of specific social and moral concepts in motifs, including male dominance and violence, female submission, patriarchal authority, sacrifice, punishment, martyrdom, pilgrimage, faith, dietary prohibitions,

[18]Motif index construction is described in Appendix Section A2.

sin, and justice. All folklore specifications condition on 2005 Muslim share (Johnson and Grim, 2008), so that results are not mechanically driven by differences in the current share of Muslims across ethnic territories.

Joshua Project The Joshua Project[19] is a Christian missionary research organization that maintains a database of ethnographic profiles for approximately 3,600 people groups across 56 African countries. For each group, the database provides qualitative narrative descriptions of the group's religious and cultural characteristics, written by missionaries and field researchers. We collect all African entries and match them to Murdock (1967) ethnic territories using a sequential name-matching algorithm described in the Appendix (Section A1). To avoid bias in extracting information from the narratives, we use a large language model to flag instances where a description explicitly mentions: (i) deep-rooted Abrahamic or prior-Muslim identity at the time of the Arab slave trade; and (ii) strict Islamic social norms or gender conservatism. These narratives come with limitations. They are written by missionaries and field researchers whose coverage and emphasis reflect the aims of a Christian evangelization project, introducing bias into the data collection: groups of greater missionary interest may be documented in more detail. We therefore treat these qualitative narratives as a novel, corroborating source for our findings on beliefs and practices.

3.3 Female Labor Supply and Fertility

To measure contemporary gender-related behavior, we use harmonized individual-level data from IPUMS-DHS. We assemble all IPUMS-DHS samples for Africa that record respondents' self-reported ethnicity, covering 135 women's samples across 29 countries, together with the corresponding men's samples used for falsification exercises. From the women's samples we construct three outcomes for women aged 15–49: an indicator for labor force participation, equal to one if the respondent is currently working or worked in the twelve months preceding the survey; the number of children ever born; and the respondent's age at first birth. For men aged 15–49 we construct the analogous labor force participation indicator. Because the hypothesized channel operates through the form of Islam adopted, our estimates restrict attention to Muslim respondents as identified by the harmonized religion variable.

As with the DHS/MICS religion data, respondents are linked to historical slave-trade exposure through their reported ethnicity. We map each (ethnicity, country) pair onto Murdock (1967) ethnic groups using a layered crosswalk that draws, in order of precedence, on the ethnicity–ethnic-homeland crosswalk of Nunn and Wantchekon (2011), the DHS mapping constructed by Corno et al. (2020), and exact name agreement, with manual adjustments particularly for country-specific groups whose proper homeland depends on the survey country.

[19] Accessed at https://joshuaproject.net.

The resulting crosswalk matches 81% of women respondents and 80% of male respondents who report an ethnicity, and the final dataset includes 491,413 Muslim women and 125,879 Muslim men.

3.4 Historical and Geographic Covariates

Our baseline covariate set includes thirteen predetermined geographic and ethnographic characteristics, chosen because they are linked to the diffusion of Islam, to the incidence of slave raiding, or to both. We describe each group of controls and the rationale for its inclusion below. In Section 5.2 we show that the results are robust to a substantially larger covariate set and to selecting the controls algorithmically.

3.4.1 Geography and Market Access.

As discussed in Section 2.3, long-distance trade was itself a channel of Islamic diffusion in Africa, distinct from the raiding routes we study. The same corridors that carried merchants and clerics also carried slave-transport caravans (though not the raiding itself), making market access a potential determinant of both raiding exposure and conversion opportunity (Lovejoy, 2011; Wright, 2007). We therefore first control for the great-circle distance (km) from each ethnic centroid to the nearest major caravan or maritime route circa 1800, based on Brice and Kennedy (2001), together with distance to the coast and distance to Mecca (km) (Michalopoulos et al., 2016, 2018).

Second, we control for the agro-climatic variables that Michalopoulos et al. (2018) identify as geographic determinants of Muslim adherence: average agricultural suitability and within-territory land-suitability inequality. Average land quality can affect population density that made territories attractive targets for raiders (Nunn, 2008). Unequal endowments favored the trade-based economies in which Islam took root.

Third, we include terrain ruggedness, which afforded protection from slave raiders and is therefore a direct determinant of exposure (Nunn and Puga, 2012). We also include irrigation potential, which is associated with hierarchical political structures (Bentzen et al., 2017); an indicator for desert terrain, since desert-edge populations were on a key slaving frontier (Webb, 1993); and absolute latitude, a standard agro-climatic control that within Africa also indexes proximity to the Sahara–Sahel belt where Islam historically concentrated.

3.4.2 Historical Characteristics.

A large literature shows that precolonial ethnographic traits durably shape contemporary culture and economic outcomes (see Giuliano, 2026, for a review). Using (Murdock, 1967), we

control for the precolonial traits most plausibly related to both raiding exposure and religious adoption. First, political centralization, measured as the number of jurisdictional hierarchies beyond the local community; slave raiding is found to negatively affect state development and centralization (Nunn, 2008). Centralization also independently predicts contemporary development (Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013). Second, settlement patterns (an indicator for fully or semi-nomadic groups) and primary subsistence (dependence on pastoralism versus agriculture) plausibly shaped early contact with Muslim trading networks, since mobile, herding populations engaged with those networks differently than sedentary agriculturalists. Ancestral pastoralism also independently predicts cultural norms of honor, violence, and female seclusion (Becker, 2025; Cao et al., 2024). Finally, we include the density of Christian missions per km^2 during the colonial period to proxy for exposure to the principal competing conversion channel. Mission location predicts contemporary Christian adherence and long-run outcomes (Nunn, 2010).

4 Empirical Strategy

Ethnic-level analysis. We estimate the association between historical exposure to the Arab slave trade and historical and contemporary adherence to Islam. Our baseline analysis is at the ethnic–territory level:

$$Y_{ec} = \alpha_c + \beta_1 \, Arab_{ec} + \beta_2 \, Atlantic_{ec} + Z'_{ec}\gamma + \varepsilon_{ec}, \tag{1}$$

where Y_{ec} is the outcome (such as the number of historical mosques or the fraction of Muslims in ethnic group e located in country c in 2005), α_c are country fixed effects and $Arab_{ec}$ and $Atlantic_{ec}$ are the natural log of one plus slaves per km^2 taken from ethnic group e along the Arab and Atlantic routes respectively. Z_{ec} is the vector of historical and geographic covariates described in Section 3.4. The coefficient of interest, β_1, captures how within–country differences in Arab-route exposure relate to past and current Muslim adherence.

We systematically include the Atlantic route as a placebo. Organized primarily by Europeans (non-Muslims), it should not be systematically related to contemporary Muslim adherence, as explained in Section 2. Hence β_2 should be statistically indistinguishable from zero. As a robustness check, we also replace *Arab* with *Old Arab*, which excludes the Indian Ocean route, where European involvement expanded in the eighteenth–nineteenth centuries and conversion incentives were therefore weaker.

Individual-level analysis. As a complementary specification, using individual-level survey data, we estimate the following linear probability model:

$$Y_{iecs} = \alpha_c + \tau_s + \beta_1 \, Arab_{ec} + \beta_2 \, Atlantic_{ec} + Z'_{ec}\gamma + X'_{iecs}\delta + u_{iecs}, \tag{2}$$

where Y_{iecs} is an outcome for respondent i in ethnic group e, country c, and survey wave s (for example, whether the individual reports Muslim as her religion); τ_s are survey-wave fixed effects. We include a vector of individual covariates X_{iecs} comprising age, age squared, an indicator for urban residence, education fixed effects (categorical dummies), and a household wealth index constructed as the first principal component of standard asset indicators (e.g., electricity, piped water, sanitation, radio, refrigerator, car/truck). Estimates are weighted by survey sampling weights, and standard errors are clustered at the ethnic–territory level. As robustness, we (i) replace country fixed effects with district fixed effects: this specification is more demanding and reduces identifying variation because many districts contain a single ethnicity and are therefore dropped; and (ii) re-estimate on alternative samples: all linked respondents, respondents from ethnic groups with any historical Muslim presence, and respondents from groups above 50% Muslim share.

Identification assumption Slave exports between 1400–1900 are predetermined with respect to contemporary religious affiliation, so reverse causation is not a concern. The main remaining threat to identification is omitted variable bias: unobserved characteristics of an ethnic group that are correlated with its historical likelihood of being raided along the Arab routes and that independently predict its propensity to adopt Islam. We address this concern in four ways. First, we compare ethnic groups *within countries* by including country fixed effects α_c, which absorb country-level characteristics common to all ethnic groups within a country—such as colonial heritage, nationwide institutions, laws, policies, or cultural norms—that could jointly influence historical raiding and contemporary religious composition. Second, in the individual-level specification we replace country with *district* fixed effects, comparing respondents from different ethnic groups within the same district, a more demanding test of within-country variation. Third, we condition on the geographic and ancestral covariates (Z_{ec}) described in Section 3.4, including distance to historical trade routes—the leading alternative driver of Islamic diffusion identified by Michalopoulos et al. (2018)—together with land-suitability inequality and the remaining predetermined controls. Finally, we instrument for Arab-route exposure using the great-circle distance from each ethnic-territory centroid to the nearest Arab slave port, following Nunn (2008) and Corno et al. (2020). As we show below, the estimated effect of Arab-route exposure is stable across all four approaches.

5 Main Results

This section asks whether exposure to the Arab slave trade raised adoption of Islam, beyond diffusion through trade networks or Muslim political rule.

5.1 Adherence to Islam

Ethnic-level analysis. We begin at the ethnic-territory level, examining first the historical presence of Islam and then contemporary Muslim adherence. Table 1 reports estimates of equation 1 with *mosque count* (the number of historical mosques in an ethnic-group territory as defined in Murdock (1967)) as the dependent variable. Column (1) presents the base specification, column (2) includes country fixed effects and column (3) adds the full set of geographic and historical controls. We find a strong positive and statistically significant association between *Arab* exposure and the presence of historical mosques: a one-log-point increase in Arab exposure is associated with about 1 additional historical mosque in an ethnic territory. By contrast, the *Atlantic* coefficient is statistically insignificant in the fully-specified model. Reassuringly, Arab raiding from the 15th century onwards (when our Arab raiding data begins) does not predict mosques built in prior centuries (Appendix Table A1). The only robustly significant predictor of pre-15th-century mosque count is distance to historical trade routes ($\hat{\beta} \approx -0.45$, s.e. $= 0.159$), consistent with the trade-based, elite-driven diffusion of Islam prior to the slave-trade era described in Section 2.3.

We next examine the contemporary share of Muslim adherents within each ethnic-territory. Results are displayed in Table 2: exposure to the Arab slave trade is positively associated with a higher Muslim share today. Column (2) includes only country fixed effects and Column (3) adds the full set of geographic and historical controls. The point estimates are sizable: a one log point increase in Arab exposure is associated with a 9.5 percentage point higher Muslim share in Column (3). The estimates are stable once country fixed effects are included: adding the full set of geographic and historical controls in Column (3) moves the coefficient only modestly, from 0.112 to 0.095. Column (4) excludes ethnic territories historically inside (or near) a Muslim kingdom. This addresses the concern that the baseline association reflects diffusion through imperial rule—administration, legal–religious institutions, or direct political influence—rather than the hypothesized channel (group conversion in response to raiding risk to avoid being enslaved). The *Arab* coefficient is similar in magnitude, though less precisely estimated given the smaller sample, indicating that the result is not driven by political and cultural influence from Muslim kingdoms. Finally, column (5) uses as outcome an indicator for being majority Muslim. *Arab* exposure remains a strong predictor of crossing the 50% threshold (about a 10 pp effect for a log point increase in slave exports). Across columns, *Atlantic* is small and statistically indistinguishable

from zero. We can also note that the signs on the geographic covariates are consistent with Michalopoulos et al. (2018), who find that trade networks and land-suitability inequality are the main predictors of Islam in Africa: distance to 1800 routes is negative and significant, while land–suitability inequality is positive in the specifications where it is precisely estimated. Importantly, Arab raiding is a significant predictor robust to the inclusion of both these variables. We confirm that these results are robust to applying an inverse hyperbolic sine transformation to Arab slave exports (as in Corno et al. (2020)) and to winsorizing the upper tail (see Table A2). Figure A2 panel (a) shows a clear upward slope between Arab raiding and Muslim share.[20]

Individual-level analysis. We then turn to the individual-level analysis using data from the DHS and MICS. Table 3 estimates equation 2 for the probability that a respondent reports being Muslim, with country and survey-wave fixed effects, survey weights, and standard errors clustered at the ethnic-territory level (cols. 1-4). Results are qualitatively consistent with the ethnic–level estimates—*Arab* is positive and statistically significant once country fixed effects are included. *Atlantic* is negative in most specifications. We also estimate a more demanding specification that replaces country fixed effects with *district* fixed effects (Column 5), comparing individuals across ethnic groups within the same district. The *Arab* coefficient remains positive and precisely estimated at the 5% level. Figure A2 panel (b) demonstrates the relationship between historical Arab slave raiding and Muslim adherence visually with a clear upward slope.

Religious composition. Table 4 estimates equation 1 using ethnic-level Christian and Animist shares as dependent variables, to examine shifts in religious composition associated with *Arab* exposure. *Arab* exposure is negatively associated with both the Christian share and the Animist share, a pattern consistent with conversion toward Islam rather than differential population growth.

Furthermore, Table 4 is consistent with the preventive conversion mechanism. If the relationship between Arab raiding and Islam is driven instead solely through a "contact" channel – that is, association with Muslim culture through raiding – then we would expect Atlantic raiding to be positively associated with a group's Christian share. We find that the relationship between Atlantic raiding and Christian share is insignificant. This test is not without limitations. Arab raiding was often led by African Muslims, while Atlantic raiding was rarely undertaken by Christians. However, we take it as additional evidence that we are isolating the preventive conversion channel unique to Arab raiding.

[20] Clustering around $x = 0$ reflects ethnic territories with no Arab raiding. Results are consistent on both the extensive margin (a binary Arab-raiding indicator) and the intensive margin (restricting to territories with non-zero Arab raiding; Tables A3 and A4).

5.2 Robustness Checks and Sensitivity Analysis

Alternative exposure definitions: excluding the Indian Ocean. Table A5 shows that the estimates are unchanged when we use an alternative exposure measure that excludes the Indian Ocean route—*Old Arab* (Red Sea + trans-Saharan)—a route that involved European raiders and could therefore dilute our hypothesized channel. The coefficient remains positive throughout and significant in all but the empire-excluded subsample, where the point estimate remains positive but the sample decreases by $\approx 30\%$. When the Indian Ocean route is entered separately (*"Indian"*), its coefficient is also generally positive and significant, consistent with Muslim participation on that route in earlier centuries. Importantly, *Atlantic* remains close to zero in all specifications.

Additional covariates. The baseline covariates were chosen based on prior literature. The geographic controls capture the trade-network and agro-ecological determinants of Islam's diffusion, as emphasized by Michalopoulos et al. (2018), and the ethnographic controls capture precolonial characteristics plausibly related to both a group's exposure to raiding and its propensity to convert. However, the concern remains that some predetermined characteristic correlated with exposure to the Arab routes and independently conducive to the adoption of Islam is driving the association. We address this concern with three exercises, following the approach of Cao et al. (2024): (1) a "kitchen sink" specification that saturates the model with every additional predetermined covariate available to us, (2), a covariate-by-covariate exercise that pits Arab exposure against each candidate confounder individually, and (3), a double lasso procedure that removes our discretion over the controls.

Table A6 reports the kitchen sink exercise. Column (1) replicates Table 2's column (3), and columns (2)–(8) successively add seven groups of predetermined covariates. Each group is included because prior work links it to the incidence of slave raiding, to the diffusion of Islam, or to both: (i) *distance to the pre-Islamic (600 CE) trade routes*, which captures a group's integration into long-distance commerce before Islam existed (Michalopoulos et al., 2018); (ii) *land use* (the fraction of territory under pasture and under cultivation), which proxies for the underlying production structure; (iii) *additional ethnographic traits* (the practice of indigenous slavery and of polygyny prior to the slave trades), since groups that already held slaves may have been differentially integrated into the slave trade, and polygyny is associated with some forms of Islam; (iv) *alternative measures of pastoralism* (predominant pastoralism and the herding-suitability index of (Becker, 2025)), since Cao et al. (2024) and Becker (2025) shows that herding economies durably shape cultural norms; (v) *log territory area*; (vi) *distance to the nearest Arab embarkation port*, which absorbs logistical proximity to Arab commerce beyond the caravan-route distance already in the baseline; and (vii) the *tsetse suitability index* of Alsan (2015), since the tsetse fly suppressed animal husbandry, po-

litical centralization, and population density (Alsan, 2015). If the estimate is insensitive to a robust set of observables that plausibly proxy for remaining unobservables, substantial bias from selection on unobservables is less likely (Oster, 2019).[21] The *Arab* coefficient moves only modestly as the specification is saturated (between 0.071 and 0.092 across columns, with an estimate of 0.080 in the most saturated specification) and remains statistically significant at the 1% level throughout; the *Atlantic* placebo remains indistinguishable from zero in every column.

Table A7 then examines the same covariates one at a time. For each of the 25 covariates we report its standardized coefficient on Muslim share with country fixed effects only, and then re-estimate with Arab and Atlantic exposure included. Two results emerge. First, several features are strong independent predictors of contemporary Muslim share (notably distance to the 1800 trade routes, average land suitability, and distance to the nearest Arab port, consistent with the trade and agro-ecology channels documented in prior work). Second, no single covariate displaces Arab exposure: across all 25 rows the standardized *Arab* coefficient ranges from 0.113 to 0.165, compared with 0.134 in the specification with no additional covariate, and it is significant at the 1% level in every row.

Finally, we let the data select the controls. We implement the post-double-selection LASSO of Belloni et al. (2014): a LASSO of the outcome on the candidate pool and a LASSO of the treatment on the same pool are estimated separately, and the union of selected covariates enters the final regression. Taking the full baseline covariate set as the candidate pool, the procedure selects six controls at the ethnic level (Table A8 lists the candidate pools and selected controls at both the ethnic and individual levels). Re-estimating equation (1) with the LASSO-selected controls (Table A9) yields an *Arab* coefficient of 0.100 (s.e. = 0.027), significant at the 1% level and essentially identical to the baseline estimate; the result is likewise robust to excluding territories under historical Muslim rule and to using majority-Muslim status as the outcome. At the individual level, the algorithm retains nearly the entire candidate set—every control except age squared—and re-estimating equation (2) with the LASSO-selected controls (Table A10) again reproduces the baseline estimates: the *Arab* coefficient is 0.11 (s.e. = 0.06) with country fixed effects and 0.15 (s.e. = 0.07) in the more demanding district fixed-effects specification, where it is significant at the 5% level. Taken together, these exercises indicate that the association between Arab-route exposure and contemporary Muslim adherence is robust to a broad selection of covariates.

Matching. To address additional potential confounding factors, we compare adjacent ethnic territories that share a common border, thereby holding constant local geography and historical environment (e.g., market access, colonial institutions). First, we pair bordering ethnic groups (dyads) and estimate equation (1) with dyad fixed effects, clustering standard

[21] Note, we separately run a test in the spirit of Oster (2019). See below.

errors by both members of the pair and at the country level. The coefficient on *Arab* remains positive and precisely estimated, as shown in Table A11, Column (1) ($\hat{\beta} \approx 0.12$, s.e. $= 0.04$). Second, we restrict to *matched dyads* that are similar in distance to 1800 trade routes and in land-suitability inequality: the *Arab* estimate is unchanged (Column (2)). A propensity-score match that balances the full set of observables yields a slightly larger coefficient (Column (3)).

Instrumental variable approach. As a further robustness check, and following Corno et al. (2020), we instrument for Arab slave-trade exposure using the great-circle distance from each ethnic-territory centroid to the nearest port on an Arab slave route (see Appendix Table A12 for a list of included ports). The intuition is straightforward: ethnic groups located closer to Arab port embarkation points faced a higher likelihood of being targeted by Arab raiders, generating variation in actual slave exports that is plausibly more exogenous than the raw exposure measure. The first stage of this 2SLS specification, reported in Panel A of Table A13, confirms that proximity to an Arab port is a reasonably consistent negative predictor of slave exports: ethnic groups closer to a port exported more slaves along the Arab routes, conditional on controls. The instrument is statistically significant at the 5% level across all five columns.

We note that the exclusion restriction is not without concern. Proximity to Arab trade ports likely captures not only raiding risk but also access to Islamic trade networks and missionary influence. We partially address this by conditioning on distance to major caravan and maritime trade routes circa 1800 (Michalopoulos et al., 2018), which absorbs much of the variation attributable to commercial integration. Nevertheless, this is a limitation of this instrumental variable approach, and we interpret these estimates as suggestive evidence that the OLS results are not solely driven by reverse causality or selection into raiding.

The 2SLS second-stage results are reported in Panel B of Table A13. We focus on the specifications that include country fixed effects and the full set of geographic and historical controls (cols. 3–5), where the first-stage F-statistic ranges from 6.14 to 8.22, generally considered on the weaker end. In column (3), the second-stage Arab coefficient on the share Muslim is positive and significant at the 10% level ($\hat{\beta} \approx 0.982$, s.e. $= 0.566$). Restricting to ethnic territories historically outside Muslim kingdoms (Column 4), the coefficient remains positive and significant ($\hat{\beta} \approx 0.823$, s.e. $= 0.475$), consistent with the main specification. Using majority-Muslim status as the outcome (Column 5), the Arab coefficient is again positive and significant ($\hat{\beta} \approx 1.046$, s.e. $= 0.616$). The Atlantic coefficient is statistically indistinguishable from zero in all fully-specified columns. Together, the IV estimates corroborate the OLS findings.

Conley spatial correlation A potential concern with clustering standard errors at the country level is that residuals may be spatially correlated across ethnic territories in different

countries that are geographically close. To address this, we estimate our main specification of equation (1) using Conley (1999) spatial standard errors with a 1,000 km cutoff, which allows for arbitrary error correlation between observations within a 1,000 km radius while remaining agnostic about the form of that correlation. Results are reported in Table A14. The Arab coefficient is positive and significant at the 1% level across all five specifications, including in the fully-specified column with geographic and historical controls (Column 3, $\hat{\beta} \approx 0.09$, s.e. $= 0.025$) and in the column that excludes ethnic territories historically inside Muslim kingdoms (Column 4, $\hat{\beta} \approx 0.08$, s.e. $= 0.02$). The Atlantic coefficient remains statistically indistinguishable from zero in all fully-specified columns. These results confirm that the baseline findings are not an artifact of underestimated standard errors due to spatial dependence in the residuals.

Sensitivity to omitted variable bias. We assess the sensitivity of our results to potential omitted variable bias using the bounding approach of Oster (2019). This method uses the movement in the coefficient on Arab exposure and the associated gain in R^2 when moving from a restricted model (country fixed effects only) to the full specification to compute two statistics: δ^*, the degree of proportional selection on unobservables relative to observables that would be required to reduce the estimated effect to zero, and $\tilde{\beta}$, the bias-adjusted estimate under the assumption of equal selection on observables and unobservables ($\delta = 1$). Following Oster (2019), we set $R_{\max} = 1.3 \times \tilde{R}^2_{\text{long}}$, bounded at 0.99, where $\tilde{R}^2_{\text{long}}$ is the R^2 of the fully-specified regression.[22]

At the ethnic level (equation 1; Table 2, Column 3), the controlled coefficient is $\hat{\beta} = 0.10$ and the model R^2 rises from 0.57 (country fixed effects only) to 0.68 (full controls), giving $R_{\max} = 0.88$. The implied $\delta^* = 0.69$, below the conventional threshold of one. We do not view this as a significant concern, however, because the primary source of residual endogeneity in this setting is likely to operate in the *opposite* direction to what Oster's proportional selection assumption requires. As discussed in Section 1, Islamic law prohibited the enslavement of free Muslims, which means groups that had already adopted Islam prior to the Arab slave trade were less likely to be targeted by Arab raiders. Unobserved pre-existing Muslim propensity is therefore likely *negatively* correlated with our treatment variable. Were this the dominant unobservable, its omission would *attenuate* the estimated coefficient toward zero—not inflate it—as groups with low treatment (because they were already Muslim) would also have high contemporary Muslim share. The Oster test's proportional selection assumption posits that omitted variables are positively correlated with the included observables in their effect on the outcome, but this assumed direction is at odds with the most plausible source of endogeneity in our setting. However, we cannot reject the influence of omitted variables confounding the

[22] Oster statistics are computed with `psacalc`, which requires unweighted `areg` estimation. At the individual level the underlying coefficients therefore differ slightly from the weighted estimates in Table 3.

ethnic-level results based solely on the Oster test.

The individual-level results support this reading. The key difference across the two individual-level specifications is the choice of fixed effects. When we include district fixed effects (Table 3, Column 5), adding the geographic and ethnographic controls *raises* the Arab coefficient from 0.13 to 0.14, and $\delta^* = 8.02$: unobservables would need to be eight times more important than all included observables combined to zero out the effect. When we instead include country fixed effects as in the ethnic analysis (Table 3, Column 4), adding the same controls reduces the Arab coefficient from 0.13 to 0.10, and $\delta^* = 0.95$, mirroring the ethnic-level pattern.

We find the divergence between these two specifications to be meaningful. The geographic and ethnographic controls capture characteristics of environments that correlate with Arab slave trade routes. Under country fixed effects, these characteristics vary freely within countries and are positively associated with Arab trade exposure, so conditioning on them shrinks the estimated coefficient and lowers δ^*. Under district fixed effects, local geography is already absorbed at the district level. Within a given district, the residual variation in these ethnographic and geographic characteristics is negatively correlated with Arab exposure, so adding the controls raises the coefficient and yields a very large δ^*.

The district fixed effects specification is the most demanding. It absorbs local environmental confounders directly, rather than relying on controls to do so. This specification passes the Oster test. This, combined with the observation that the dominant source of endogeneity in this setting would likely *increase* our estimates, leads us to conclude that the baseline findings are unlikely to be explained by omitted variable bias.

6 Mechanisms

Section 5 shows that historical Arab slave raiding is associated with greater adherence to Islam, both historically and today. This section asks why. We first test whether preventive conversion, the adoption of Islam to protect from slave raiding, drives these results, as hypothesized in the historical literature (Section 6.1). We then examine the downstream implications of conversion under this specific threat, in terms of both the form of Islam adopted (Section 6.2) and its consequences for economic behavior today, in particular for women's labor force participation (Section 6.3).

6.1 Preventive Conversion as a Strategic Response to Risk of Enslavement

The preventive conversion hypothesis posits that populations under threat of Arab raiding adopted Islam to claim the legal protections Islamic law granted free Muslims. This hypothesis generates two predictions. First, the incentive to convert should extend to groups that were not themselves raided but faced a credible threat of raiding, such as neighbors of raided communities. Because raiding on the trans-Saharan and Red Sea routes was carried out on horseback or camelback, this incentive should also be weaker where such raiding was largely infeasible, as in land suited to the tsetse fly, where trypanosomiasis made horses and camels prohibitively costly to keep. Second, among groups that were themselves raided, the incentive to convert should be weaker where rugged terrain provided protection from raiders, whether by enabling groups to fend off raids or by letting individuals evade capture once a raid was underway. We test each of these predictions in turn.

6.1.1 Raided Neighbors and Mobility Frictions

Risk of enslavement, and therefore incentives to convert preemptively, should increase when nearby groups are raided, even for groups not directly raided. We therefore augment equation (1) with neighbors' average *Arab* exposure (mean across contiguous ethnic territories). In the full sample, neighbors' *Arab* exposure strongly predicts Muslim adherence (Table 5, Column (1); $\hat{\beta} \approx 0.143$, s.e. $= 0.038$). Results hold when restricting to ethnic territories never directly raided on the Arab routes ($Arab_{own} = 0$, in Column (2)). The Atlantic placebo remains insignificant throughout, including when limiting to groups that experienced no Atlantic raiding themselves (Column (3)).

We next ask whether such risk spillovers are weaker where movement between groups was historically costly and groups were poorly connected. Restricting to ethnic-territories whose own Arab raiding exposure is 0 ($Arab_{own} = 0$), we interact neighbors' *Arab* exposure with the Human Mobility Index (*HMI*), a preindustrial travel–time measure from Özak (2018): higher values denote harder overland movement. For a given group, the calculated HMI is the mean of the HMIs on *the lowest cost routes* from a tribe's centroid to each of its neighbors' centroids. The interaction coefficient of Arab Neighbor × HMI in Column 4 is negative, though not statistically significant. We then interact neighbors' *Arab* exposure with a dummy variable that takes a value of 1 if a group's HMI is over the 75th and 90th percentile (cols. 5 and 6 respectively). The coefficient is negative at both thresholds. At the 75th percentile it is not statistically distinguishable from zero. At the 90th percentile it is negative and significant at the 5% level. This is reinforced by two additional specifications (cols. 7 and 8): we test whether neighbors' *Arab* exposure is associated with a group's

Muslim share when the group is above and below the median HMI, limiting to groups that themselves were not raided ($Arab_{own} = 0$). The coefficient on *Arab Neighbor* is only marginally significant if a group's HMI is above the median, but is statistically significant at the 1% level, positive, and ≈ 3 times larger if a group's HMI is below the median ($\hat{\beta} \approx 0.303$, s.e. $= 0.104$). These results indicate that the spillover effect of a raided neighbor's exposure on Muslim adherence weakens where mobility between groups is difficult.

However, this spillover could still capture ordinary contact-based diffusion of Islam between adjacent groups, unrelated to risk. To distinguish these two channels, we exploit a feature specific to raiding technology: raiding on the trans-Saharan and Red Sea routes relied on horses and camels, while raiding on the Indian Ocean route did not. Trypanosomiasis, a disease carried by the tsetse fly and lethal to livestock, made mounted travel prohibitively costly on tsetse-suitable land (Law, 1976; Webb, 1993). A neighbor's exposure should therefore matter less for a group's own conversion where the group's land is more tsetse-suitable than its neighbor's, and this pattern should appear only on the trans-Saharan and Red Sea routes.

We test this using the Tsetse Suitability Index (TSI) of Alsan (2015), interacting neighbors' *Arab* exposure with a group's tsetse suitability relative to its neighbors', controlling for HMI so the interaction does not simply capture general connectivity (Table 6). Across the pooled specifications, the interaction is negative, as predicted, whether tsetse advantage is measured continuously (Column (1), $\hat{\beta} \approx -0.109$) or with indicators above the 50th and 75th percentiles, where it is also statistically significant at the 10% level (Columns (2)–(3), $\hat{\beta} \approx -0.227$ and -0.225). Splitting the sample by how raiding was carried out sharpens this pattern further: the interaction is negative and significant at the 5% level on the trans-Saharan and Red Sea routes (Column (4), $\hat{\beta} \approx -0.237$), where raiding relied on horses, while on the Indian Ocean route the point estimate is positive, and loses precision once we account for the underlying variation, which is concentrated almost entirely within a single country.[23] Note, further, that though they attenuate slightly, these results are directionally consistent to the inclusion of the climate variables that Alsan used to nonlinearly construct to the TSI (Appendix Table A15).

We cannot fully rule out that tsetse suitability operated through channels beyond raiding. The same conditions that made mounted raiding costly would also have made mounted trade and missionary activity costly, though contact carried on foot would not have been affected. Controlling for HMI helps to mitigate this concern, but we interpret this evidence as suggestive rather than as fully disentangling risk from contact. Taken together with the

[23] All treatment row values are bootstrapped. Because all specifications absorb country fixed effects, the interaction is identified by within-country variation. On the Indian Ocean route, that variation is largely in Tanzania: Tanzania accounts for 85.8 percent of the within-country sum of squares of the interaction term. We therefore interpret this comparison with caution.

mobility results above, it is consistent with the threat of raiding, rather than contact alone, contributing to the neighbor spillover.

The absence of any effect of Atlantic exposure on Christian adherence, established directly in Section 5, is itself informative: if raided groups tended to adopt the religion associated with the network that raided them, independent of any protection incentive, Atlantic raiding should predict Christian adherence much as Arab raiding predicts Muslim adherence. It does not. As a further check, we test whether a similar neighbor-spillover pattern appears for Christianity (Table 7): neighbor Atlantic exposure is not associated with a group's own Christian share, and no differential effect by HMI is precisely estimated. This spillover comparison is only weakly informative on its own, since the absence of a direct effect leaves little for a neighbor to diffuse regardless of the mechanism. However, these nulls are consistent with preventive conversion rather than a general tendency of raided groups to adopt their raiders' religion. Raiding predicts adherence, directly and through neighbors' exposure, only where conversion carried protective value.

6.1.2 Terrain Ruggedness and Natural Protection

The previous section considers how groups that were not directly raided responded to risk posed by a neighbor's raiding. This section turns to groups that were themselves exposed. For them, terrain that offered natural protection from raiders should have lowered the benefit of converting for protection.

Unlike the previous tsetse analysis, where the near-total absence of ethnic territories that were both raided and highly tsetse-suitable leaves little support for a within-group interaction,[24] raided and highly rugged territories are well represented in our sample, allowing us to test ruggedness's effect directly by interacting a group's own Arab exposure with its own terrain ruggedness. Nunn and Puga (2012) document that terrain irregularities (caves, cliffs, and small-scale elevation variability) protected populations from slave raiding, giving people places to watch for and hide from raiders and, in some documented cases, making an area effectively defensible against raiding parties altogether (Bah, 1976; Brasseur, 1968). Where terrain already offered this protection, the marginal benefit of conversion should have been lower. We therefore expect the association between Arab exposure and Muslim adherence to weaken with terrain ruggedness, using a group's own terrain–ruggedness index.

Table 8 interacts *Arab* exposure with indicators for above-median, above-80th-percentile, and above-90th-percentile ruggedness. The main *Arab* effect is positive across all three

[24] Very few ethnic territories that were raided also have tsetse suitability above the median—at higher thresholds, essentially none—which precludes a direct-effect test analogous to the one used for ruggedness below. This is consistent with tsetse analysis deterring raiders, as is documented in the literature (Law, 1976; Webb, 1993).

specifications. The interaction is close to zero at the median threshold (Column 1, $\hat{\beta} \approx 0.016$), but turns negative and significant at the 1% level at the higher thresholds—$\hat{\beta} \approx -0.099$ (s.e. $= 0.036$) at the 80th percentile and $\hat{\beta} \approx -0.088$ (s.e. $= 0.025$) at the 90th (Columns 2–3). The association between Arab exposure and Muslim adherence is therefore weaker specifically in the most rugged territories, consistent with rugged terrain substituting for conversion's protective role once terrain is rugged enough to meaningfully impede raiders.

Across these two exercises, the evidence is consistent with raiding risk, rather than contact alone, driving some of the conversion to Islam, though we do not rule out that other channels also contribute. We also also note that this section helps us circumvent an empirical challenge in the analysis: as discussed in Section 1, our measure of exposure is the number of individuals captured from a given ethnic group, which reflects actual captures, not the underlying threat of enslavement. If conversion to Islam provided effective protection, areas where conversion succeeded should display fewer recorded captures, even where the initial threat was high—so our exposure measure and our outcome of interest are jointly determined, and using realized captures as a proxy for threat likely biases our main estimates toward zero. Because the neighbor-based tests above are identified from a neighbor's captures rather than a group's own, a group's own conversion does not mechanically affect the regressor: successful conversion lowers what is recorded for the converting group, but not for its neighbor. These tests therefore isolate variation in exposure that is less contaminated by this simultaneity, and the fact that they point in the same direction as our main results is reassuring.

6.2 Implications for Beliefs and Practices

Section 6.1 provides evidence that preventive conversion explains why Arab raiding increased Muslim adherence. We now ask what form of Islam this conversion produced. Two priors are plausible *ex ante*. Conversion under coercion could produce a largely nominal adoption: higher Muslim shares, but persistent traditional beliefs and practices, with no corresponding change in religiosity or social norms. Alternatively, because conversion was self-initiated for protection, communities had a strong incentive to adopt a credible, publicly observable Muslim identity. Syncretic practice or partial adoption of Islam offered little protection against enslavement (García Novo, 2011; Oualdi, 2024, discussed below). We find evidence for the second view. Muslim populations in more heavily raided territories report stronger religiosity, more conservative gender norms, and a sharper break from traditional beliefs than religious affiliation alone would predict. We document this using three independent sources: beliefs and attitudes in survey data, ethnographic narratives and folklore data.

6.2.1 A Stricter Form of Islam

Survey Evidence: Religiosity, Gender Norms, and Traditional Beliefs We first examine individual-level attitudes using Pew's "Tolerance and Tension: Islam and Christianity in Sub-Saharan Africa" survey, restricting to Muslim respondents.[25] We estimate equation (2) with country, education, and wealth fixed effects, the geographic and historical controls used throughout, and individual-level controls, clustering standard errors two-way at the ethnic-territory level (Table 9). We stratify the sample into three panels—all Muslim respondents (Panel A), those from ethnic territories with any recorded Muslim presence in 2005 (Panel B), and those from majority-Muslim territories (Panel C).

In the full sample (Panel A), Arab exposure predicts lower female autonomy over the religious veil ($\hat{\beta} \approx -0.05$), greater consumption of religious media ($\hat{\beta} \approx 0.08$), lower acceptance of interfaith marriage ($\hat{\beta} \approx -0.06$), more support for the death penalty for apostasy ($\hat{\beta} \approx 0.04$), and more religious education ($\hat{\beta} \approx 0.05$). Effects on Islamic knowledge, self-reported religiosity, and trust in other religions are indistinguishable from zero. Restricting to territories with any Muslim presence (Panel B) leaves this pattern unchanged. In majority-Muslim territories (Panel C), all eight outcomes are significant. The strengthening of effects from Panel A to Panel C is consistent with the mechanism, though we cannot distinguish whether this reflects a more complete historical conversion process in these territories or contemporary social reinforcement of Islamic practice where Muslims form a local majority.

Atlantic exposure is largely null throughout. Distance to 1800 routes, our proxy for the trade- and merchant-based diffusion of Islam, is rarely significant and shows no consistent relationship with these measures across any panel, indicating that diffusion through trade routes does not produce the same pattern of stricter adoption as raiding-driven conversion. Appendix Table A16 confirms this, restricting the sample to ethnic territories that experienced no Arab raiding: trade- and merchant-based diffusion is again not consistently associated with a stricter form of Islam.

A complementary prediction is that more heavily raided populations moved further from a nominal or syncretic form of Islam, in which traditional beliefs persisted alongside it. Using the same survey, we construct an index of traditional beliefs and its components—belief in witchcraft, evil spirits, miracles, juju, and sacrifices to ancestors (Table A17). Arab exposure predicts a broad retreat from traditional beliefs on the summary index ($\hat{\beta} \approx -0.25$), driven by lower belief in miracles, witchcraft, and evil spirits ($\hat{\beta} \approx -0.07$, -0.07, and -0.10); coefficients on juju and sacrifices are small and statistically indistinguishable from zero. As before, neither Atlantic exposure nor distance to trade routes shows a consistent pattern.

[25] These outcomes are most interpretable for individuals who identify as Muslim; results are qualitatively robust to including the full sample.

Independent Corroboration: Ethnographic Narratives and Folklore We complement the survey evidence with two sources that do not rely on contemporary self-reported variables: qualitative ethnographic narratives and a catalog of oral traditions.

We first turn to ethnographic narratives for roughly 3,600 African people groups compiled by the Joshua Project, described in Section 3, from which we built a binary variable indicating narratives that mention conservative gender or social norms. We estimate equation (1) on these indicators, conditioning additionally on a group's contemporary Muslim share so that results are not simply a reflection of having more Muslims in a territory today. Arab exposure predicts a higher likelihood that a group's narrative describes conservative social norms or gender restrictions (Table A18, Column 2; $\hat{\beta} \approx 0.106$, s.e. $= 0.045$). Neither the Atlantic coefficient nor distance to 1800 routes is distinguishable from zero. Because the Joshua Project's narratives are not drawn from a systematic survey instrument and may reflect selective reporting by missionary authors, we treat this evidence as corroborating rather than as an independent test on its own.

We then turn to the catalog of oral traditions compiled by Michalopoulos and Xue (2021), spanning roughly 1,000 societies and linked to ethnic groups in Murdock's Ethnographic Atlas, described in Section 3, which captures motifs embedded in oral traditions that predate any contemporary survey response. We construct a *gender conservatism motif index* and estimate equation (1) using the same controls as in the main analysis, again additionally conditioning on a group's 2005 Muslim share.[26] A one log-point increase in Arab exposure raises the index by 1.5 points (Table 10, Column 1), significant at the 1% level, with a comparable magnitude inside historical Muslim kingdoms and a somewhat larger one outside them (Columns 2–3, $\hat{\beta} \approx 1.52$ and 1.92). A placebo test that replaces the index with 1,000 randomly drawn motifs as the dependent variable confirms this is not a spurious pattern: the true coefficient exceeds 99.7% of the placebo distribution (Figure A3, panel (a)).

Beyond the gender conservatism motif index, Arab exposure predicts a higher frequency of specific concepts in a group's oral traditions (Tables 11–12). Motifs of gender hierarchy and social sanction are more frequent: male dominance and violence ($\hat{\beta} \approx 1.86$, s.e. $= 0.79$), female submission ($\hat{\beta} \approx 2.52$, s.e. $= 0.74$), sacrifice ($\hat{\beta} \approx 0.85$, s.e. $= 0.24$), patriarchal authority ($\hat{\beta} \approx 0.71$, s.e. $= 0.23$), and punishment ($\hat{\beta} \approx 0.90$, s.e. $= 0.34$). Motifs tied specifically to Islamic practice are also more frequent: martyrdom ($\hat{\beta} \approx 0.36$, s.e. $= 0.13$), pilgrimage ($\hat{\beta} \approx 0.42$, s.e. $= 0.09$), faith ($\hat{\beta} \approx 0.15$, s.e. $= 0.07$), pork prohibition ($\hat{\beta} \approx 0.69$, s.e. $= 0.23$), alcohol prohibition ($\hat{\beta} \approx 0.41$, s.e. $= 0.20$), sin ($\hat{\beta} \approx 0.45$, s.e. $= 0.16$), and justice ($\hat{\beta} \approx 0.35$, s.e. $= 0.10$). With the exception of the coefficient on sin, Atlantic coefficients on these twelve concepts are indistinguishable from zero at conventional levels and much smaller than the corresponding Arab coefficients. Distance to 1800 routes shows no consistent association with any of them.

[26] Motif index construction is described in Appendix Section A2.

Because folklore motifs accumulate over generations and are not shaped by the immediate social or political context the way contemporary survey responses can be, this evidence shows that the stricter form of Islam we document is also encoded in oral traditions, indicating that the mechanism operated historically.

Across the survey, ethnographic, and folklore evidence, Muslim populations from more heavily raided territories show a stricter adoption of Islam: stronger religiosity, more conservative gender norms, and weaker adherence to traditional belief and practice. These sources differ in construction, timing, and susceptibility to bias yet all three show the same pattern. We do not find similar results under trade-based diffusion, reinforcing that these are specific to conversion under the threat of raiding and consistent with our broader argument that the channel through which Islam diffuses shapes the form it takes.

It is worth highlighting that the implications of this finding — that the preventive conversion mechanism incentivized adopting a more conservative and stricter form of Islam — are substantiated by the historical literature. Sources make explicit that how observably faithful a group appeared to be was a determinant of whether they were deemed acceptable to take as slaves: for example, Aḥmad Bābā devotes much of his *Miʿrāj al-Ṣʿuūd*, a 1615 legal treatise (*fatwa*), to identifying which groups are and are not appropriate to enslave based on the extent of their demonstrated belief or unbelief in Islam (García Novo, 2011). Historians further identify instances where Muslims were nonetheless enslaved on the grounds that their observance was judged insufficient (Oualdi, 2024).

Our finding that the preventive conversion channel differed from trade-based diffusion is also consistent with the literature. When Islam spread through top-down religious diffusion, often led by political or economic elites (Insoll, 2003; Lapidus, 2002; Levtzion, 1979; Levtzion and Pouwels, 2000), the incentive for demonstrated conservatism was weaker or did not exist at all. As an example, in kingdoms like Malal and Gao, the king and his court converted while the population did not, and Islam was blended with local traditions rather than replacing them (Levtzion, 1979).

6.2.2 Interiorized Racial Hierarchy

Preventive conversion may have produced a second, distinct byproduct: even as religion, not race, determined who could licitly be enslaved, a separate racial-status hierarchy emerged among Muslims themselves. Islamic doctrine treats all believers as equal regardless of race (Lewis, 1990). Enslavability in this setting turned on religion, not race. Yet in practice, a status hierarchy emerged alongside this religious criterion: blackness became associated with slave status, and Arab identity with freedom and religious prestige (Lewis, 1990). This left black African converts in an ambiguous position—Muslim by religion, yet racially associated with the enslaved—and their claim to protection correspondingly more exposed to challenge.

Persistent questioning of a convert's authenticity may itself have reinforced stricter Islamic practice, consistent with theoretical work showing that external discrimination against a religious community tends to increase its internal strictness and cohesion (Carvalho and Sacks, 2021). We do not test this reinforcement channel directly here, and treat the racial-status hierarchy documented below as a separate, parallel byproduct of preventive conversion.

This logic generates two predictions. First, oral traditions of more heavily raided groups should show stronger positive associations with Arab or lighter-skinned identity. Second, because this status ambiguity was specific to populations racially distinct from the Arab and Muslim elite, the pattern should be concentrated among groups historically outside Muslim kingdoms, where converts' racial distance from that elite was greatest, and largely absent among groups historically inside them.

We also use the catalog of oral traditions compiled by Michalopoulos and Xue (2021) to construct a *white-bias motif index*, following the same procedure as the gender conservatism index. Consistent with the first prediction, Arab exposure is positively associated with this index in the full sample (Table 10, Column 4; $\hat{\beta} \approx 0.07$, s.e. $= 0.04$). The effect is concentrated among ethnic territories historically outside Muslim kingdoms (Column 6, $\hat{\beta} \approx 0.20$, s.e. $= 0.06$) and negligible within them (Column 5).

A placebo test analogous to that used for the gender conservatism index confirms this is not a spurious pattern, though less decisively: the true coefficient exceeds 87% of coefficients from regressions using randomly selected motifs (Figure A3, panel (b)), a markedly weaker margin than the gender-conservatism placebo (99.7%).

Given this, and that the result rests on a single measure and a single placebo check, we interpret this result as indicative evidence of a second, independent byproduct of preventive conversion: exposure to Arab slave raiding shaped not only the depth of Islamic practice but also who was perceived as socially superior.

6.3 Implications for Economic Behavior: Effect on Female Labor Force Participation, Fertility and Age at First Birth

Section 6.2 shows that Arab raiding produced a stricter, more conservative form of Islam, including more conservative gender norms. This section asks whether those norms carry consequences beyond stated attitudes and oral tradition, for economic behavior and development. Specifically, we study whether they are visible in women's actions in the labor market. We test this using the IPUMS-DHS data described in Section 3, estimating equation (2) on Muslim women aged 15–49 with the full set of geographic and historical controls, and individual controls. Becker (2025) finds that pastoral economies are themselves associated with female seclusion and lower female labor participation, and Alesina et al. (2013) find

that plough agriculture is associated with lower female participation. Because both could conceivably be correlated with Arab raiding, we add ancestral pastoralism controls (Becker (2025)'s herding measure and pastoral and cultivation land suitability) and, borrowing from Alesina et al. (2013), settlement-pattern, large-animal, and plough controls.

Table 13 presents the results. Muslim women from ethnic groups more exposed to Arab raiding are significantly less likely to participate in the labor force: the coefficient is stable as controls are added, reaching -0.028 (s.e. $= 0.011$) in the most demanding specification (Panel A, Column 3), or a 2.8 percentage-point decline in participation per log point of Arab exposure against a sample mean of 59%. Fertility outcomes move in the same direction: Arab exposure predicts more children ever born (Panel B, Column 3, $\hat{\beta} \approx 0.074$, s.e. $= 0.030$) and a younger age at first birth (Panel C, Column 3, $\hat{\beta} \approx -0.202$, s.e. $= 0.076$). This result is robust to controlling for Distance to 1800 Routes. Strikingly, Distance to 1800 Routes on its own predicts the opposite pattern—higher female labor force participation, fewer children born, and older age at first birth. Together with the results in Section 6.2, this indicates that the channel through which a religion diffuses shapes the practices that emerge from it, and those practices in turn shape economic behavior.

These estimates also speak to Teso (2019). Reassuringly, we replicate his central finding on this Muslim sample: Atlantic exposure predicts *higher* female participation and *later* first birth among these women, consistent with his account of the Atlantic trade raising female labor participation through the demographic shock to male-female sex ratios (Table 13). Teso (2019), however, raises a natural concern about our own estimates: the Arab trades were also a demographic shock, and there is some evidence that Arab raiders favored women. Therefore, the same sex-ratio logic would leave raided groups with male-biased populations and could depress female labor participation today. Appendix Table A19 shows that this is not the case. Estimating on the full sample of women of all religions and interacting Arab exposure with Muslim identity, non-Muslim women descended from the same raided groups show no decrease in labor force participation ($\hat{\beta} \approx 0.003$, s.e. $= 0.034$). However, Muslim women do (total effect -0.052, s.e. $= 0.009$) and the difference between the two effects is statistically significant ($\hat{\beta} \approx -0.056$, s.e. $= 0.033$). A demographic shock centuries ago would have depressed labor force participation for all women descended from raided groups, not only Muslim ones. The effect we find is specific to Muslim women.

If the preventive conversion mechanism led to conservative gender norms, the effect of Arab raiding on labor force participation should be absent for men. Table A20 repeats the analysis for Muslim men: the Arab coefficient is indistinguishable from zero in all specifications (Column 3, $\hat{\beta} \approx 0.004$, s.e. $= 0.006$). Figure A4 displays the partial correlations underlying these two baseline specifications, for women and men separately. Table A21 sharpens this comparison by pooling women and men and interacting Arab exposure with a female indicator, absorbing survey-by-gender fixed effects so that the interaction is identified

from gender gaps varying across ethnic groups within a survey. The male-female participation gap widens significantly with Arab exposure (Column 1, $\hat{\beta} \approx -0.069$, s.e. $= 0.015$), with a total effect for women of -0.043 (s.e. $= 0.008$). Column (2) confirms that this relationship is not solely driven by the relationship between ancestral pastoralism and female labor force participation identified by Becker (2025): pastoralist descent does predict a wider gender gap ($\hat{\beta} \approx -0.032$, s.e. $= 0.010$), consistent with her finding, but the Arab interaction remains similar in magnitude ($\hat{\beta} \approx -0.041$, s.e. $= 0.011$).

Arab raiding exposure predicts gender conservatism in stated attitudes and folklore, and it also predicts behavior: Muslim women descended from raided groups today work less, become mothers earlier, and have more children, while the labor supply of their male counterparts is unaffected.

7 Conclusion

This paper asks whether the Arab slave trade contributed to the spread of Islam in Africa through preventive conversion: because Islamic law prohibited the enslavement of free Muslims, adopting Islam offered credible legal protection against capture, raising the incentive to convert in areas exposed to Arab raiding. Using newly digitized historical mosque records, ethnic-territory slave-export data along the three Arab slave routes, and survey data, we show that exposure to the Arab slave trade is strongly and robustly associated with both historical Islamic presence and higher Muslim adherence today, with corresponding declines in Christian and animist shares consistent with conversion rather than differential population growth. There is no such effect on the Atlantic route, where Islamic legal protections were irrelevant. These results are robust to excluding historically Muslim-ruled territories, to dyad and propensity-score matching, to Conley spatial standard errors, and to an instrumental-variables strategy based on distance to Arab slave ports.

Two tests focus on the *risk* posed by raiding (as opposed to its actualization), supporting the preventive conversion hypothesis as the main mechanism driving the results. First, neighbor exposure raises a group's own Muslim adherence even if the groups was never raided, and this spillover attenuates where mobility between a group and its neighbors was costly and where terrain or tsetse-borne disease impeded mounted raiders. Second, among groups that were themselves raided, the association between exposure and adherence weakens in more rugged terrain where groups were better able to fend off raiders – and thus the incentive to convert preventively was lower.

We also show that conversion under this threat produced a qualitatively different form of Islam than diffusion through trade. Muslim respondents from more heavily raided territories report stronger religiosity, more conservative gender norms, and a sharper retreat from tradi-

tional African beliefs. Independent evidence from folklore data and missionary ethnographies corroborates this pattern. Distance to historical trade routes predicts none of these patterns, indicating that the *channel* through which Islam spread, not merely its presence, shaped the form it took. The same folklore data point to a further, distinct byproduct: despite Islamic doctrine's rejection of racial distinction, oral traditions of more heavily raided groups, particularly outside historic Muslim kingdoms, show more favorable associations with lighter skin and Arab identity—suggestive evidence that preventive conversion carried an emergent racial-status hierarchy alongside its religious effects.

Finally, we test whether this stricter form of Islam has consequences for economic behavior. We find that Muslim women from more heavily raided ethnic groups are less likely to participate in the labor force, have more children, and become mothers earlier than women from other Muslim populations. These effects are absent for men and specific to Muslim women within raided groups, ruling out a purely demographic explanation tied to historical sex-ratio shocks. Trade-based diffusion predicts the opposite pattern on every outcome.

This paper documents a channel of religious diffusion largely absent from the literature: preventive conversion driven by the threat of enslavement, in which communities adopted a religion collectively to secure legal protection. We show that this channel shaped the form Islam took, producing a stricter, more conservative form of Islam, and that this stricter practice carries real consequences for economic behavior: women from more heavily raided communities work less and have more children today. More broadly, the way a religion diffuses shapes the practices that take root within it, and these practices have lasting consequences for cultural norms and economic outcomes.

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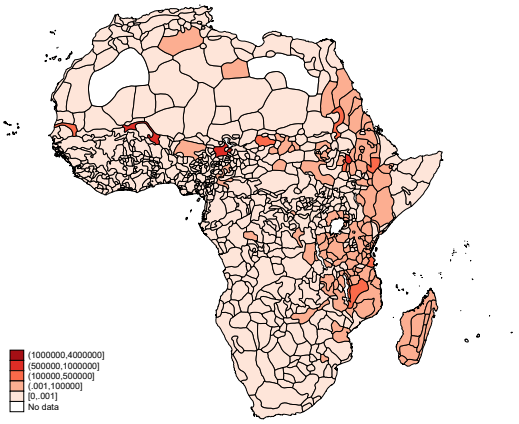
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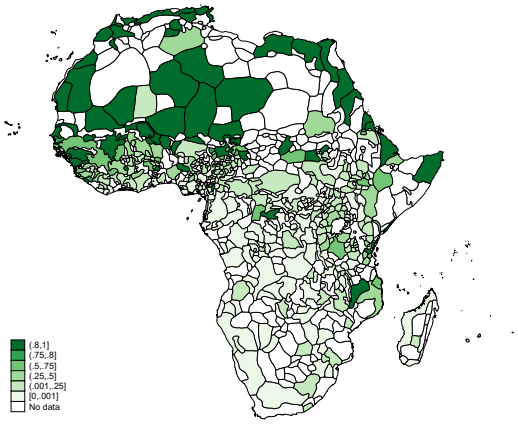
Figures

Figure 1: Arab–Route Exposure and Muslim Share by Ethnic Territory

(a) Arab routes

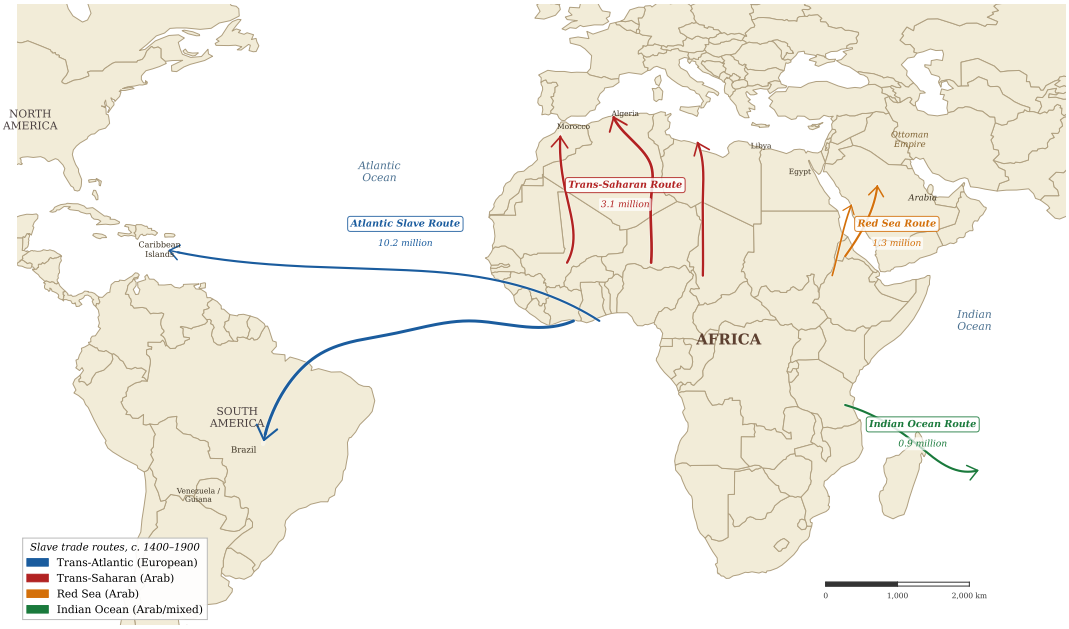


(b) Share of Muslims (2005)



Notes: Panel (a) shades ethnic–territory exposure to the Arab slave trade (area-normalized exports); darker tones denote higher exposure. Panel (b) shades the ethnic–territory fraction Muslim in 2005 (Johnson and Grim, 2008). Ethnic polygons follow Murdock (1959). See Section 3 for construction details.

Figure 2: African Slave Trade Routes, c. 1400-1900



Notes: Arrows indicate the principal Arab/Red Sea, Trans-Saharan, Atlantic, and Indian Ocean routes. Sources: Nunn and Wantchekon (2011).

Tables

Table 1: Arab Slave Trade Exposure and Historical Mosque Presence

	Historical Mosques		
	(1)	(2)	(3)
Arab	1.058***	0.936**	1.016**
	(0.375)	(0.354)	(0.384)
Atlantic	0.213*	-0.028	-0.034
	(0.108)	(0.122)	(0.138)
Distance to trade			-1.074**
			(0.434)
Land inequality			0.068
			(0.096)
Observations	629	629	573
Clusters	48	48	48
R^2	0.132	0.237	0.258
Country FE	No	Yes	Yes
Geo. Controls	No	No	Yes
Historic Controls	No	No	Yes

Notes: OLS estimates of equation 1. The dependent variable is the total number of mosques in a given ethnic-group territory, digitized from Pradines (2022). The primary explanatory variables are Arab-route exposures—log (1 + slaves/km^2). The included mosques are limited to those built after the 14th century, as this is when Arab-route exposure data begins. Country fixed effects are included in columns (2)–(3). Geographic controls (col. 3) are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls (col. 3) are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization (jurisdiction beyond the local community), and the density of Christian missions during the colonial period. Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2: Arab Slave Trade Exposure and Muslim Share (2005)

	% Muslim				Maj. Muslim
	(1)	(2)	(3)	(4) Muslim Empire = 0	(5)
Arab	0.194***	0.112***	0.095***	0.077*	0.104***
	(0.044)	(0.034)	(0.027)	(0.044)	(0.027)
Atlantic	0.021	-0.032	-0.013	-0.036	-0.036
	(0.026)	(0.022)	(0.026)	(0.028)	(0.032)
Distance to trade			-0.344***	-0.247**	-0.305***
			(0.097)	(0.098)	(0.100)
Land inequality			0.038**	0.022	0.045**
			(0.018)	(0.018)	(0.019)
Observations	629	629	573	390	573
Clusters	48	48	48	42	48
R^2	0.051	0.511	0.639	0.505	0.539
Country FE	No	Yes	Yes	Yes	Yes
Geo. Controls	No	No	Yes	Yes	Yes
Historic Controls	No	No	Yes	Yes	Yes

Notes: OLS estimates of equation (1). The unit of observation is an ethnic–territory group. The primary explanatory variables are Arab-route exposures—log (1 + slaves/km^2). *Distance to trade* captures the great–circle distance from the ethnic centroid to the nearest major caravan or maritime route circa 1800 (Brice and Kennedy, 2001). *Land inequality* captures within–territory land–suitability inequality for each Murdock (1959) ethnic territory. In columns (1)–(4), the dependent variable is the ethnic–territory share that is Muslim in WRD (2005); in column (5) it is an indicator equal to one if the ethnic group is majority Muslim. Country fixed effects are included in columns (2)–(5). Geographic controls are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization, and the density of Christian missions during the colonial period. Column (4) excludes ethnic territories inside the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Robust standard errors clustered at the country level in parentheses. Results are robust to limiting to controls selected through a double LASSO procedure detailed in Table A8 and presented in Table A9. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Arab Slave Trade Exposure and Muslim Affiliation (individual-level analysis)

	Dependent variable: Muslim				
	(1)	(2)	(3)	(4)	(5)
Arab	0.125*	0.246***	0.234***	0.101*	0.141**
	(0.073)	(0.085)	(0.077)	(0.058)	(0.063)
Atlantic	-0.128***	-0.136***	-0.105***	-0.027	-0.070**
	(0.038)	(0.033)	(0.032)	(0.032)	(0.029)
Observations	580,806	580,806	499,251	412,129	412,043
Clusters	173	173	172	121	121
R^2	0.217	0.510	0.526	0.610	0.593
Wave FE	Yes	Yes	Yes	Yes	Yes
Country FE	No	Yes	Yes	Yes	No
District FE	No	No	No	No	Yes
Geographic Controls	No	No	No	Yes	Yes
Historic Controls	No	No	No	Yes	Yes
Individual Controls	No	No	Yes	Yes	Yes

Notes: Linear probability model estimates of equation (2). The unit of observation is an individual. The dependent variable is an indicator equal to one if the respondent reports their religion as Islam. Wave fixed effects are included throughout. Columns (1)–(4) sequentially add country fixed effects, geographic and historical controls, and individual covariates (age, age^2, urban indicator, education and occupation dummies, and a household wealth index). Individual data is from all available DHS and MICS surveys that elicit religion and ethnicity (1994-2017). Column (5) replaces country fixed effects with district fixed effects. Sampling weights are used throughout. Standard errors are two-way clustered by ethnicity and district. Results are robust to limiting to controls selected through a double LASSO procedure detailed in Table A8. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Arab Slave Trade Exposure and Prevalence of Other Religions (2005)

	(1) Christian Share	(2) Animist Share	(3) Other Religion
Arab	-0.069***	-0.048***	-0.001***
	(0.023)	(0.017)	(0.000)
Atlantic	0.016	-0.008	-0.000
	(0.020)	(0.040)	(0.001)
Distance to 1800 Routes	0.234**	0.094	0.002
	(0.096)	(0.087)	(0.001)
Observations	488	488	488
Clusters	48	48	48
R^2	0.629	0.348	0.454
Country FE	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes
Historic Controls	Yes	Yes	Yes

Notes: OLS estimates of equation 1. The unit of observation is an ethnic–territory group. The dependent variable is the ethnic–territory share Christian (col. 1), share Animist (col. 2), and share Other Religion (col. 3), from WRD (2005). Country fixed effects, geographic and historical controls are included in all columns. Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Neighbor Arab Slave Trade Exposure and Muslim Share (2005), Heterogeneity by Mobility Costs (HMI)

	Dependent Variable: % Muslim							
	Baseline			HMI Interactions			HMI Median Split	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Arab = 0	Atlantic = 0	Arab = 0	Arab = 0	Arab = 0	Above 50 Arab = 0	Below 50 Arab = 0
Arab	0.112***		0.098**					
	(0.030)		(0.046)					
Atlantic	-0.020	-0.012		-0.014	-0.015	-0.013	0.013	-0.041
	(0.027)	(0.029)		(0.029)	(0.029)	(0.031)	(0.038)	(0.050)
Arab Neighbor	0.143***	0.157***		0.378**	0.176***	0.168***	0.102*	0.303***
	(0.038)	(0.046)		(0.161)	(0.047)	(0.046)	(0.056)	(0.104)
Atlantic Neighbor	-0.045		0.113					
	(0.051)		(0.113)					
HMI				0.001**				
				(0.000)				
Arab Neighbor × HMI				-0.004				
				(0.003)				
HMI (75pt) × Arab Nbr.					-0.212			
					(0.153)			
HMI (90pt) × Arab Nbr.						-0.332**		
						(0.156)		
Number of Obs	528	430	337	430	430	430	206	223
Number of Clusters	46	42	41	42	42	42	39	30
R^2	0.60	0.64	0.60	0.65	0.64	0.64	0.76	0.39
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Historic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates. Unit of observation is an ethnic–territory group. Dependent variable is ethnic–territory Muslim share (WRD, 2005). Neighbor Arab-route exposures are the mean exposure of all neighbors for each ethnic group. The Human Mobility Index (HMI) is a preindustrial travel-time measure from Ozak (2018), calculated as the mean sum of HMIs from a tribe's centroid to each neighbor's centroid. *HMI (75pt)* and *HMI (90pt)* are dummies for groups whose HMI exceeds the 75th and 90th percentiles, respectively. Columns (7) and (8) split the sample at the median HMI (above = less accessible; below = more accessible). Column (3) acts as a placebo demonstrating no spillover effects of Atlantic slave exports on Muslim share, including when a group's own Atlantic slave exports are 0. All specifications include country fixed effects and the full set of geographic and historical controls described in Section 3.4. Robust standard errors clustered at the country level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Neighbor Slave Trade Exposure and Muslim Share (2005) - Heterogeneity by Relative Tsetse Suitability

	% Muslim				
	(1)	(2)	(3)	(4)	(5)
		All Arab routes		Sah. + Red Sea (cavalry)	Indian Ocean (no cavalry)
	Diff	Diff > p50	Diff > p75	Diff	Diff
Neighbor Exposure	0.171***	0.313**	0.285**	0.209***	-1.133
	(0.058)	(0.088)	(0.086)	(0.036)	(0.317)
Tsetse Advantage	0.018			0.052**	-0.018
	(0.022)			(0.024)	(0.014)
Tsetse Adv. × Exposure	-0.109	-0.227*	-0.225*	-0.237**	2.337
	(0.098)	(0.092)	(0.092)	(0.093)	(0.551)
Neighbor Tsetse	-0.014	-0.021	-0.016		
	(0.027)	(0.026)	(0.026)		
Atlantic	-0.013	-0.011	-0.011	-0.028	-0.008
	(0.029)	(0.027)	(0.028)	(0.029)	(0.027)
Human Mobility Index	0.001*	0.001**	0.001*	0.001**	0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Distance to 1800 routes	-0.202**	-0.197**	-0.207**	-0.263***	-0.249***
	(0.090)	(0.089)	(0.090)	(0.080)	(0.075)
Observations	428	430	430	481	466
Exposed observations	131	131	131	89	53
Clusters	42	42	42	44	42
R^2	0.640	0.649	0.649	0.582	0.637
Country FE & full controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates on the full continental sample. The unit of observation is an ethnic–territory group and the dependent variable is the ethnic–territory share Muslim (WRD, 2005). *Neighbor Exposure* is the mean slave trade exposure of neighboring territories—log (1 + slaves/km^2)—for the route(s) in the column heading. *Tsetse Advantage* is the group's tsetse suitability Alsan (2015) minus the neighborhood mean: raiders operating next door are impeded by the suitability gradient at the border, and differencing removes the smooth continental gradient. Columns (1)–(3) pool the Arab routes, using the continuous advantage and indicators for its 50th and 75th percentile (indicator level terms estimated but not shown). Columns (4)–(5) split by raiding technology: the trans-Saharan and Red Sea routes used mounted raiders, the Indian Ocean route porters on foot. Each column drops territories with own-group exposure to the route(s) in question; columns (1)–(3) also control for mean neighbor suitability. All specifications include country fixed effects and the full set of geographic and historical controls described in Section 3.4. Standard errors clustered at the country level in parentheses throughout. For the three treatment rows, stars are assigned from wild-cluster bootstrap p-values (boottest with 999 replications) and so need not agree with the ratio of the coefficient to its standard error; remaining stars are analytic. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Neighbor Atlantic Slave Trade Exposure and Christian Share (2005), Heterogeneity by Mobility Costs (HMI)

	Dependent Variable: % Christian			
	Baseline		HMI Median Split	
	(1)	(2)	(3)	(4)
		Atlantic = 0	Above 50 Atlantic = 0	Below 50 Atlantic = 0
Atlantic	0.031			
	(0.023)			
Atlantic Neighbor	0.012	-0.092	-0.334	0.088
	(0.047)	(0.141)	(0.262)	(0.188)
Arab	-0.068***	-0.081**	0.029	-0.070
	(0.025)	(0.040)	(0.084)	(0.059)
Arab Neighbor	-0.143***			
	(0.047)			
Number of Obs	451	289	137	151
Number of Clusters	46	41	36	26
R^2	0.60	0.56	0.58	0.52
Country FE	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes
Historic Controls	Yes	Yes	Yes	Yes

Notes: OLS estimates. This table mirrors Table 5 with the Atlantic slave trade in place of the Arab slave trade and Christian share in place of Muslim share. Unit of observation is an ethnic–territory group. Dependent variable is ethnic–territory Christian share (WRD, 2005). Neighbor Atlantic-route exposures are the mean exposure of all neighbors for each ethnic group. Columns (2)–(4) restrict to groups with zero own Atlantic slave exports and control for own Arab exposure, mirroring the treatment of the other trade in Table 5. Columns (3) and (4) split the sample at the median Human Mobility Index (HMI), a preindustrial travel-time measure from Özak (2018) calculated as the mean sum of HMIs from a tribe's centroid to each neighbor's centroid (above = less accessible; below = more accessible). Two blocks of Table 5 have no counterpart here. Its column (3) is the Atlantic placebo, which is the subject of this table. Its columns (4)–(6), which interact neighbor exposure with HMI, are omitted because the interaction cannot be identified on the Atlantic trade: the Atlantic trade is coastal and West-Central African while the top HMI decile is Saharan, so above the 90th percentile the two conditions almost never co-occur. All specifications include country fixed effects and the full set of geographic and historical controls described in Section 3.4. Robust standard errors clustered at the country level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Arab Slave Trade Exposure and Muslim Share (2005) - Heterogeneity by Ruggedness

	% Muslim		
	(1) 50pt	(2) 80pt	(3) 90pt
Arab	0.065***	0.083***	0.076***
	(0.024)	(0.020)	(0.018)
Ruggedness × Arab	0.016	-0.099***	-0.088***
	(0.035)	(0.036)	(0.025)
Ruggedness	0.047	0.026	-0.025
	(0.039)	(0.047)	(0.056)
Atlantic	-0.010	-0.008	-0.009
	(0.026)	(0.026)	(0.027)
Distance to 1800 routes	-0.337***	-0.331***	-0.329***
	(0.099)	(0.105)	(0.106)
Land inequality	0.038**	0.039**	0.038**
	(0.017)	(0.019)	(0.018)
Observations	573	573	573
Clusters	48	48	48
R^2	0.644	0.644	0.644
Country FE	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes
Historical Controls	Yes	Yes	Yes

Notes: OLS estimates of equation 1. The unit of observation is an ethnic–territory group. The dependent variable is the ethnic–territory share Muslim (WRD, 2005). *Ruggedness* is a measure of ethnic-territory terrain ruggedness (Nunn and Puga, 2012). Column (1) uses a binary for above-the-median ruggedness, column (2) uses above the 80th percentile, and column (3) uses above the 90th percentile. Columns (1)–(3) additionally include Atlantic exposure. All specifications include country fixed effects and the full set of geographic and historical controls described in Section 3.4. Robust standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 9: Arab Slave Trade Exposure and Religious Attitudes (individual-level analysis)

	(1) Women Dec. Veil	(2) Relig. Educ.	(3) Relig. Media	(4) Interfaith Marriage	(5) Death Apostasy	(6) Know Islam	(7) Very Relig.	(8) Trust Other Relig.
Panel A: Full Sample								
Arab	-0.050**	0.054*	0.075***	-0.060***	0.043**	-0.024	0.006	-0.012
	(0.023)	(0.030)	(0.027)	(0.022)	(0.019)	(0.028)	(0.021)	(0.020)
Atlantic	0.015	-0.018	-0.009	-0.033	0.023	0.038**	0.010	-0.023
	(0.024)	(0.027)	(0.014)	(0.027)	(0.026)	(0.017)	(0.021)	(0.023)
Distance to 1800 Routes	-0.235***	0.046	0.101	0.013	0.135	-0.007	0.020	-0.146
	(0.077)	(0.079)	(0.096)	(0.083)	(0.083)	(0.081)	(0.048)	(0.097)
Observations	3,580	2,581	3,837	3,786	3,713	3,840	3,817	3,630
Clusters	136	125	139	139	136	138	138	136
R^2	0.067	0.067	0.060	0.094	0.050	0.149	0.134	0.050
Panel B: $> 0\%$ Muslim								
Arab	-0.055**	0.075**	0.072**	-0.034*	0.043**	-0.030	0.018	-0.017
	(0.024)	(0.030)	(0.028)	(0.019)	(0.020)	(0.029)	(0.020)	(0.021)
Atlantic	0.020	-0.012	-0.019	-0.021	-0.006	0.048**	0.010	-0.018
	(0.026)	(0.034)	(0.014)	(0.024)	(0.027)	(0.019)	(0.023)	(0.024)
Distance to 1800 Routes	-0.222**	0.105	0.016	0.153	0.195*	-0.083	0.039	-0.150
	(0.102)	(0.103)	(0.121)	(0.113)	(0.104)	(0.109)	(0.071)	(0.117)
Observations	3,434	2,469	3,682	3,637	3,568	3,685	3,663	3,499
Clusters	104	98	106	106	105	106	105	103
R^2	0.066	0.069	0.063	0.099	0.051	0.147	0.140	0.050
Panel C: Majority Muslims								
Arab	-0.097***	0.122***	0.090***	-0.075***	0.066***	0.050*	0.044*	-0.073***
	(0.036)	(0.038)	(0.023)	(0.020)	(0.024)	(0.026)	(0.022)	(0.018)
Atlantic	0.008	0.030	-0.004	-0.078***	0.054**	0.091***	-0.014	-0.009
	(0.023)	(0.041)	(0.017)	(0.025)	(0.026)	(0.030)	(0.029)	(0.028)
Distance to 1800 Routes	-0.617	-0.588	-0.173	-0.014	0.621***	0.276	-0.049	-0.825***
	(0.408)	(0.529)	(0.252)	(0.231)	(0.201)	(0.310)	(0.276)	(0.275)
Observations	2,098	1,533	2,276	2,250	2,208	2,283	2,267	2,180
Clusters	37	36	37	37	37	37	37	36
R^2	0.064	0.086	0.048	0.119	0.074	0.123	0.178	0.060
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Historic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Linear probability model (OLS) estimates of equation (2). The unit of observation is an individual. The dependent variable is indicated by each column header. The sample is restricted to Muslim respondents. Panel A utilizes the full sample, Panel B only ethnic-territories with non-zero Muslim adherence, and Panel C only ethnic-territories with $> 50\%$ Muslim adherence (WRD, 2005). Individual data is from Pew's "Tolerance and Tension: Islam and Christianity in Sub-Saharan Africa" (2020 update). All specifications include country and survey-wave fixed effects, geographic and historical controls, and individual controls. Sampling weights are used throughout. Robust standard errors are clustered at the ethnic-territory level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10: Arab Slave Trade Exposure and Motif Prevalence in Folklore

	Gender Conservatism Motif			White-Bias Motif		
	(1) Full	(2) Empire=1	(3) Empire=0	(4) Full	(5) Empire=1	(6) Empire=0
Arab	1.528***	1.516***	1.919**	0.071**	-0.003	0.200***
	(0.253)	(0.280)	(0.738)	(0.035)	(0.007)	(0.058)
Atlantic	0.067	0.636**	-0.030	0.002	0.007	-0.014
	(0.088)	(0.282)	(0.096)	(0.006)	(0.008)	(0.011)
Distance to Trade	0.519	1.052	-0.212	-0.044	-0.238	-0.083
	(0.499)	(1.587)	(0.464)	(0.035)	(0.232)	(0.056)
Observations	569	179	382	569	179	382
Clusters	44	25	34	44	25	34
R^2	0.235	0.301	0.202	0.103	0.226	0.158
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Historic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Muslim	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates. The unit of observation is an ethnic–territory group. The dependent variable in columns (1)–(3) is the gender motif index from Michalopoulos and Xue (2021). Columns (4)–(6) replicate the white-bias motif specification. All specifications include geographic and historical controls and a control for Muslim share (2005). Empire refers to the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Robust standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 11: Arab Slave Trade Exposure and Social Concepts in Folklore, Part A

	(1) Male Violent	(2) Female Submissive	(3) Sacrifice	(4) Patriarch	(5) Punishment
Arab	1.860**	2.517***	0.848***	0.708***	0.896**
	(0.793)	(0.740)	(0.241)	(0.230)	(0.340)
Atlantic	0.545	0.400	0.388*	0.021	0.243
	(0.438)	(0.440)	(0.201)	(0.135)	(0.233)
Distance to 1800 Routes	-0.318	0.315	-0.442	0.982	1.227
	(1.666)	(1.576)	(0.679)	(0.886)	(0.869)
Observations	565	565	565	565	565
Clusters	44	44	44	44	44
R^2	0.377	0.322	0.293	0.273	0.240
Country FE	Yes	Yes	Yes	Yes	Yes
Geo Controls	Yes	Yes	Yes	Yes	Yes
Hist. Controls	Yes	Yes	Yes	Yes	Yes
Muslim Share (2005) Control	Yes	Yes	Yes	Yes	Yes

Notes: HDFE OLS estimates. The unit of observation is an ethnic–territory group. The dependent variable is the count of concepts appearing in the folklore of a given ethnic-territory group (Michalopoulos and Xue, 2021). All specifications include geographic and historical controls, country fixed effects, and a control for the ethnic-territory Muslim share (WRD, 2005). Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 12: Arab Slave Trade Exposure and Social Concepts in Folklore, Part B

	(1) Martyr	(2) Pilgrim	(3) Faith	(4) Mosque	(5) Pork	(6) Alcohol	(7) Sin	(8) Justice
Arab	0.361**	0.418***	0.147**	0.014	0.687***	0.410**	0.452***	0.354***
	(0.134)	(0.086)	(0.070)	(0.009)	(0.226)	(0.197)	(0.162)	(0.098)
Atlantic	0.053	-0.054	-0.068	-0.001	-0.096	0.210	0.075**	0.098
	(0.042)	(0.063)	(0.053)	(0.002)	(0.146)	(0.127)	(0.033)	(0.068)
Distance	0.218	-0.012	0.083	0.006	0.605	-0.144	0.502*	-0.246
	(0.192)	(0.206)	(0.147)	(0.025)	(0.614)	(0.329)	(0.297)	(0.289)
Observations	565	565	565	565	565	565	565	565
Clusters	44	44	44	44	44	44	44	44
R^2	0.347	0.343	0.396	0.011	0.309	0.109	0.306	0.328
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geo Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hist. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Muslim Share (2005)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: HDFE OLS estimates. The unit of observation is an ethnic–territory group. The dependent variable is the count of concepts appearing in the folklore of a given ethnic-territory group (Michalopoulos and Xue, 2021). Concepts are categorized as described in Appendix. "Distance" is Distance to 1800 Routes based on Brice and Kennedy (2001). All specifications include geographic and historical controls, country fixed effects, and a control for the ethnic-territory group Muslim share (WRD, 2005). Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 13: Arab Slave Trade Exposure and Muslim Women's Labor Supply and Fertility (individual-level analysis)

	(1)	(2)	(3)
Panel A: Female Labor Force Participation			
Arab	-0.018*	-0.025***	-0.028**
	(0.009)	(0.008)	(0.011)
Atlantic	0.023**	0.020**	0.023**
	(0.010)	(0.009)	(0.009)
Distance to 1800 Routes	0.099	0.081	0.148**
	(0.068)	(0.067)	(0.066)
Observations	357,788	355,601	355,571
Clusters	136	135	134
R^2	0.157	0.159	0.159
Panel B: Number of Children Ever Born			
Arab	0.047	0.038	0.074**
	(0.039)	(0.037)	(0.030)
Atlantic	-0.106**	-0.077*	-0.011
	(0.046)	(0.044)	(0.041)
Distance to 1800 Routes	-0.672**	-0.735***	-0.957***
	(0.272)	(0.266)	(0.247)
Observations	358,922	356,730	356,700
Clusters	136	135	134
R^2	0.628	0.629	0.629
Panel C: Age at First Birth			
Arab	-0.219***	-0.177***	-0.202***
	(0.055)	(0.060)	(0.076)
Atlantic	0.179**	0.173**	0.148*
	(0.075)	(0.077)	(0.077)
Distance to 1800 Routes	1.313**	1.437***	1.808***
	(0.540)	(0.516)	(0.503)
Observations	265,061	263,400	263,374
Clusters	135	134	133
R^2	0.166	0.165	0.166
Survey FE	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
Geographic and Historical Controls	Yes	Yes	Yes
Pastoralism Controls (Becker)	No	Yes	Yes
Settlement and Animal Controls (Alesina)	No	No	Yes

Notes: OLS estimates. The unit of observation is an individual woman aged 15–49. The sample is restricted to Muslim respondents. The dependent variable is indicated by each panel header: an indicator for currently working or having worked in the last twelve months (Panel A), the number of children ever born (Panel B), and age at first birth (Panel C). Arab and Atlantic denote ethnic-group slave export intensity, $\ln(1 + \text{exports/area})$. Individual data is from all IPUMS-DHS surveys with ethnicity information; respondents are matched to Murdock (1967) ethnic groups by reported ethnicity. All specifications include survey (country-round) fixed effects, individual controls (age, age^2, urban indicator, education dummies, and ever-married indicator), and geographic and historical controls. Column (2) adds ancestral pastoralism controls (the Becker (2025) herding measure and pastoral and cultivation land suitability); column (3) further adds settlement-pattern fixed effects, a large domesticated animals indicator, and plough use (Alesina et al., 2013; Ethnographic Atlas). Sampling weights are used throughout. Robust standard errors are clustered at the ethnic-territory level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix

A1 Matching Joshua Project to Ethnographic Atlas

We use narrative text from the Joshua Project (JP), a missionary database covering approximately 3,600 people groups across 56 African countries, to construct binary indicators for features of Islamic practice at the ethnic-group level. To merge these indicators with the Ethnographic Atlas (EA) cultural variables that form the basis of our analysis dataset, we require a crosswalk linking each JP people group to its corresponding EA society. No pre-existing crosswalk exists, so we construct one using a sequential name-matching algorithm.

Matching algorithm. Both the JP and EA databases identify ethnic groups primarily by name. The key challenge is that JP uses modern naming conventions, while the EA reflects Murdock's (1967) mid-century ethnographic classifications, which in turn draw on colonial-era terminology. We address this through a series of matching passes applied in order, with each pass operating only on the subset of JP groups not yet matched:

1. *Exact match.* The normalized JP name (uppercased, whitespace-stripped, parentheticals removed) matches an EA tribe name exactly.

2. *Exact match on alternate names.* JP records carry a field of alternate and historical names. We apply the same normalization and check whether any alternate name matches an EA tribe exactly, accepting the match only when it is unique.

3. *Compound name part.* For JP names containing a comma or slash (e.g. "Mijikenda, Digo"), we check whether any comma- or slash-separated component matches an EA tribe, again accepting only unambiguous cases.

4. *First and last word.* For multi-word JP names, we check whether the first or last word (minimum four characters) matches an EA tribe.

5. *Bantu prefix stripping.* We strip common Bantu person-class prefixes (*Ba-*, *Wa-*, *Ki-*, *Bu-*, *Mu-*, *Ma-*, *Mi-*) from the normalized JP name and test for an exact EA match on the resulting stem.

6. *Word-level matching on alternate names.* We extend the first-word, last-word, and any-word tests to the alternate names field, again requiring uniqueness.

7. *Manual matches.* A list of ethnographic equivalences handles cases not recoverable by name matching: spelling variants that diverge by more than a letter or two (e.g. *Zarma*

→ ZERMA), colonial names that differ systematically from the modern self-name (e.g. *Mafa* → MATAKAM), and subgroup-to-parent mappings where JP distinguishes subvarieties that Murdock and the EA treat as a single unit (e.g. *Makhuwa-Meetto* → MAKUA).

The algorithm is deliberately conservative: steps 1–6 require exact string match (after normalization). False positives are therefore rare, though the procedure misses groups whose JP and EA names diverge substantially.

Validation against Kincaide et al. (2025) We validate the matching procedure using the concordance of Kincaide et al. (2025), which provides a hand-constructed mapping from all 833 ethnic group polygons in Murdock's Africa map to their corresponding EA societies and serves as our benchmark. In this reference concordance, the correct matches are externally established. Applying our algorithm to this problem and comparing the output with the reference matches identified by Kincaide et al. (2025) gives some idea of the accuracy of our algorithm.

Concretely, we take the 833 Murdock group names from the Kincaide et al. concordance as input and apply the automated name-matching passes described above against the set of all 486 unique EA names referenced in the concordance. We omit the alternate-names passes since no alternate names field exists for Murdock tribes. We then compare each algorithmic assignment to the concordance's assignment.

The algorithm matches 438 of 833 Murdock groups (52.6%), yielding **precision of 99.1 percent**: of the 438 groups matched, 434 agree with the Kincaide et al. assignment, and only four disagree. Inspection of the four disagreements reveals that in every case the algorithm recovers a name that genuinely exists in the EA but Kincaide et al. assign the group to a different EA society on cultural clustering grounds that cannot be inferred from the name alone. For instance, BISA matches *BISA* exactly in the EA, but Kincaide et al. assign it to *BEMBA* because Murdock treated the Bisa as a Bemba cultural cluster. These cases represent substantively different matching decisions.

Recall is 52.1 percent (434 of 833). Of the 833 pairs in the Kincaide et al. concordance, 464 (55.7%) were established by direct name matching or near-name variants; the remaining 37 percent were matched through Murdock's (1959) cultural clustering assignments and alternative ethnographic sources, information that names do not encode. Our algorithm recovers **93.5 percent of the theoretically recoverable pairs** (434 of 464).

Taken together, the validation results support two conclusions. First, the algorithm is reliable when it finds a match, giving confidence that our JP–EA crosswalk does not systematically introduce measurement error by assigning JP groups to the wrong EA society. Second, the recall relative to Kincaide et al. reflects the limits on name-based matching

rather than an algorithmic problem, and our manual pass addresses some of that gap. Because measurement error in outcome variables attenuates estimates toward zero, any residual misclassification from unmatched or incorrectly matched groups will likely bias our estimates conservatively.

A2 Folklore Motif Indices

This section provides the motifs selected from the Michalopoulos and Xue (2021) Folklore data for use in the *white-bias motif index* and the *gender conservativism motif index*. The *gender conservatism motif index* aggregates the prevalence of eight motifs that concern female chastity, the punishment of perceived sexual transgression, the portrayal of women as sexually threatening, and the forcible taming of dominant female figures:

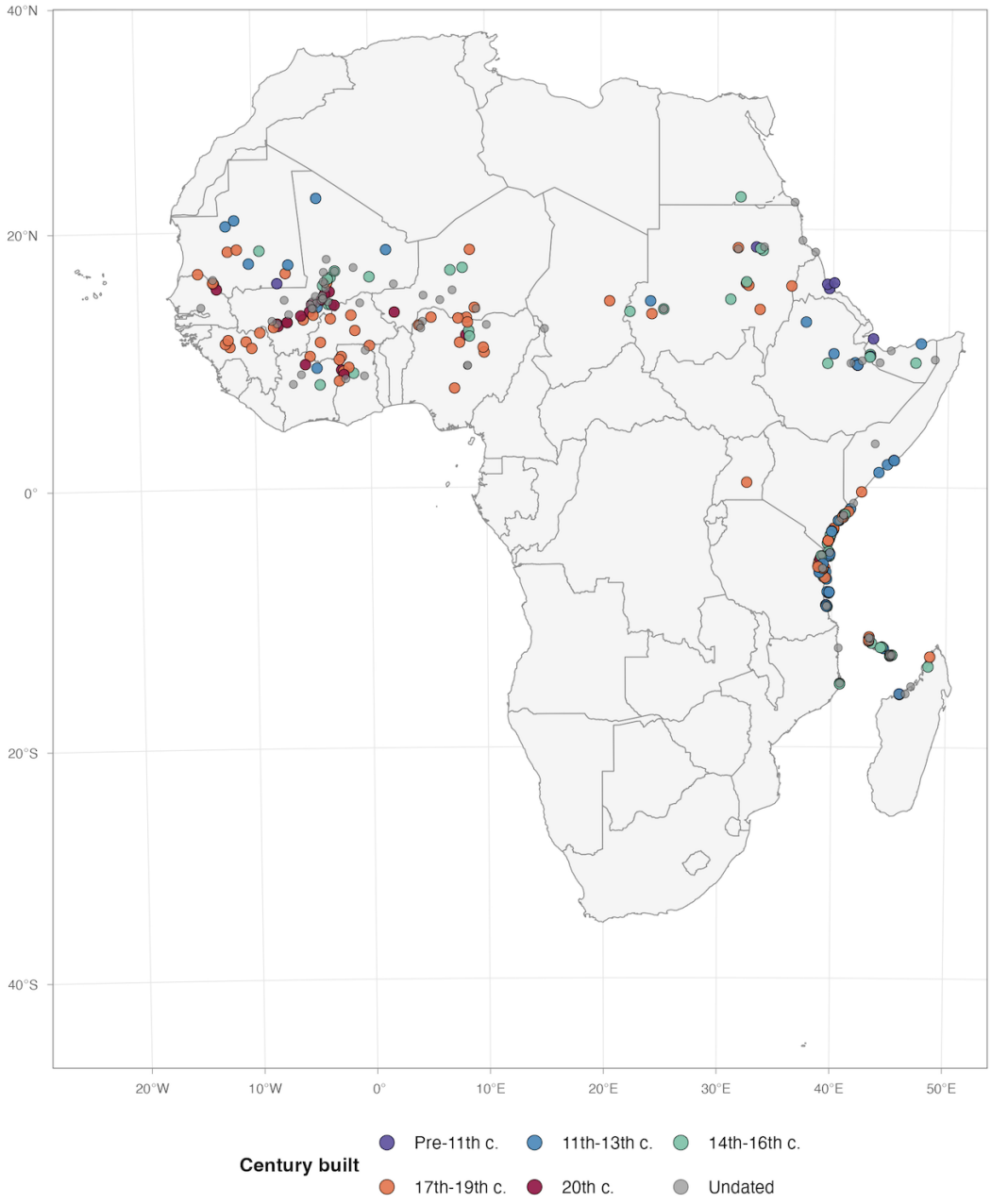
- **f100**: The magic tool shows that the only chaste woman is a maid, an orphan, etc.
- **f71**: Having been refused, a man falsely accuses a woman of debauchery; others believe and try to punish her cruelly.
- **f9**: For various reasons, mating with a woman is deadly for a man.
- **f9a**: Teeth, blades, or sharp stones in the vagina or on the inside of the thighs; vagina—toothed mouth.
- **k93a**: When he lies with a woman, a man places a sharp or thorny object between her and himself as a sign that at night he will observe abstinence.
- **k55**: A powerful character finds out if his guest made love to his daughter or wife.
- **k56**: One of the sisters, friends or acquaintances of girls or young women meets a character who is able to reward and punish.
- **f9g**: A strong woman overcomes and kills suitors. Hero or his helper tames her (usually whips in the wedding night). The hero marries her.

The second index, the *white-bias motif index*, aggregates two motifs reflecting skin-colour-based hierarchies and a preference for whiteness:

- **i21**: The inhabitants of the underworld or country at sunrise have red (yellow) hair and/or red or black skin and/or suffer from the heat of the sun.
- **n11**: The character takes possession of the desire to have a child or spouse, who are as white and rosy as snow (milk) and blood.

A3 Additional Figures and Tables

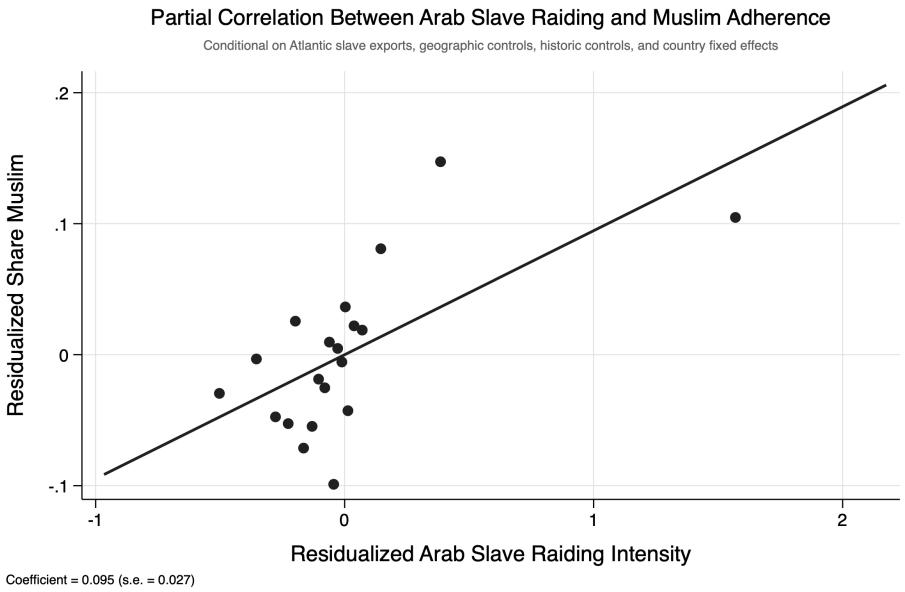
Figure A1: Spatial Distribution of Historical Mosques in Africa, by Century of Construction



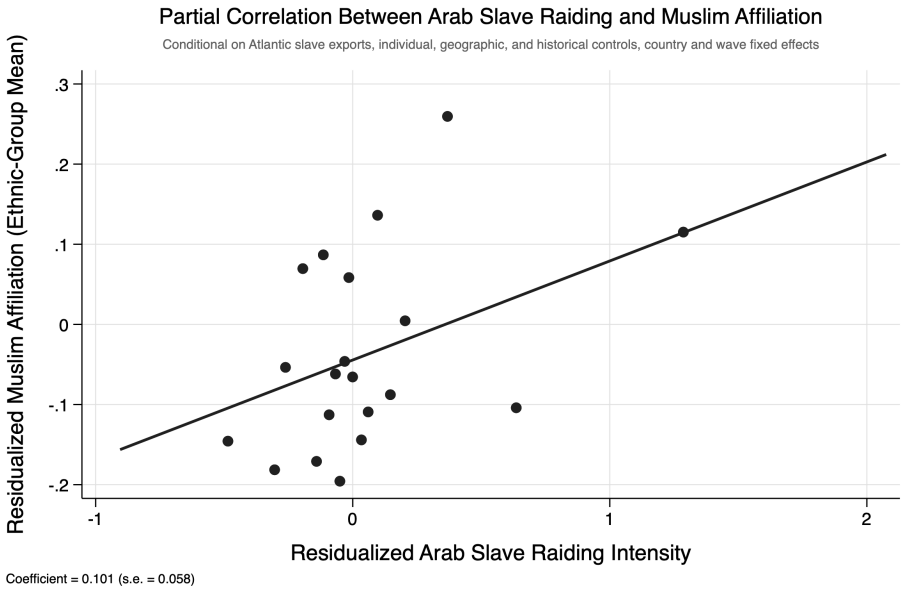
Notes: This figure plots the locations of historical mosques across 18 African countries, drawn from Pradines (2022). The sample is restricted to mosques whose geographic coordinates could be verified with high confidence; mosques with uncertain or unverifiable locations are excluded. Points are colored by the century in which the mosque was originally built, binned into five eras: pre-11th century (8 mosques), 11th–13th century (47), 14th–16th century (71), 17th–19th century (81), and 20th century (23); the 83 mosques without a documented construction date are shown in gray.

Figure A2: Partial Correlation Plots – Main Individual and Ethnic Analyses

(a) Ethnic-level data



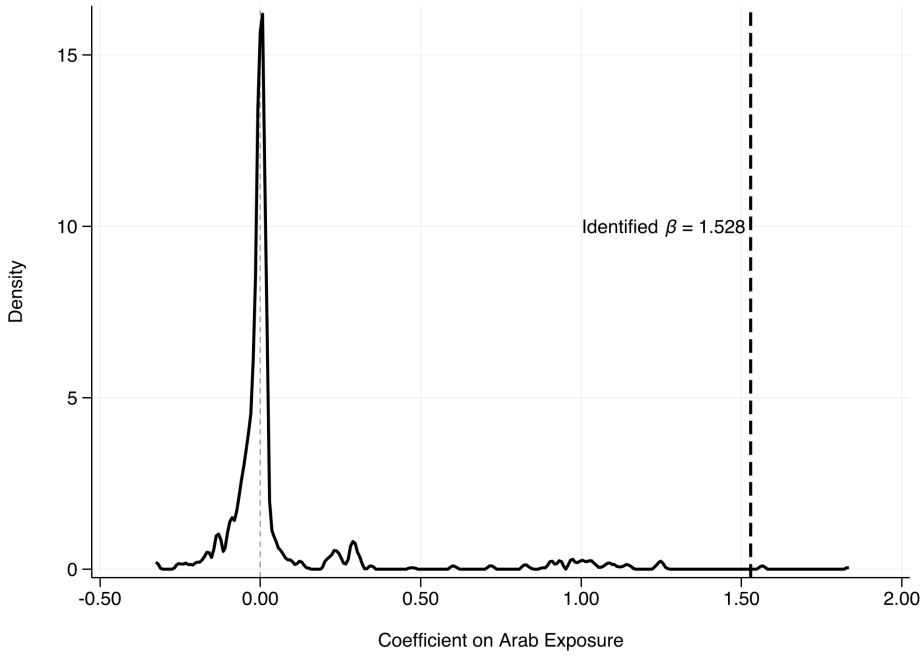
(b) Individual-level data



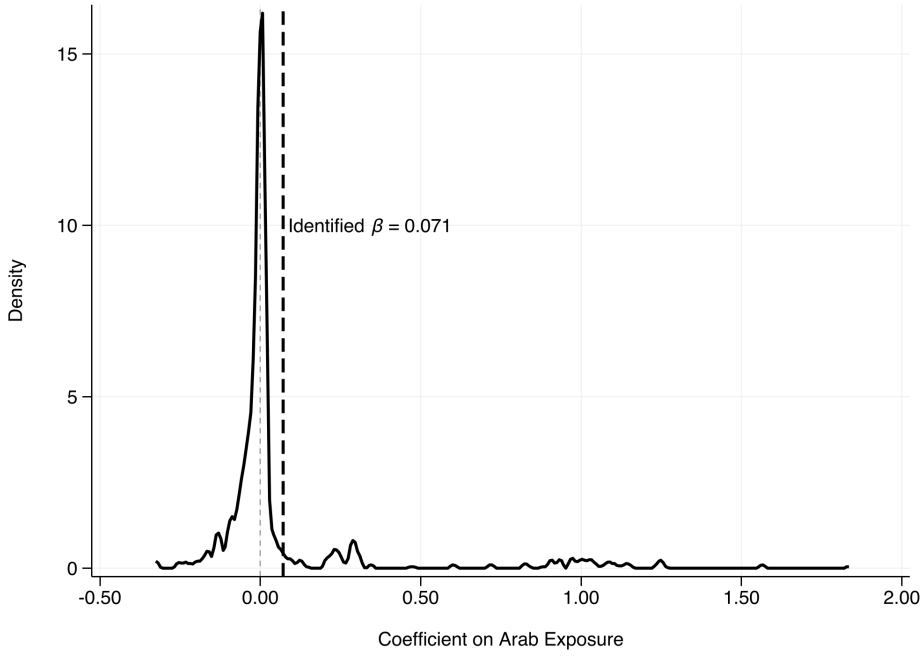
Notes: Partial correlation plots. Residuals are ranked by the residualized x-value and grouped into 20 equal-frequency bins; each point plots the mean of the x- and y-residuals within a bin. Reported coefficients and standard errors are from the underlying regression, not the binned averages. Panel (a) plots residuals from Column (3) of Table 2, conditional on *Atlantic* exposure, country fixed effects, and the geographic and historical covariates described in Section 3.4. The pattern is not driven by high-exposure outliers: Table A2 shows the coefficient remains significant at the 1% level under winsorization. Panel (b) plots residuals from Column (4) of Table 3, conditional on *Atlantic* exposure, country and survey-wave fixed effects, the same geographic and historical covariates, and the individual controls described in Section 4.

Figure A3: Distribution of Placebo Coefficients on Arab Slave Trade Exposure – Folklore Motifs (1,000 iterations)

(a) Gender motif index



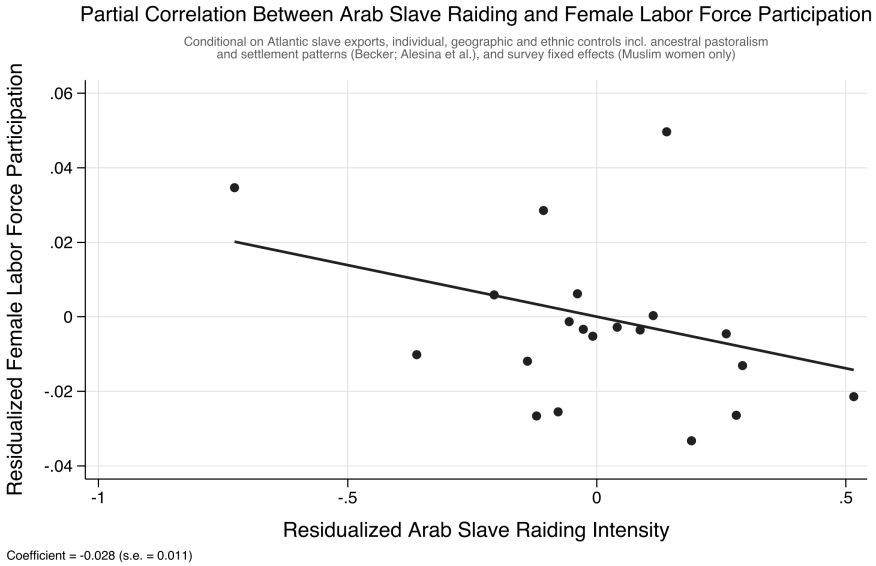
(b) White-bias motif index



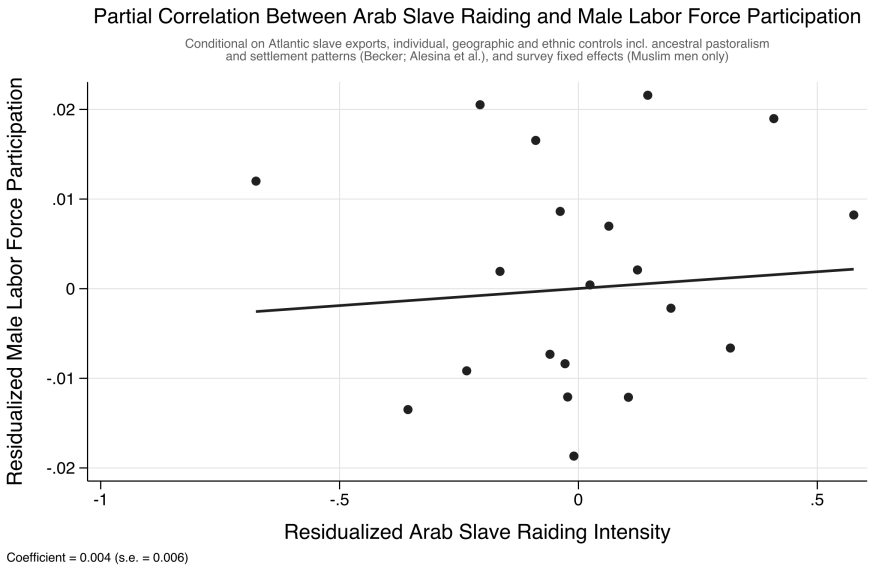
Notes: Coefficients on placebo Arab raiding after 1,000 iterations, as compared with (a) the true coefficient on the gender motif index, and (b) the true coefficient on the white-bias motif index. See Section 6.

Figure A4: Partial Correlation Plots – Labor Force Participation (Women and Men)

(a) Female Labor Force Participation



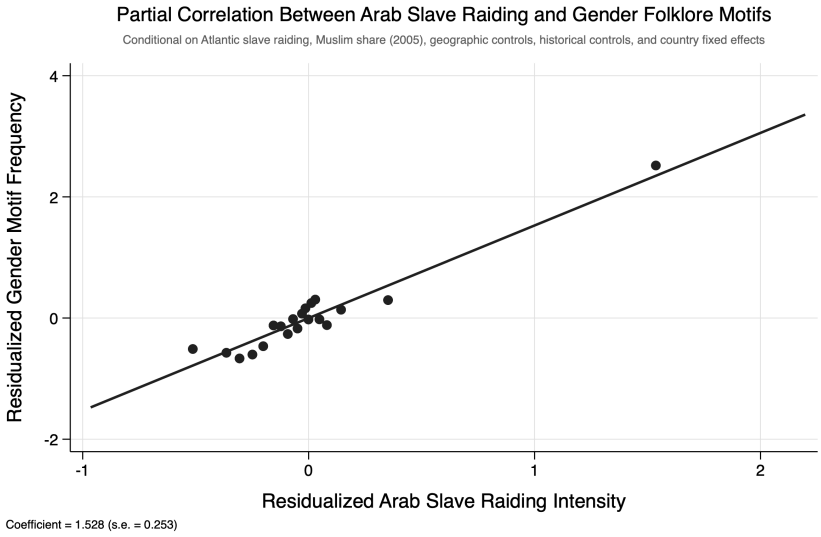
(b) Male Labor Force Participation



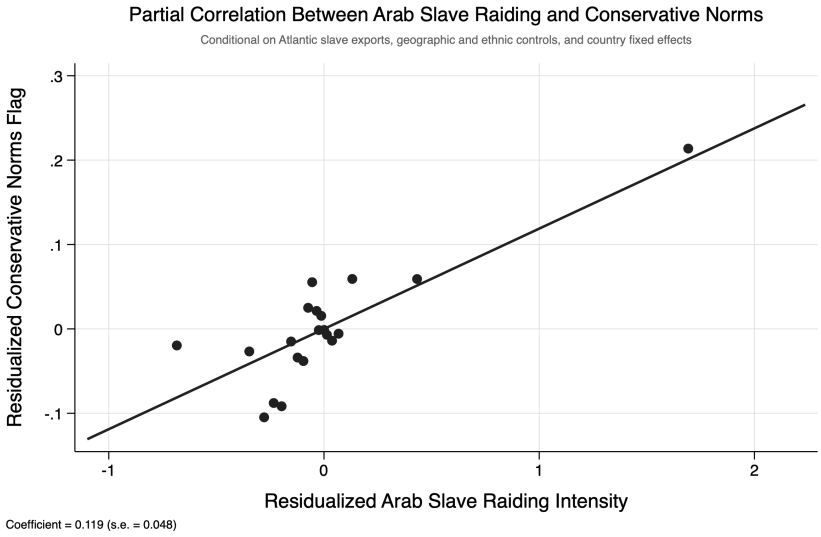
Notes: Partial correlation plots. Residuals are ranked by the residualized x-value and grouped into 20 equal-frequency bins; each point plots the mean of the x- and y-residuals within a bin. Reported coefficients and standard errors are from the underlying individual-level regression; the line is the weighted linear fit on the underlying individual-level residuals. Panel (a) plots residuals from Column (3) of Panel A of Table 13 for Muslim women aged 15–49; panel (b) plots residuals from Column (3) of Table A20 for Muslim men aged 15–49. Both are conditional on *Atlantic* exposure, survey fixed effects, the individual controls described in Section 4, the geographic and historical covariates described in Section 3.4, log population density, ancestral pastoralism controls (Becker, 2025), and settlement-pattern and plough controls (Alesina et al., 2013). Regressions and residualizations use DHS person weights.

Figure A5: Partial Correlation Plots – Gender Conservatism Motifs (Folklore) and Social Conservative Flag (Joshua Project)

(a) Gender conservatism motifs (Folklore)



(b) Conservative social norms flag (Joshua Project)



Notes: Partial correlation plots. Residuals are ranked by the residualized x-value and grouped into 20 equal-frequency bins; each point plots the mean of the x- and y-residuals within a bin. Reported coefficients and standard errors are from the underlying ethnic-group-level regression, not the binned averages. Panel (a) plots residuals from Column (1) of Table 10: the outcome is the gender conservatism motif index constructed from Michalopoulos and Xue (2021), conditional on *Atlantic* exposure, country fixed effects, the group's contemporary Muslim share, and the geographic and historical covariates described in Section 3.4. Panel (b) plots residuals from Column (2) of Table A18: the outcome is an indicator for mentions of conservative social norms in a group's Joshua Project narrative, conditional on *Atlantic* exposure, country fixed effects, and the same geographic and historical covariates. Only groups with a non-zero Joshua Project Muslim share are included. Standard errors are clustered at the country level.

Table A1: Arab Slave Trade Exposure and Pre-15th Century Mosque Presence

	Pre-15th Century Mosques		
	(1)	(2)	(3)
Arab	0.111	0.057	0.059
	(0.078)	(0.072)	(0.070)
Atlantic	-0.020*	-0.016	-0.026
	(0.011)	(0.016)	(0.028)
Distance to trade			-0.450***
			(0.159)
Land inequality			-0.037
			(0.029)
Observations	629	629	573
Clusters	48	48	48
R^2	0.015	0.071	0.114
Country FE	No	Yes	Yes
Geo. Controls	No	No	Yes
Historic Controls	No	No	Yes

Notes: OLS estimates of equation 1. The dependent variable is the total number of pre-15th century mosques in a given ethnic-group territory, digitized from Pradines (2022). The primary explanatory variables are Arab-route exposures—log (1 + slaves/km^2). Country fixed effects are included in columns (2)–(3). Geographic controls (col. 3) are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls (col. 3) are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization (jurisdiction beyond the local community), and the density of Christian missions during the colonial period. Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A2: Arab Slave Trade Exposure (IHS Transformed and Winsorized) and Muslim Share (2005)

	% Muslim				Maj. Muslim
	(1)	(2)	(3)	(4) Muslim Empire = 0	(5)
Arab (Normalized)	0.302***	0.220***	0.190***	0.186***	0.211***
	(0.071)	(0.056)	(0.048)	(0.065)	(0.045)
Atlantic	0.084*	-0.009	0.019	-0.036	-0.007
	(0.045)	(0.028)	(0.034)	(0.047)	(0.048)
Distance to trade			-0.337***	-0.248**	-0.297***
			(0.094)	(0.096)	(0.095)
Land inequality			0.035*	0.021	0.042**
			(0.017)	(0.018)	(0.019)
Observations	629	629	573	390	573
Clusters	48	48	48	42	48
R^2	0.060	0.521	0.649	0.523	0.547
Country FE	No	Yes	Yes	Yes	Yes
Geo. Controls	No	No	Yes	Yes	Yes
Historic Controls	No	No	Yes	Yes	Yes

Notes: OLS estimates of equation (1). The unit of observation is an ethnic–territory group. The primary explanatory variables are Arab-route exposures normalized by tribe area. These values are modified with an inverse hyperbolic sine transformation and winsorized at the 95% level. *Distance to trade* captures the great–circle distance from the ethnic centroid to the nearest major caravan or maritime route circa 1800 (Brice and Kennedy, 2001). *Land inequality* captures within–territory land–suitability inequality for each Murdock (1959) ethnic territory. In columns (1)–(4), the dependent variable is the ethnic–territory share that is Muslim in WRD (2005); in column (5) it is an indicator equal to one if the ethnic group is majority Muslim. Country fixed effects are included in columns (2)–(5). Geographic controls are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization, and the density of Christian missions during the colonial period. Column (4) excludes ethnic territories inside the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Robust standard errors clustered at the country level in parentheses. Results are robust to limiting to controls selected through a double LASSO procedure detailed in Table A8. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Arab Slave Trade Exposure and Muslim Share (2005) (Extensive Margin)

	% Muslim				Maj. Muslim
	(1)	(2)	(3)	(4)	(5)
				Muslim Empire = 0	
Arab (Binary)	0.120***	0.148***	0.084***	0.092***	0.066*
	(0.044)	(0.028)	(0.031)	(0.026)	(0.035)
Atlantic	0.018	-0.034	-0.015	-0.033	-0.040
	(0.027)	(0.022)	(0.026)	(0.028)	(0.033)
Distance to trade			-0.337***	-0.230**	-0.303***
			(0.095)	(0.092)	(0.099)
Land inequality			0.036**	0.020	0.044**
			(0.017)	(0.017)	(0.018)
Observations	629	629	573	390	573
Clusters	48	48	48	42	48
R^2	0.012	0.510	0.633	0.509	0.530
Country FE	No	Yes	Yes	Yes	Yes
Geo. Controls	No	No	Yes	Yes	Yes
Historic Controls	No	No	Yes	Yes	Yes

Notes: OLS estimates of equation (1). The unit of observation is an ethnic–territory group. The primary explanatory variable is a dummy variable for whether a group experienced Arab slave raiding. *Distance to trade* captures the great–circle distance from the ethnic centroid to the nearest major caravan or maritime route circa 1800 (Brice and Kennedy, 2001). *Land inequality* captures within–territory land–suitability inequality for each Murdock (1959) ethnic territory. In columns (1)–(4), the dependent variable is the ethnic–territory share that is Muslim in WRD (2005); in column (5) it is an indicator equal to one if the ethnic group is majority Muslim. Country fixed effects are included in columns (2)–(5). Geographic controls are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization, and the density of Christian missions during the colonial period. Column (4) excludes ethnic territories inside the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Robust standard errors clustered at the country level in parentheses. Results are robust to limiting to controls selected through a double LASSO procedure detailed in Table A8. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A4: Arab Slave Trade Exposure and Muslim Share (2005) (Intensive Margin)

	% Muslim				Maj. Muslim
	(1)	(2)	(3)	(4)	(5)
				Muslim Empire = 0	
Arab (Intensity)	0.207***	0.086	0.199***	0.142***	0.211***
	(0.054)	(0.072)	(0.038)	(0.046)	(0.047)
Atlantic	0.132	0.259	0.571	2.434***	0.460
	(0.154)	(0.194)	(0.568)	(0.620)	(0.479)
Distance to trade			-0.262	0.136	-0.147
			(0.301)	(0.235)	(0.358)
Land inequality			-0.006	-0.052	0.006
			(0.056)	(0.112)	(0.067)
Observations	118	118	109	79	109
Clusters	27	27	27	18	27
R^2	0.162	0.255	0.520	0.475	0.410
Country FE	No	Yes	Yes	Yes	Yes
Geo. Controls	No	No	Yes	Yes	Yes
Historic Controls	No	No	Yes	Yes	Yes

Notes: OLS estimates of equation (1). The unit of observation is an ethnic–territory group. The primary explanatory variables are Arab-route exposures—log (1 + slaves/km^2). The sample is limited to observations with non-zero Arab route exposure. *Distance to trade* captures the great–circle distance from the ethnic centroid to the nearest major caravan or maritime route circa 1800 (Brice and Kennedy, 2001). *Land inequality* captures within–territory land–suitability inequality for each Murdock (1959) ethnic territory. In columns (1)–(4), the dependent variable is the ethnic–territory share that is Muslim in WRD (2005); in column (5) it is an indicator equal to one if the ethnic group is majority Muslim. Country fixed effects are included in columns (2)–(5). Geographic controls are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization, and the density of Christian missions during the colonial period. Column (4) excludes ethnic territories inside the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Robust standard errors clustered at the country level in parentheses. Results are robust to limiting to controls selected through a double LASSO procedure detailed in Table A8. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5: Arab Slave Trade Exposure and Muslim Share (2005)—Excluding the Indian Ocean Route

	% Muslim				Maj. Muslim
	(1)	(2)	(3)	(4) Muslim Empire = 0	(5)
Old Arab	0.218***	0.109***	0.068***	0.040	0.075***
	(0.057)	(0.038)	(0.018)	(0.049)	(0.019)
Indian	0.122*	0.122*	0.239**	0.186	0.260**
	(0.065)	(0.066)	(0.115)	(0.135)	(0.114)
Atlantic	0.021	-0.032	-0.009	-0.034	-0.032
	(0.026)	(0.022)	(0.027)	(0.029)	(0.032)
Distance to trade			-0.335***	-0.245**	-0.296***
			(0.103)	(0.100)	(0.105)
Land Inequality			0.038**	0.021	0.045**
			(0.018)	(0.019)	(0.020)
Observations	629	629	573	390	573
Clusters	48	48	48	42	48
R^2	0.052	0.510	0.644	0.509	0.542
Country FE	No	Yes	Yes	Yes	Yes
Geo. controls	No	No	Yes	Yes	Yes
Historical controls	No	No	Yes	Yes	Yes

Notes: OLS estimates of equation 1. The unit of observation is an ethnic–territory group. The dependent variable is the ethnic–territory share Muslim (WRD, 2005) in columns (1)–(4); in column (5) it is an indicator equal to one if the ethnic–territory is majority Muslim. Old Arab aggregates Red Sea and trans-Saharan routes; Indian denotes the Indian Ocean route; Atlantic denotes the Atlantic route. Country fixed effects are included in columns (2)–(5). Geographic controls and historical controls are described in Section 3.4. Column (4) excludes ethnic territories inside the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Robust standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: Kitchen Sink Robustness: Arab Slave Trade Exposure and Muslim Share (2005)

	% Muslim (2005)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Arab	0.092***	0.090***	0.088***	0.078***	0.075***	0.073***	0.071***	0.080***
	(0.028)	(0.027)	(0.028)	(0.029)	(0.028)	(0.026)	(0.024)	(0.025)
Atlantic	-0.016	0.001	0.001	-0.001	-0.007	-0.014	-0.015	-0.013
	(0.025)	(0.026)	(0.026)	(0.026)	(0.029)	(0.029)	(0.030)	(0.029)
Observations	501	501	501	501	501	501	501	501
Clusters	47	47	47	47	47	47	47	47
R^2	0.642	0.662	0.662	0.671	0.673	0.675	0.674	0.679
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
+ 600 CE trade routes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
+ Land use	No	No	Yes	Yes	Yes	Yes	Yes	Yes
+ Ethnographic (extra)	No	No	No	Yes	Yes	Yes	Yes	Yes
+ Pastoralism (extra)	No	No	No	No	Yes	Yes	Yes	Yes
+ Log territory area	No	No	No	No	No	Yes	Yes	Yes
+ Distance to port	No	No	No	No	No	No	Yes	Yes
+ Tsetse suitability	No	No	No	No	No	No	No	Yes

Notes: Kitchen-sink robustness following Cao et al. (2024). The unit of observation is an ethnic–territory group. The dependent variable is the ethnic–territory share that is Muslim in WRD (2005). Column (1) replicates the fully-saturated specification in Table 2, column (3): country fixed effects, Atlantic-route exposure (placebo), the geographic controls (distance to 1800 trade routes, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, desert indicator, distance to the coast, distance to Mecca), and the ethnographic controls (nomadic settlement pattern, dependence on pastoralism, jurisdictional hierarchies, density of Christian missions). Columns (2)–(8) successively add control groups: distance to the pre-Islamic (600 CE) trade routes, land use (pasture and cultivation fractions), additional ethnographic traits (historical slavery, historical polygyny), alternative pastoralism measures (predominant pastoralism, Becker's herding index), log ethnic-territory area, distance to the nearest Arab embarkation port, and the tsetse suitability index of Alsan (2015). High-missingness controls (high gods, class stratification) are excluded to preserve sample. The sample is held constant across columns at the set of observations with non-missing values for every control in column (8), so that coefficient changes reflect only added controls. Robust standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: All Control Variables and Contemporary Muslim Share (2005)

	Dependent variable: % Muslim (2005), standardized				
	Specification A		Specification B		
	Additional variable (1)	Obs. (2)	Additional variable (3)	Arab (4)	Obs. (5)
Arab				0.134*** (0.040)	629
Nomadic settlement	0.133*** (0.040)	589	0.151*** (0.041)	0.165*** (0.043)	589
Animal husbandry	0.198*** (0.064)	629	0.198*** (0.064)	0.139*** (0.039)	629
Jurisdictional hierarchy	0.143** (0.067)	574	0.135** (0.065)	0.139*** (0.041)	574
Christian missions density	-0.085*** (0.019)	629	-0.085*** (0.018)	0.139*** (0.041)	629
Distance to 1800 trade routes	-0.422*** (0.139)	629	-0.427*** (0.142)	0.131*** (0.039)	629
Land-suitability inequality (log)	0.255*** (0.062)	629	0.247*** (0.061)	0.122*** (0.037)	629
Absolute latitude	0.621** (0.237)	629	0.585** (0.234)	0.113*** (0.038)	629
Average land suitability (log)	-0.375*** (0.080)	629	-0.371*** (0.085)	0.134*** (0.036)	629
Terrain ruggedness (log)	-0.087 (0.055)	629	-0.087* (0.051)	0.133*** (0.039)	629
Irrigation potential	0.201** (0.082)	629	0.200** (0.080)	0.132*** (0.037)	629
Desert indicator	0.173** (0.070)	629	0.167** (0.071)	0.131*** (0.042)	629
Distance to coast	0.049 (0.113)	629	0.036 (0.112)	0.134*** (0.040)	629
Distance to Mecca	-0.267*** (0.090)	629	-0.244*** (0.087)	0.131*** (0.042)	629
Distance to 600 CE trade routes	-0.521** (0.209)	629	-0.499** (0.206)	0.124*** (0.039)	629
Pasture fraction	0.134** (0.058)	629	0.107* (0.058)	0.125*** (0.041)	629
Cultivation fraction	-0.027 (0.045)	629	-0.029 (0.044)	0.135*** (0.041)	629
Predominant pastoralism	0.149*** (0.044)	629	0.162*** (0.045)	0.149*** (0.038)	629
Animal herding	-0.021 (0.031)	589	-0.024 (0.028)	0.149*** (0.045)	589
Becker herding index	0.209*** (0.068)	589	0.209*** (0.069)	0.153*** (0.044)	589
Historical slavery	0.153*** (0.048)	558	0.140*** (0.046)	0.122*** (0.044)	558
Historical polygyny	-0.008 (0.049)	618	-0.021 (0.051)	0.136*** (0.041)	618
Class stratification	0.106* (0.061)	520	0.111* (0.056)	0.121*** (0.032)	520
Log territory area	0.174*** (0.031)	629	0.182*** (0.031)	0.136*** (0.033)	629
Distance to nearest Arab port	-0.491*** (0.155)	629	-0.449*** (0.151)	0.117*** (0.035)	629
Tsetse suitability index	-0.084 (0.101)	620	-0.058 (0.098)	0.127*** (0.041)	620

Notes: Following Cao et al. (2024). The unit of observation is an ethnic–territory group. The dependent variable is the standardized share of Muslims in the ethnic territory in 2005 (WRD). In Specification A (columns 1–2), Muslim share is regressed on each alternative pre-colonial ethnic feature with country fixed effects only. In Specification B (columns 3–5), exposure to the Arab slave routes and the Atlantic placebo are added to the regression alongside the alternative feature. Each row is an independent regression that includes only the listed feature alongside the baseline; controls are not cumulative across rows. All variables are standardized so that reported coefficients are standardized beta coefficients. The top row reports the standardized Arab coefficient from a baseline regression with country fixed effects and Atlantic exposure as the only controls, without any additional pre-colonial feature. The tsetse suitability index is from Alsan (2015). The high-gods indicator from the Ethnographic Atlas is excluded because it is missing for approximately 40% of ethnic territories. Robust standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Baseline and LASSO Control Variables

Full Set of Baseline Covariates Used in the Post-Double-Selection Lasso	
Historical Characteristics	Nomadic; Animal husbandry; Jurisdictional hierarchies; Christian missions density
Geography & Market Access	Distance to 1800 trade routes; Within-territory land-suitability inequality; Average land suitability; Absolute latitude; Territory ruggedness; Irrigation potential; Desert; Distance to sea; Distance to Mecca
Individual Controls	Age; Age^2; Urban; Primary education; Secondary education; Ever married; Household wealth index
LASSO Control Variables Used	
Ethnic-level Analysis	Animal husbandry; Jurisdictional hierarchies; Distance to 1800 trade routes; Absolute latitude; Average land suitability; Desert
Individual-level Analysis	Distance to 1800 trade routes; Nomadic; Animal husbandry; Jurisdictional hierarchies; Christian missions density; Within-territory land-suitability inequality; Average land suitability; Absolute latitude; Territory ruggedness; Irrigation potential; Desert; Distance to sea; Distance to Mecca; Age; Urban; Primary education; Secondary education; Ever married; Household wealth index

Table A9: Arab Slave Trade Exposure and Muslim Share (2005), LASSO Controls

	% Muslim				Maj. Muslim
	(1)	(2)	(3)	(4)	(5)
				Muslim Empire = 0	
Arab	0.194***	0.112***	0.100***	0.098**	0.109***
	(0.044)	(0.034)	(0.027)	(0.046)	(0.027)
Atlantic	0.021	-0.032	-0.020	-0.032	-0.035
	(0.026)	(0.022)	(0.024)	(0.027)	(0.031)
Distance to trade			-0.376***	-0.313***	-0.370***
			(0.107)	(0.104)	(0.104)
Observations	629	629	574	390	574
Clusters	48	48	48	42	48
R^2	0.051	0.511	0.628	0.483	0.524
Country FE	No	Yes	Yes	Yes	Yes
Geo. Controls	No	No	Yes	Yes	Yes
Historic Controls	No	No	Yes	Yes	Yes

Notes: OLS estimates of equation (1). The unit of observation is an ethnic–territory group. The primary explanatory variables are Arab-route exposures—log (1 + slaves/km^2). *Distance to trade* captures the great–circle distance from the ethnic centroid to the nearest major caravan or maritime route circa 1800 (Brice and Kennedy, 2001). In columns (1)–(4), the dependent variable is the ethnic–territory share that is Muslim in WRD (2005); in column (5) it is an indicator equal to one if the ethnic group is majority Muslim. Country fixed effects are included in columns (2)–(5). Controls are as described in Table A8. Column (4) excludes ethnic territories inside the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Robust standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: Arab Slave Trade Exposure and Muslim Affiliation (Individual-Level Analysis), LASSO Controls

	Dependent variable: Muslim			
	(1)	(2)	(3)	(4)
Arab	0.125*	0.246***	0.106*	0.142**
	(0.073)	(0.085)	(0.060)	(0.064)
Atlantic	-0.128***	-0.136***	-0.035	-0.071**
	(0.038)	(0.033)	(0.032)	(0.029)
Observations	580,806	580,806	412,129	412,043
Clusters	173	173	121	121
R^2	0.217	0.510	0.608	0.593
Wave FE	Yes	Yes	Yes	Yes
Country FE	No	Yes	Yes	No
District FE	No	No	No	Yes
LASSO Controls	No	No	Yes	Yes

Notes: Linear probability model estimates of equation (2), the individual-level companion to Table A9. The unit of observation is an individual. The dependent variable is an indicator equal to one if the respondent reports their religion as Islam. Individual data is from all available DHS and MICS surveys that elicit religion and ethnicity (1994–2017). Wave fixed effects are included throughout; column (2) adds country fixed effects. Columns (3)–(4) include the controls selected by the post-double-selection LASSO of Belloni et al. (2014) from the baseline geographic and historical controls and the individual covariates; the algorithm retains every candidate except age^2 (the candidate and selected sets are listed in Table A8). Column (4) replaces country fixed effects with district fixed effects. Standard errors clustered by ethnicity in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: Matching on Observables: Arab Slave Trade Exposure and Muslim Share (2005)

	% Muslim		
	(1) Dyad	(2) Dyad Restricted	(3) Pscore
Arab	0.115***	0.111***	0.121***
	(0.036)	(0.036)	(0.035)
Atlantic	0.024	0.016	-0.012
	(0.024)	(0.027)	(0.050)
Distance to 1800 Routes	-0.156**		-0.340**
	(0.065)		(0.159)
Land Inequality	0.021		0.062*
	(0.020)		(0.031)
Constant	0.207	-0.061	1.138***
	(0.215)	(0.445)	(0.278)
Observations	7,870	2,002	437
R^2	0.506	0.513	0.580
Dyad FE	Yes	Yes	No
Geographic controls	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes

Notes: OLS estimates of equation 1, including neighbor pair fixed effects or using propensity score matching. The unit of observation is an ethnic–territory group. The dependent variable is the ethnic–territory share Muslim (WRD, 2005). Column (1) includes dyads as described in Section 5.2. Column (2) restricts to matched dyads that are similar in distance to 1800 trade routes and in land-suitability inequality. Column (3) uses a propensity-score restriction that balances the full set of observables. Dyad fixed effects are included. Robust standard errors in parentheses clustered at (i) ethnic and bordering group level; (ii) country level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A12: Ports Used to Construct the Distance-to-Port Instrument

Port	Modern country
Panel A: Trans-Saharan route (Mediterranean coast)	
Algiers	Algeria
Benghazi	Libya
Cairo	Egypt
Tripoli	Libya
Tunis	Tunisia
Panel B: Red Sea / Gulf of Aden route	
Assab	Eritrea
Berbera	Somalia
Djibouti	Djibouti
Massawa	Eritrea
Suakin	Sudan
Tajura	Djibouti
Zeila	Somalia
Panel C: Indian Ocean route (Swahili coast)	
Ibo	Mozambique
Kilwa	Tanzania
Mombasa	Kenya
Mozambique Island	Mozambique
Pemba	Tanzania
Quelimane	Mozambique
Zanzibar	Tanzania

Notes: List of the ports used to construct the *Distance to Port* instrument in Appendix Table A13. The instrument is the great-circle distance, in kilometers, from each ethnic–territory centroid to the nearest of the ports listed above. The ports are the African embarkation points of the three Arab slave-trade routes—the trans-Saharan, Red Sea, and Indian Ocean trades. The port set follows Corno et al. (2020) (Online Appendix Table B1, "Slave ports of exit and destination"). The Indian Ocean and Red Sea ports correspond to the ports of exit in Corno et al. (2020); Algiers, Tunis, and Djibouti are added to the trans-Saharan and Red Sea coasts. Port locations are ultimately drawn from Lovejoy (1989), Miège (1981), Mowafi (1981), and Zdanowski (2008), as compiled in Corno et al. (2020).

Table A13: IV Estimates: Distance to Port and Muslim Share (2005)

Panel A: First Stage					
	Arab Slave Exports				
	(1)	(2)	(3)	(4)	(5)
Distance to Port (100 km)	-0.005**	-0.011**	-0.012**	-0.012**	-0.012**
	(0.002)	(0.005)	(0.006)	(0.005)	(0.006)
Atlantic	-0.039**	-0.057**	-0.080**	-0.004	-0.080**
	(0.016)	(0.027)	(0.031)	(0.016)	(0.031)
Observations	629	625	570	382	570
Country FE	No	Yes	Yes	Yes	Yes
Geographic Controls	No	No	Yes	Yes	Yes
Historic Controls	No	No	Yes	Yes	Yes
Panel B: Second Stage					
	% Muslim				Majority Muslim
	(1)	(2)	(3)	(4)	(5)
				Muslim Empire = 0	
Arab	0.001	1.451**	0.982*	0.823*	1.046*
	(0.530)	(0.696)	(0.566)	(0.475)	(0.616)
Atlantic	0.007	0.062	0.061	-0.017	0.047
	(0.034)	(0.053)	(0.054)	(0.028)	(0.059)
Observations	629	625	570	382	570
Weak-IV stat (F)	9.44	6.45	6.14	8.22	6.14
Country FE	No	Yes	Yes	Yes	Yes
Geographic Controls	No	No	Yes	Yes	Yes
Historic Controls	No	No	Yes	Yes	Yes

Notes: 2SLS estimates where *Distance to Port*, the distance (in hundreds of kilometers) from each ethnic-territory centroid to the nearest port on an Arab slave route, is used as an instrument for *Arab*, a measure of Arab-route exposures—log (1 + slaves/km^2). The unit of observation is an ethnic–territory group. The second-stage dependent variable is the ethnic–territory share Muslim (WRD, 2005) in columns (1) - (4); in column (5) it is an indicator equal to one if the ethnic group is majority Muslim. Column (4) limits the sample to ethnic territories outside the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Country fixed effects are included in columns (2)–(5). Columns (3) - (5) include geographic and historical controls. Robust standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A14: Arab Slave Trade Exposure and Muslim Share (2005): Conley Spatial Standard Errors

	% Muslim				Maj. Muslim
	(1)	(2)	(3)	(4)	(5)
				Muslim Empire = 0	
Arab	0.194***	0.112***	0.095***	0.077***	0.104***
	(0.062)	(0.035)	(0.025)	(0.021)	(0.023)
Atlantic	0.021	-0.032*	-0.013	-0.036	-0.036
	(0.045)	(0.020)	(0.030)	(0.031)	(0.026)
Distance to Trade			-0.344***	-0.247***	-0.305***
			(0.084)	(0.078)	(0.082)
Land inequality			0.038***	0.022*	0.045***
			(0.010)	(0.011)	(0.006)
Observations	629	629	573	390	573
Countries	48	48	48	42	48
Country FE	No	Yes	Yes	Yes	Yes
Geo Controls	No	No	Yes	Yes	Yes
Historic Controls	No	No	Yes	Yes	Yes

Notes: OLS estimates of equation (1). The unit of observation is an ethnic–territory group. The primary explanatory variables are Arab-route exposures—log (1 + slaves/km^2). *Distance to trade* captures the great–circle distance from the ethnic centroid to the nearest major caravan or maritime route circa 1800 (Brice and Kennedy, 2001). *Land inequality* captures within–territory land–suitability inequality for each Murdock (1959) ethnic territory. In columns (1)–(4), the dependent variable is the ethnic–territory share that is Muslim in WRD (2005); in column (5) it is an indicator equal to one if the ethnic group is majority Muslim. Country fixed effects are included in columns (2)–(5). Geographic controls are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization, and the density of Christian missions during the colonial period. Column (4) excludes ethnic territories inside the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Conley (1999) spatial standard errors with a 1,000 km cutoff in parentheses. Results are robust to limiting to controls selected through a double LASSO procedure detailed in Table A8. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A15: Neighbor Slave Trade Exposure and Muslim Share (2005) - Heterogeneity by Relative Tsetse Suitability, with Tsetse Climate Controls

	% Muslim				
	(1)	(2)	(3)	(4)	(5)
	All Arab routes			Sah. + Red Sea (cavalry)	Indian Ocean (no cavalry)
	Diff	Diff > p50	Diff > p75	Diff	Diff
Neighbor Exposure	0.174**	0.318**	0.282**	0.166***	-1.063
	(0.066)	(0.100)	(0.100)	(0.041)	(0.315)
Tsetse Advantage	0.037			0.061**	0.012
	(0.030)			(0.024)	(0.019)
Tsetse Adv. × Exposure	-0.104	-0.229	-0.218*	-0.155*	2.013
	(0.108)	(0.097)	(0.096)	(0.094)	(0.525)
Neighbor Tsetse	0.011	-0.006	0.004		
	(0.038)	(0.028)	(0.028)		
Atlantic	-0.013	-0.010	-0.010	-0.032	-0.010
	(0.029)	(0.027)	(0.027)	(0.030)	(0.028)
Human Mobility Index	0.001*	0.001*	0.001	0.002**	0.000
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Distance to 1800 routes	-0.207**	-0.199*	-0.209**	-0.247***	-0.217***
	(0.101)	(0.101)	(0.101)	(0.084)	(0.080)
Observations	428	428	428	481	466
Exposed observations	131	131	131	89	53
Clusters	42	42	42	44	42
R^2	0.637	0.642	0.643	0.588	0.640
Country FE & full controls	Yes	Yes	Yes	Yes	Yes
Tsetse Climate Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates on the full continental sample. The unit of observation is an ethnic–territory group and the dependent variable is the ethnic–territory share Muslim (WRD, 2005). *Neighbor Exposure* is the mean slave trade exposure of neighboring territories—log (1 + slaves/km^2)—for the route(s) in the column heading. *Tsetse Advantage* is the group's tsetse suitability Alsan (2015) minus the neighborhood mean: raiders operating next door are impeded by the suitability gradient at the border, and differencing removes the smooth continental gradient. Columns (1)–(3) pool the Arab routes, using the continuous advantage and indicators for its 50th and 75th percentile (indicator level terms estimated but not shown). Columns (4)–(5) split by raiding technology: the trans-Saharan and Red Sea routes used mounted raiders, the Indian Ocean route porters on foot. Each column drops territories with own-group exposure to the route(s) in question; columns (1)–(3) also control for mean neighbor suitability. All columns additionally include the climate inputs to the suitability index—temperature, relative humidity, their first-order interaction, and the share of the homeland in the tropics—so that the identifying variation in tsetse suitability is the nonlinear temperature–humidity interaction. All specifications include country fixed effects and the full set of geographic and historical controls described in Section 3.4. Standard errors clustered at the country level in parentheses throughout. For the three treatment rows, stars are assigned from wild-cluster bootstrap p-values (boottest; Rademacher weights, 999 replications, null imposed) and so need not agree with the ratio of the coefficient to its standard error; remaining stars are analytic. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A16: Historical Trade Route Exposure and Religious Attitudes (individual-level analysis)

	(1) Women Dec. Veil	(2) Relig. Educ.	(3) Relig. Media	(4) Interfaith Marriage	(5) Death Apostasy	(6) Know Islam	(7) Very Relig.	(8) Trust Other Relig.
Panel A: Full Sample								
Distance to 1800 Routes	-0.179	-0.003	0.068	-0.051	0.016	0.086	0.049	-0.328**
	(0.123)	(0.125)	(0.111)	(0.146)	(0.114)	(0.160)	(0.070)	(0.143)
Observations	2,338	1,706	2,554	2,514	2,467	2,558	2,537	2,423
Clusters	100	89	102	102	99	101	102	99
R^2	0.069	0.059	0.050	0.113	0.065	0.098	0.175	0.056
Panel B: $>0\%$ Muslim								
Distance to 1800 Routes	0.060	-0.473	0.282**	0.177	0.003	0.216	0.092	-0.021
	(0.245)	(0.288)	(0.128)	(0.232)	(0.213)	(0.256)	(0.171)	(0.220)
Observations	2,207	1,605	2,414	2,379	2,337	2,418	2,398	2,305
Clusters	74	68	75	75	74	75	75	72
R^2	0.066	0.066	0.051	0.118	0.064	0.095	0.183	0.058
Panel C: Majority Muslims								
Distance to 1800 Routes	0.055	-0.914	0.937*	-0.709	1.184	-0.633	0.106	-0.196
	(0.792)	(0.896)	(0.519)	(0.678)	(0.830)	(0.504)	(0.196)	(0.797)
Observations	1,699	1,253	1,866	1,839	1,807	1,871	1,855	1,783
Clusters	28	27	28	28	28	28	28	27
R^2	0.066	0.083	0.050	0.131	0.077	0.066	0.215	0.063
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Historic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Linear probability model (OLS) estimates. The unit of observation is an individual. The dependent variable is indicated by each column header. The sample is restricted to Muslim respondents and ethnic group territories that experienced zero Arab raiding. Panel A utilizes the full sample, Panel B only ethnic-territories with non-zero Muslim adherence, and Panel C only ethnic-territories with $>50\%$ Muslim adherence (WRD, 2005). Individual data is from Pew's "Tolerance and Tension: Islam and Christianity in Sub-Saharan Africa" (2020 update). All specifications include country and survey-wave fixed effects, geographic and historical controls, and individual controls. Sampling weights are used throughout. Robust standard errors are clustered at the ethnic-territory level in parentheses. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table A17: Arab Slave Trade Exposure and Reported Traditional Beliefs

	Traditional Beliefs					
	(1) Index	(2) Miracles	(3) Witchcraft	(4) Evil Spirits	(5) Juju	(6) Sacrifices
Arab	-0.248***	-0.071***	-0.071***	-0.096***	0.004	-0.010
	(0.081)	(0.023)	(0.025)	(0.028)	(0.016)	(0.021)
Atlantic	0.065	-0.027	0.053*	0.033	0.010	-0.023
	(0.072)	(0.019)	(0.030)	(0.026)	(0.016)	(0.019)
Distance to 1800 Routes	0.036	-0.035	0.099	-0.028	0.082	-0.079
	(0.254)	(0.109)	(0.082)	(0.087)	(0.068)	(0.069)
Observations	3,373	3,658	3,650	3,655	3,628	3,671
Clusters	136	138	138	138	136	139
R^2	0.274	0.080	0.224	0.180	0.220	0.198
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes	Yes
Wealth FE	Yes	Yes	Yes	Yes	Yes	Yes
Indiv. Controls	Yes	Yes	Yes	Yes	Yes	Yes
Geo. Controls	Yes	Yes	Yes	Yes	Yes	Yes
Hist. Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports a separate weighted OLS regression with HDFE (country, education, wealth fixed effects). The sample is restricted to Muslim respondents. Column (1) uses the first principal component of traditional beliefs (miracles, witchcraft, evil spirits, juju, and sacrifices). Standard errors are clustered at the ethnic-territory level. All specifications include geographic, historical, and individual controls. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A18: Arab Slave Trade Exposure and Thematic Flags (Joshua Project)

	Joshua Project Narrative Flags	
	(1) *Abrahamic Roots*	(2) *Social Conserv.*
Arab	-0.026**	0.106**
	(0.011)	(0.045)
Atlantic	-0.004	-0.017
	(0.006)	(0.016)
Distance to 1800 Routes	-0.046	-0.004
	(0.056)	(0.073)
Count of Groups	14	35
Observations	638	638
Clusters	47	47
R^2	0.347	0.268
Country FE	Yes	Yes
Geographic Controls	Yes	Yes
Historic Controls	Yes	Yes
Muslim Share (2005) Control	Yes	Yes

Notes: OLS regressions. The unit of observation is an ethnic–territory group. Dependent variables are binary flags coded from Joshua Project ethnographic narratives. *Abrahamic Roots* = group described as being "people of the book" (*Ahl al-Kitāb*), themselves Muslim Arabs or holding deep-rooted historical ties to Abrahamic religions at the time of the Arab slave trade. *Social Conservatism* = group described as having conservative social norms or gender restrictions. As in the folklore analysis, all specifications condition on the group's contemporary Muslim share (WRD, 2005), so that results are not mechanically driven by differences in the current share of Muslims across ethnic territories. Robust standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A19: The Arab-Route Participation Penalty Is Specific to Muslim Women (individual-level analysis)

	Dependent variable: FLFP	
	(1)	(2)
Arab	-0.052***	0.003
	(0.010)	(0.034)
Muslim		-0.042***
		(0.013)
Arab × Muslim		-0.056*
		(0.033)
Atlantic	0.014**	0.012**
	(0.006)	(0.006)
Distance to 1800 Routes	0.048	0.028
	(0.045)	(0.045)
Arab + Arab × Muslim		-0.052***
		(0.009)
Observations	773,306	749,542
Clusters	142	142
R^2	0.186	0.188
Survey FE	Yes	Yes
Individual Controls	Yes	Yes
Geographic and Historical Controls	Yes	Yes
Pastoralism and Settlement Controls	Yes	Yes
Arab × Muslim	No	Yes

Notes: OLS estimates. The unit of observation is an individual woman aged 15–49. The sample includes women of all religions. The dependent variable is an indicator equal to one if the respondent is currently working or worked in the last twelve months. Arab and Atlantic denote ethnic-group slave export intensity, ln(1 + exports/area); Muslim is an indicator equal to one if the respondent reports Islam as her religion. The coefficient on Arab in column (2) captures the participation penalty for non-Muslim women from raided groups and the interaction captures the additional penalty for Muslim women. The row *Arab + Arab × Muslim* reports the total effect of Arab exposure for Muslim women. Individual data is from all IPUMS-DHS surveys with ethnicity information. All specifications include survey (country-round) fixed effects, individual controls (age, age^2, urban indicator, education dummies, and ever-married indicator), geographic and historical controls, ancestral pastoralism controls (the Becker (2025) herding measure and pastoral and cultivation land suitability), settlement-pattern fixed effects, a large domesticated animals indicator, and plough use (Alesina et al., 2013). Sampling weights are used throughout. Robust standard errors are clustered at the ethnic-territory level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A20: Falsification: Arab Slave Trade Exposure and Muslim Men's Labor Force Participation (individual-level analysis)

	Dependent variable: Male LFP		
	(1)	(2)	(3)
Arab	0.005	0.001	0.004
	(0.004)	(0.004)	(0.006)
Atlantic	-0.002	-0.001	0.013**
	(0.005)	(0.005)	(0.006)
Distance to 1800 Routes	-0.031	-0.015	-0.027
	(0.034)	(0.038)	(0.036)
Observations	99,922	99,061	99,057
Clusters	129	128	127
R^2	0.274	0.275	0.275
Survey FE	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
Geographic and Historical Controls	Yes	Yes	Yes
Pastoralism Controls (Becker)	No	Yes	Yes
Settlement and Animal Controls (Alesina)	No	No	Yes

Notes: OLS estimates. The unit of observation is an individual man aged 15–49. The sample is restricted to Muslim respondents. The dependent variable is an indicator equal to one if the respondent is currently working or worked in the last twelve months. Arab and Atlantic denote ethnic-group slave export intensity, ln(1 + exports/area). Individual data is from all IPUMS-DHS men's surveys with ethnicity information; respondents are matched to Murdock (1967) ethnic groups by reported ethnicity. All specifications include survey (country-round) fixed effects, individual controls (age, age^2, urban indicator, education dummies, and ever-married indicator), and geographic and historical controls. Column (2) adds ancestral pastoralism controls (the Becker (2025) herding measure and pastoral and cultivation land suitability); column (3) further adds settlement-pattern fixed effects, a large domesticated animals indicator, and plough use (Alesina et al., 2013; Ethnographic Atlas). Sampling weights are used throughout. Robust standard errors are clustered at the ethnic-territory level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A21: Arab Slave Trade Exposure and the Female-Specific Employment Penalty (individual-level analysis)

	Dependent variable: LFP	
	(1)	(2)
Arab	0.026*	0.004
	(0.013)	(0.010)
Arab × Female	-0.069***	-0.041***
	(0.015)	(0.011)
Pastoralism × Female		-0.032***
		(0.010)
Arab + Arab × Female	-0.043***	-0.038***
	(0.008)	(0.008)
Observations	454,628	454,628
Clusters	136	136
R^2	0.209	0.209
Survey × Gender FE	Yes	Yes
Individual Controls	Yes	Yes
Geographic and Historical Controls	Yes	Yes
Pastoralism and Settlement Controls	Yes	Yes
Pastoralism × Female	No	Yes

Notes: OLS estimates on the pooled sample of women and men aged 15–49. The sample is restricted to Muslim respondents. The dependent variable is an indicator equal to one if the respondent is currently working or worked in the last twelve months. Arab denotes ethnic-group slave export intensity, ln(1 + exports/area); Pastoralism is the Becker (2025) ancestral-herding measure. Survey-by-gender fixed effects allow each country-round its own male–female employment gap, so the interaction terms are identified from gender gaps varying across ethnic groups within a country-round. The row *Arab + Arab × Female* reports the total effect of Arab exposure for women. Individual data is from all IPUMS-DHS surveys with ethnicity information; respondents are matched to Murdock (1967) ethnic groups by reported ethnicity. All specifications include individual controls (age, age^2, urban indicator, education dummies, and ever-married indicator), geographic and historical controls, ancestral pastoralism controls (the Becker (2025) herding measure and pastoral and cultivation land suitability), settlement-pattern fixed effects, a large domesticated animals indicator, and plough use (Alesina et al., 2013). Sampling weights are used throughout. Robust standard errors are clustered at the ethnic-territory level in parentheses. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A22: Arab Slave Trade Exposure and Muslim Share (Joshua Project)

	% Muslim
Arab	7.051**
	(3.178)
Atlantic	-2.846
	(2.183)
Observations	638
Clusters	47
R^2	0.638
Country FE	Yes
Geographic Controls	Yes
Historic Controls	Yes

Notes: OLS estimates of equation (1). The unit of observation is an ethnic–territory group. The dependent variable is the group's Muslim share, as provided by the Joshua Project. Country fixed effects, and geographic and historical controls are included. Robust standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A23: Arab Slave Trade Exposure and Historical Mosque Presence (PPML)

	Historical Mosques		
	(1)	(2)	(3)
Arab	1.161***	0.857***	0.890***
	(0.120)	(0.121)	(0.148)
Atlantic	0.510***	0.150	0.569**
	(0.147)	(0.239)	(0.282)
Distance to trade			-8.258***
			(1.408)
Land inequality			0.563*
			(0.313)
Observations	629	423	352
Clusters	48	23	22
Country FE	No	Yes	Yes
Geo. Controls	No	No	Yes
Historic Controls	No	No	Yes

Notes: Poisson pseudo-maximum-likelihood estimates of equation 1. Coefficients are semi-elasticities. The dependent variable is the total number of mosques in a given ethnic-group territory, digitized from Pradines (2022). The primary explanatory variables are Arab-route exposures—log (1 + slaves/km^2). The included mosques are limited to those built after the 14th century, as this is when Arab-route exposure data begins. Country fixed effects are included in columns (2)–(3). Geographic controls (col. 3) are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls (col. 3) are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization (jurisdiction beyond the local community), and the density of Christian missions during the colonial period. Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A24: Historical Mosque Presence and Muslim Share (2005)

	% Muslim			
	(1)	(2)	(3)	(4) Muslim Empire = 0
Post-14th c. mosques	0.074***	0.035***	0.037***	0.072**
	(0.008)	(0.006)	(0.005)	(0.035)
Pre-15th c. mosques	0.128***	0.088***	0.029	-0.011
	(0.041)	(0.026)	(0.029)	(0.083)
Atlantic	0.001	-0.035	-0.015	-0.028
	(0.024)	(0.021)	(0.025)	(0.027)
Distance to trade			-0.296***	-0.184**
			(0.079)	(0.087)
Land inequality			0.037**	0.023
			(0.017)	(0.016)
Observations	629	629	573	390
Clusters	48	48	48	42
R^2	0.108	0.521	0.645	0.524
Country FE	No	Yes	Yes	Yes
Geo. Controls	No	No	Yes	Yes
Historic Controls	No	No	Yes	Yes

Notes: OLS estimates. The dependent variable is the share of Muslims in the ethnic territory in 2005 (WRD). The primary explanatory variable is the number of historical mosques in a given ethnic-group territory built after the 14th century—when the slave-trade era begins—digitized from Pradines (2022); the count of mosques built before the 15th century is included as a control, and Atlantic exposure is included in all columns. Country fixed effects are included in columns (2)–(4). Geographic controls (cols. 3–4) are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls (cols. 3–4) are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization (jurisdiction beyond the local community), and the density of Christian missions during the colonial period. Column (4) excludes ethnic territories inside the historical Muslim/Arab empires: groups under historical Muslim rule in sub-Saharan Africa, as identified by Bauer et al. (2022), together with Muslim-majority groups in North Africa. Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A25: Arab Slave Trade Exposure and Historical Mosque Presence (IHS Transformed and Winsorized)

	Historical Mosques (Normalized)		
	(1)	(2)	(3)
Arab	0.034*	0.029*	0.031**
	(0.017)	(0.015)	(0.015)
Atlantic	-0.003	-0.012**	-0.014***
	(0.002)	(0.006)	(0.005)
Distance to trade			-0.066**
			(0.027)
Land inequality			-0.000
			(0.005)
Observations	629	629	573
Clusters	48	48	48
R^2	0.054	0.138	0.180
Country FE	No	Yes	Yes
Geo. Controls	No	No	Yes
Historic Controls	No	No	Yes

Notes: OLS estimates of equation 1. The dependent variable is the total number of mosques in a given ethnic-group territory, digitized from Pradines (2022). These values are modified with an inverse hyperbolic sine transformation and winsorized at the 95% level, then multiplied by 1,000 for readability. The primary explanatory variables are Arab-route exposures—log (1 + slaves/km^2). The included mosques are limited to those built after the 14th century, as this is when Arab-route exposure data begins. Country fixed effects are included in columns (2)–(3). Geographic controls (col. 3) are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls (col. 3) are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization (jurisdiction beyond the local community), and the density of Christian missions during the colonial period. Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A26: Arab Slave Trade Exposure and Historical Mosque Presence (Dummy)

	Historical Mosques		
	(1)	(2)	(3)
Arab	0.116**	0.089**	0.088**
	(0.045)	(0.039)	(0.040)
Atlantic	0.020	-0.038	-0.031
	(0.021)	(0.024)	(0.021)
Distance to trade			-0.265***
			(0.074)
Land inequality			0.003
			(0.019)
Observations	629	629	573
Clusters	48	48	48
R^2	0.031	0.179	0.235
Country FE	No	Yes	Yes
Geo. Controls	No	No	Yes
Historic Controls	No	No	Yes

Notes: OLS estimates of equation 1. The dependent variable is a dummy for whether a mosque appears in a given ethnic-group territory, digitized from Pradines (2022). The primary explanatory variables are Arab-route exposures—log (1 + slaves/km^2). The included mosques are limited to those built after the 14th century, as this is when Arab-route exposure data begins. Country fixed effects are included in columns (2)–(3). Geographic controls (col. 3) are: distance to major caravan/maritime routes ca. 1800, land-suitability inequality, absolute latitude, average land quality, log terrain ruggedness, irrigation potential, a desert indicator, distance to the coast, and distance to Mecca. Historical controls (col. 3) are: indicators for nomadic settlement pattern, dependence on pastoralism, political centralization (jurisdiction beyond the local community), and the density of Christian missions during the colonial period. Standard errors clustered at the country level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$