Domar in Africa

Land Abundance, the Slave Trades, and the Choice of Institutions

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First draft — comments welcome

September 2026

Abstract

Does land abundance explain the choice of institutions in precolonial Africa? This paper deploys a three-step empirical strategy to address reverse causality and to account for slave trade effects. The results show that: (1) before the slave trade era, land abundance predicts African slavery; (2) the slave trades replaced this with a strong co-determination of political centralization and slavery; (3) African polygyny is not an ancient African tradition but spread in the slave trade era; and (4) political decentralization is never explained by land abundance.

JEL codes: B52, D02, N37, N57, Z13

Keywords: Domar Hypothesis, Polygyny, African Slavery, African Demography; Slave Trades, Africanisms

Summary

The Domar hypothesis holds that where land is abundant relative to labor, free labor markets cannot hold workers on the land, so coercive institutions arise instead. It is the traditional theory of institutional choice in African economic history and development economics. In this paper I test whether land abundance explains the choice of three African institutions that were widespread on the eve of colonial occupation: slavery, polygyny, and political decentralization. The test is complicated by the fact that land abundance (proxied by population density) and institutions are jointly determined, and both carry the imprint of centuries of exposure to international slave trades, an important prior history that predates the empirical snapshot used to measure both population density and institutional choice.

The analysis deploys a three-step empirical strategy designed to address these problems. The sample merges data found in the Ethnographic Atlas with GIS environmental data on the African continent and three measures of exposure to international slave trades. First, population density is predicted from purely environmental and geographic characteristics of the continent that cannot themselves be caused by institutions. Second, I remove the component of population density attributable to slave-trade exposure, the residual being a counterfactual population density before slave trade exposure. I then regress this counterfactual population density on the environmental factors to produce an estimate of Domar effects before slave trade exposure. Third, the three measures of slave-trade exposure are added back in a full model to estimate their independent effects on institutional choice net of the population density channel.

The results show that the Domar effect was real but historically contingent: population scarcity before the slave trade era strongly predicts indigenous slavery, but the density channel disappears once the slave-trade era is restored to the picture. In its place, the slave trades produced a strong, direct co-determination of political centralization and slavery. The two institutions move together almost one-for-one, independent of population density. Polygyny, by contrast, increases with population density rather than land abundance, and political centralization is never well explained by population density at any point in the analysis. The results have implications for a variety of literatures and debates in African economic history and development economics.

1. Introduction

The Domar hypothesis is the proposition that the ratio of land to labor determines whether societies rely on free or coerced labor (Domar 1970, building on Nieboer 1900). Where land is abundant and labor scarce, workers can simply walk away from any employer who does not pay a competitive wage, which in this case would be the average product of labor, leaving no profit for the employer. The only way to hold labor on the land and make a profit is to take away their freedom to leave. Where land is scarce and labor abundant, competition for access to land does the disciplining, and free labor markets can begin to spread.

The argument has an old pedigree in African economic history and development economics. Hopkins (1973) built it into the foundations of modern African economic history. Herbst (2000) argues that land abundance weakened precolonial state capacity by raising the cost of broadcasting political authority over dispersed people. Sharman (2026) has recently connected the two, arguing that in land abundant Africa, slavery was the mechanism through which political authority was centralized. Iliffe (2007) reads African polygyny as a population-growth strategy that ties kinship groups to land. Austin (2008) extends the argument to the cost of clearing land, and Fenske (2013a) finds that tighter land markets reduced overlapping property claims among the Egba of Nigeria. In each variant, low population density — land abundance — is doing the explanatory work.

Testing this idea across Africa runs into two problems, both of which this paper is designed to address. The first is ordinary reverse causality: population density and institutions can determine each other. Denser settlement can itself be a product of political centralization, and there is an influential literature that argues precisely this point (Gennaioli and Rainer 2007; Michalopoulos and Papaioannou 2013). Similarly, a successful "wealth in people" strategy in a land-abundant Africa would increase population density through precisely the mechanisms of slavery and polygyny (Domar 1970; Nieboer 1900; Iliffe 2007; Miers and Kopytoff 1977).

The second problem is rooted in the structure of the available data and its ability to control for historical context. The Ethnographic Atlas, our best cross-sectional source on precolonial African institutions and population density, was compiled from ethnographies written around the turn of the twentieth century — after more than three centuries of intermittent, and in some

places intense, exposure to the Atlantic, Indian Ocean, trans-Saharan and Red Sea slave trades. Any population density measured at the end of the slave trade era already reflects whatever demographic and institutional legacies the slave trades left behind. A regression of 1900 institutions on 1900 population density conflates the land-abundance channel that Domar had in mind with a slave-trade channel that may have operated on both variables directly.

This paper separates the two channels (the slave trades and Domar) in three empirical steps. The 1900 population estimates are those found in Murdock's 1959 book, *Africa: Its Peoples and Their Culture History*, where he reports population numbers for most of the African ethnic groups in the Ethnographic Atlas. These were digitized by Alsan (2015), who divides them by the square kilometers of the groups' homelands as depicted in the map at the end of that book, which had previously been digitized by Nunn (2008). First, I estimate the variation in population density due to variations in purely environmental and geographic characteristics of the African continent — including the Tsetse Fly Suitability Index (TSI) of Alsan (2015). None of these factors can themselves be caused by institutional choice.

Second, I remove from the population measures whatever portion is explained by exposure to the slave trades, exposure being proxied by travel time to the nearest slave port (Whatley 2022, 2026) and by the intensity of people captured and exported as slaves (Nunn 2008; Nunn and Wantchekon 2011). This produces an estimate of a counterfactual population density that existed before the slave-trade era. The two population densities are used to test for Domar effects before and after slave trade exposure. Third, I add the measures of slave-trade exposure back into a full model, to estimate their surviving, independent effect on institutional choice net of the population density channel.

The headline result is that the Domar effect predicts the distribution of African slavery before the slave trade era, but exposure to the slave trades produces a strong co-determination of political centralization and slavery that swamps the population density channel once the historical record catches up to 1900. With a baseline, a shock, and an outcome we can begin to theorize about paths of development during the slave trade era and better interpret the historical outcomes. The paper ends by proposing a research strategy designed to extract from this history a uniquely African legacy of freedom.

The main body of the paper is organized as follows: Section 2 discusses what we know about pre-colonial African population history and the effects of the slave trades. Section 3 discusses the Domar hypothesis and five related issues found in African economic history and development economics. Section 4 describes the data used in the analysis. Section 5 describes the 3-step empirical strategy. Section 6 presents the main results. Section 7 reports a robustness check that disaggregates the slavery variable by types of slavery. Section 8 draws out the implications for the view that African political decentralization was a successful Africanism. Section 9 concludes and proposes future research directions.

2. African Population and the Slave Trades

Africa is a large continent, and variation in population density across time and place was almost certainly enormous — as were its social and economic causes and consequences. We know less about Africa's population history than many other parts the world, but we know something about the impact of the slave trades. We know that late nineteenth century Africa was a comparatively low-population place (Kuczynski 1948, 1949), but these and all later population counts are contaminated by slave trade effects. Development economists have relied on McEvedy and Jones' *Atlas of World Population History* (1978) for earlier population estimates, despite its acknowledged thinness and biases (Guinnane 2023). Manning (2014) calls McEvedy and Jones the conservative "post-1978 consensus," claiming the slave trades had little impact on African population growth. When run against the same measures of slave trade exposure used in this study, these population estimates imply that the slave trades reduced Africa's population growth rate by roughly three-quarters of a percentage point (Price and Whatley forthcoming). Manning (1990), using back-cast simulation that account for the demographic effects of the slave trades, concludes that Africa's counterfactual population in the absence of the slave trades would have been double what actually lived in 1850. Later, Manning (2014) reports updates to the simulations showing the slave trades reducing the rate of population growth by 100 percent.

Rare attempts to reconstruct a pre-slave trade baseline population from primary historical documents, like Thornton's (1977) count of unbaptized Kongo newborns circa 1650–1700, are also downstream of slave-trade disruption. The Kongo kingdom was among the earliest to be destabilized by the Atlantic slave trade (Thornton 1983; Heywood 2009), so even a successful reconstruction of seventeenth-century Kongo population would not recover a pre-slave-trade

baseline. Nwokeji (2000) demonstrates how the slave trades altered population density in the Biafran hinterland through migration and cultural assimilation, in addition to conflict and capture. Similar points are made by Nunn and Puga (2012) and Diouf (2003). The broader point is that no available population series for Africa, however carefully constructed, escapes the slave-trade era, which motivates our identification strategy.

If population density in the historical record is contaminated by the slave trades, the empirical strategy needs a source of density variation that predates and is independent of both institutions and slave-trade exposure. The tsetse fly supplies one. The fly (Glossina spp.) is unique to sub-Saharan Africa, transmits trypanosomiasis to livestock and humans, and predates any conceivable agricultural revolution on the continent (Giblin 1990). Where the fly is endemic, draft animals cannot be kept at scale, plow agriculture does not diffuse, and the labor-intensive, hand-hoe technology that remains supports a lower population density. Alsan (2015) develops the Tsetse Fly Suitability Index (TSI) as a lab-based measure of the ecological probability of fly infestation, constructed from insect population growth models and gridded climate data, and is therefore independent of human presence or institutional choice. The TSI and a small set of purely environmental characteristics of the African continent are used to predict population density that could not have been affected by the institutions of slavery, polygyny, or political centralization.

The empirical strategy also removes the effects of slave-trade exposure from Murdock's population densities using three well-known measures of exposure developed by Nunn and Wantchekon (2011) and Whatley (2022, 2026). This produces a counterfactual pre-slave trade population density and allows for tests of Domar effects before the slave trade era. The role and extent of slavery in pre-slave trade Africa have been a contentious issue in African economic history, dating back to the 1960s (Rodney 1966, Fage 1969, 1980, 1989). The problem is the missing baseline. Baseline evidence that has survived — like the ninth-century tribute dispute between the Christian Nubian kingdom of Nobadia and Islamic Egypt (Fauvelle 2013: 28-35), or the fifteenth- and sixteenth-century record of Benin's and Kongo's intermittent embargoes on the sale of captives (Ryder 1969; Thornton 2001) — does not obviously describe slavery as a pervasive, pre-existing institution so much as a managed and, at times, deliberately restricted institution. There is no documented evidence of slavery around Great Zimbabwe, and the Zulu

centralized their political system without it. None of this is evidence against ancient African slavery, only an acknowledgment that we lack a clean historical baseline. This is exactly the gap in the historical record that our empirical strategy is designed to fill statistically.

3. The Domar Hypothesis and Related Literature

An empirical study of institutional choice in precolonial Africa has direct implications for a wide range of literatures. The Domar hypothesis has become the traditional theory of African traditions, in the sense that it is often the implicit and sometime explicit assumption underlying the image of precolonial Africa. As a consequence, the findings have implications for six distinct literatures. This section considers each in turn.

3.1 The Domar Hypothesis and Labor Coercion

Domar (1970) formalized an argument implicit in Nieboer (1900): where land is abundant relative to labor, free labor markets cannot hold workers on the land at a competitive wage, so landowners turn to non-market coercion — slavery or serfdom — to secure a labor force. The hypothesis was long thought to be disproven for its original application. Postan (1966) and North and Thomas (1973) argued that rising land-labor ratios after the Black Death coincided with the decline, not the rise, of serfdom in Western Europe. Domar (1970) criticizes Nieboer for missing the importance of the power to collude among land owners. More recent work presents evidence that the conjecture holds once political and institutional confounds are properly controlled. Acemoglu and Wolitzky (2011) provide the modern theoretical foundation, modeling coercion as a principal-agent problem in which an employer's use of force substitutes for a worker's outside option. Klein and Ogilvie (2017), studying more than eleven thousand Bohemian serf villages in 1757, find that higher land-labor ratios robustly increased coercion once serfdom is studied within a single political-institutional environment rather than across different political systems.

This study's three-step empirical strategy —purging the slave-trade component, predicting density from ecology, and only then testing the Domar effect — is best understood as an African application of the same methodological lesson. In this case, the cross-sectional correlation between land abundance and coercion is confounded by more than three and a half centuries of exposure to international slave trades. An underlying Domar mechanism is recoverable once that confound is addressed.

3.2 Slavery in Africa

The vast majority of the evidence on African slavery, except Vansina (1990) and similar linguistic reconstructions, is based on post-slave trade observations confounded by precisely the kinds of slave trade effects this study is designed to isolate. Miers and Kopytoff (1977) established the foundational anthropological framework, arguing that slave status in most precolonial African societies existed on a continuum of kinship, clientship, and dependency rather than as a sharply bounded property relation like Atlantic chattel slavery — a point Henn and Robinson (2025) invoke in explaining why slavery could be absorbed into, rather than contradict, a kinship-centered social order. Klein (1998) extends the analysis into the French West African colonial encounter and a materialist anthropology of slavery as a mode of appropriating persons rather than land. Stilwell (2014) offers a recent synthesis along these lines spanning the precolonial, colonial, and abolition eras.

Meillassoux (1991) argues the exact opposite -- that kinship is family and slaves are captured outsiders, and once captives are introduced into kinship systems the social structure changes dramatically, similar to the "first problem of slavery" emphasized by Patterson (1982) and developed in Whatley (2026). Cooper (1979) cautions against Africanists downplaying the exploitation and inequality in African slave systems. Sharman (2023, 2026) develops the political implications: labor scarcity produces chronic risk of political decentralization, so internal authority relies on strengthening external networks that tie outsiders to insiders, like slaves.

None of these studies isolate a baseline Domar effect from a slave trade effect. Lovejoy's *Transformations in Slavery* (2000 [1983]) provides the standard synthesis of the historical literature on slave trade effects, emphasizing how varied they were by region. Whatley (2022, 2026) presents econometric evidence on how the scale and character of African slavery changed with exposure to international slave trades. Those findings motivate this paper's own three-step design, and can be read as a more rigorous econometric sequel designed to isolate a baseline, pre-slave trade Domar effect, and then add slave trade effects.

3.3 Political Centralization and Long-Run Development

A third influential literature finds that precolonial political centralization in Africa impacted post-colonial subnational development, but not always in the same direction. Gennaioli and Rainer (2007) and Michalopoulos and Papaioannou (2013) are the foundational cross-ethnic-group studies, finding that areas governed by more centralized precolonial polities have higher future incomes, better public goods, and higher educational attainment. More recent work qualifies this centralization-development link. Archibong (2019) finds the opposite in Nigeria, where colonial authorities punished centralized African societies that resisted occupation. Wilfahrt (2022) shows that precolonial administrative overlap, not political centralization per se, shapes redistributive politics in contemporary Senegal, the complement of the heightened conflict observed when colonial boundaries divided precolonial administrative units (Michalopoulos and Papaioannou 2016). Acemoglu, Reed, and Robinson (2014) show that centralized chieftaincy in Sierra Leone can entrench elite capture rather than deliver broad-based development.

Land abundance is often invoked to explain the pattern of political centralization in precolonial Africa, or lack thereof. Herbst (2000) offers the classic state-capacity account of why African states struggled to "broadcast power" over low-density, difficult terrain. Osafo-Kwaako and Robinson (2013) provide an early systematic mapping of precolonial centralization across Africa, but they do not find a robust correlation between population density and centralization — a null result this paper helps explain. Dincecco, Fenske and Onorato (2019) ask "is Africa different" from the Old World, where conflict produced high fiscal capacity and low civil conflict, but in Africa produced "special-interest states" with high fiscal capacity and high civil conflict.

This paper contributes to this literature by showing that centralization's other historical correlate —slavery — is not a separate independent institution, like in Bezemer et al. (2014). Instead, slavery is shown to be co-determined with centralization through a common historical event -- exposure to international slave trades. This finding complicates any one-sided reading of the long-term benefits of political centralization because the channel is confounded by the negative long-term effects of slavery.

3.4 Colonial Rule, Customary Authority, and Institutional Persistence

Slavery in Africa did not magically disappear with colonial occupation (Miers and Roberts 1988). While this study does not confront the abolition literature directly, it does identify the historical sources of polygyny, centralization and the kinds of slavery encountered by colonizers at the end of the slave trade era: centralized vs. decentralized; hereditary vs. non-hereditary; recent vs. ancient. All of these factors influenced how the legacy of land-abundance, slavery and the slave trades would persist into the colonial era and beyond.

Lugard's (1922, chapter 18) own colonial-administration treatise is a primary source for the British colonial doctrine, implemented unevenly across Africa: manage rather than abolish slavery outright by withdrawing legal recognition and support for slaveholders' property claims, but did not formally outlaw the institution, a model that had been tried and tested in Western European empires for centuries (Whatley 2026, pp. 62-67). Centralized societies, like the Sokoto Caliphate in northern Nigeria, were better equipped to resist abolition pressures, and since African slavery was often embedded in family and kin systems, colonists were reluctant to adjudicate family conflicts. Chiefly, or customary, authority filled the void.

Mamdani (1996) frames this type of colonial rule as a "bifurcated state" that governed Africans through a distinct system of customary law and chiefly authority, a mode of rule he terms "decentralized despotism." Chanock (1985) shows, for Malawi and Zambia, that much of what came to be called customary law was itself a colonial-era construct, shaped disproportionately by the interests of male elders defending the authority they were losing under colonial occupation, including their authority over slaves, wives, and dependents. Berry (1993) extends this to land tenure, showing that "custom" has functioned throughout the colonial and postcolonial periods as a continually renegotiated (rather than fixed) basis for authority. The chapters in Comaroff and Comaroff (2018) argue that the late 20th century Washington consensus on the benefits of decentralization gave customary authority a new lease on life. Baldwin (2015) shows that this same institution of chieftaincy can, under the right conditions, improve democratic responsiveness in the present day. All of this literature argues that customary authority was shaped by the slavery, polygyny and political centralization that this paper explains, the co-determination of slavery and centralization being the most important.

3.5 Polygyny, Demography, and Development

Iliffe (2007) reads African polygyny as a deliberate population-growth strategy in a land-abundant environment operating through kinship and marriage to tie population to the land. Boserup (1970) was the first to systematically link together African farming systems, the sexual division of agricultural labor, and polygyny, arguing that female farming systems increase the economic value of additional wives. Teso (2019) offers a slave trade mechanism, showing that the male-biased sex ratio of trans-Atlantic slave exports caused surviving African women to take up traditionally male economic roles. Dalton and Leung (2014) link the sex-ratios of exports directly to polygyny, showing that the Atlantic trade's disproportionate export of men, and the Indian Ocean trade's disproportionate export of women, predict contemporary polygyny rates at the ethnic-group level. Nwokeji (2001) interprets the substantial regional and temporal variation in the export ratios as reflecting African supply-side conceptions of gender and kinship that influenced who would be retained and who would be exported.

There are long-term effects here too. Gobbi, Hannusch, and Rossi (2026) show that sub-Saharan Africa's fertility transition has stalled well below what income and mortality trends would predict, arguing that family institutions — marriage and inheritance customs, including polygyny — are a major missing explanatory factor. Hertrich (2017) shows that delayed female age at marriage is one of the most reliable early signals of the beginning of a fertility transition in Africa. Polygyny depresses age at marriage because it increases the demand for young brides. At the household level, co-wife competition for access to husband's wealth blunts the fertility effects of free contraceptive services. (Rossi 2019; D'Exelle et al. 2023, Dupas et al. 2025)

Tertilt (2005, 2006) shows in a calibrated model that eliminating polygyny would substantially raise savings and output by eliminating the unproductive bridewealth invested in wives and daughters. Since bridewealth, patriliny and polygyny tend to be bundled together, polygyny bans have generally been ineffective because they create powerful losers (White and Burton 1988; Schoellman and Tertilt 2006; Lawson et al. 2015). Fathers of daughters lose the bride-price income they depend on for old-age security, and women lose access to the wealth of potential husbands. Tertilt et al. (2022) argues that transferring marriage decisions from fathers to daughters is a more effective long-term strategy, but only if tied to general improvements in economic opportunities for women. Education alone produces mixed results, sometimes

decreasing polygyny, sometimes increasing it and sometimes having no effect at all (Fenske 2015; Becker 2022).

To summarize, polygyny persists in Africa as a structural institution resistant to change, one that generates incentives to increase fertility rates today. This study finds little evidence that African polygyny is an ancient African "tradition." There is no evidence of a Domar effect for polygyny, and inconclusive evidence that polygyny spread with population growth before the slave trade era. The robust finding is that polygyny spread during the slave trade era, so if it is an African tradition, it is a tradition born of that era.

3.6 Political Decentralization as Choice

A recent study by Henn and Robinson (2025) reframes African political decentralization from a residual absence of state capacity into a positive institutional choice. Vansina (1990), studying Central Africa, argues that Bantu-speaking communities "refused centralization" to safeguard local autonomy. Henn and Robinson (2025) generalize this claim to the entire continent, documenting an estimated 45,000 independent polities circa 1880 — at most 2% of them classifiable as states — and arguing that this extreme decentralization was deliberately organized and, on its own terms, successful.

This study engages this view directly. It finds narrow support but broad exception to the view that political decentralization is evidence of an Africanism, as if a window into a timeless African past. Since exposure produced a co-determination of centralization and slavery, a successful African strategy would have been resistance to centralization *and* slavery. Any legacy of this strategy, therefore, should be retrievable from among the decentralized societies that remained free of slavery of any kind. The conclusion identifies these societies and proposes a research strategy designed to recover the legacy.

4. Data

The empirical analysis merges three well-established data sources. Murdock's *Ethnographic Atlas* (EA), published in Ethnology between 1962 and 1980 and digitized by Gray (1999), codes over one hundred variables describing roughly 1,267 societies worldwide as of the late nineteenth and early twentieth centuries, several hundred of them in sub-Saharan and North

Africa. The institutional measures are drawn directly from the EA: the presence of polygyny, "ever slavery," [1] and the often-used definition of political centralization (large chiefdom and above). Added to the TSI are other geographic and ecological controls — absolute latitude, mean altitude, coastal access, river access, mean daily humidity and temperature, and an index of agricultural suitability.

Measures of exposure to slave trades come from Nunn (2008), Nunn and Wantchekon (2011) and Whatley (2022, 2026). They rely heavily on the Trans-Atlantic Slave Trade Database (TSTD) and its records on 35,000 individual slave trade voyages. Nunn (2008) uses samples of enslaved Africans in the Americas (coded to 229 distinct African ethnic origins) to allocate international slave exports to specific ethnic homelands depicted in the EA map. This merge is the source of the ethnicity-level slave-export intensity measure used below.

Table A1. Summary Statistics, Core Variables (n=274)

	Mean	SD	Min	Max
Indigenous slavery (0/1)	0.832	0.374	0.000	1.000
Hereditary slavery (0/1)	0.325	0.469	0.000	1.000
Type of slavery (0-4 scale)	2.872	1.049	1.000	4.000
Polygyny (0/1)	0.810	0.393	0.000	1.000
Political centralization (0/1)	0.343	0.476	0.000	1.000
Log population density, 1900 (Murdock)	1.728	1.643	-5.717	5.060
Log population density, 2000	3.157	1.869	-4.605	6.193
Tsetse Suitability Index (TSI)	0.053	0.947	-3.119	1.495
Absolute latitude	9.726	7.358	0.000	36.000
Mean altitude (m)	0.335	0.335	0.001	1.676
Coastal (0/1)	0.172	0.378	0.000	1.000
River access (0/1)	0.613	0.488	0.000	1.000
Mean daily humidity	57.425	15.275	20.932	83.959
Mean daily temperature	24.640	2.951	14.686	29.400
Agricultural Suitability Index	0.529	0.195	0.000	0.885
Date of observation (year)	1917.847	21.810	1830.000	1960.000
Ever slave-trade exposure (0/1)	0.500	0.501	0.000	1.000
Path distance to ports (days)	20.836	16.752	0.793	80.189
Path distance to Sahel (days)	30.900	20.750	2.595	119.878
ln(1+slave exports per km2)	0.229	0.620	0.000	3.714
High Gods (0-4 scale)	1.489	1.488	0.000	4.000

Notes: All statistics computed on the n=274 ethnic-group sample used in Table 2 (the intersection of non-missing outcome, density, slave-trade, geographic, and cross-institution variables). Binary variables' mean equals the sample proportion.

[1] Murdock (1967) defines this variable (Column 71, p. 166) as: "Slave status is here treated entirely independently of both class and caste status. Its forms and prevalence are indicated by the following symbols: H — Hereditary slavery present and of at least modest social significance. I — Incipient or nonhereditary slavery, i.e., where slave status is temporary and is not transmitted to the children of slaves. O — Absence or near absence of slavery. S — Slavery reported but not identified as hereditary or nonhereditary."

The other measures of exposure come from (Whatley 2022, 2026), and measure proximity to major slave ports. Rather than straight-line distance, exposure here is travel-time, a measure that captures variations in landscape, river velocity, headload norms, and local knowledge of routes. Travel time outperforms straight-line distance and recovers slave-trade effects that distance misses (Whatley 2022). Two travel-time measures are used here: average travel times to the nearest major Atlantic or Indian Ocean slave port, and average travel time to the nearest major slave ports in the Sahel, capturing exposure to both the trans-Saharan and Red Sea slave trades. Finally, the Tsetse Fly Suitability Index (Alsan 2015) is merged in at the ethnicity level. The estimation sample across the three regressions in Table 2 is 274 ethnic groups with 84 distinct ethnolinguistic clusters.[2] Table A1 reports descriptive statistics.

5. Empirical Strategy

The empirical analysis proceeds in three linked steps. Since institutional choice can itself affect population density, isolating the effect of density on institutions requires first identifying exogenous determinants of population density, and only then examining how the predicted population density relates to institutional choice.

Step 1: Environmentally predicted population density.

First, the 1900 log population density is regressed by OLS on the Tsetse Fly Suitability Index and the environmental controls, none of which can be caused by institutional choice. The fitted value from this regression -- environmentally predicted population density -- is then used as the sole regressor in a probit of each institution (polygyny, indigenous slavery, political centralization), circa 1900. Because the second-stage regressor in the probit is itself an estimated quantity, inference relies on a paired bootstrap that re-estimates both stages together in each of 1,500 resamples.

Formally, with i indexing ethnic groups, the first-stage regression and second-stage probit are:

$$\ln(Density_i) = \alpha_0 + \alpha_1 TSI_i + \alpha_2 Latitude_i + \ldots + \alpha_8 AgSuitability_i + \varepsilon_i \qquad (1)$$

[2] The two-stage predicted-density regressions in Table 1 use slightly larger, outcome-specific samples of 277–315 groups, since they do not require the full cross-institution and slave-trade-block variables simultaneously.

$$\Pr(Institution_{k,i} = 1) = \Phi(\gamma_k \hat{D}_i) \quad (2)$$

Where $\hat{D}_i$ = fitted value of (1) ≡ environmentally predicted density, and for each institution k ∈ {Polygyny, Slavery, Centralization}, with bootstrap standard errors on γ_k, since $\hat{D}_i$ is itself a generated regressor.[3]

Step 2: Predicted density net of slave-trade exposure.

The predicted density from Step 1 still retains centuries of demographic disruption from the international slave trades. To purge this, population density is first regressed on the measures of exposure to slave trades. The residual is retained as "population density net of slave-trade effects." This residualized population density is then run back through Step 1's environmental regression to produce a counterfactual "pre-slave-trade" population density, which is again used as the sole regressor in a probit of each institution, with inference by a three-stage pairs bootstrap. The difference between the Step 1 and Step 2 coefficients isolates how much of the 1900-era Domar effect is attributable to the slave trades' direct imprint on density.

Formally, the purge and re-estimation are:

$$\hat{D}_i = \delta_0 + \delta_1 PortTime_i + \delta_2 SahelTime_i + \delta_3 \ln(1 + Exports_i/Area_i) + \nu_i \quad (3)$$

$\hat{\nu}_i$ (residual) run back through eq. (1) $\rightarrow \tilde{D}_i \equiv$ pre-slave-trade predicted density (4)

$$\Pr(Institution_{k,i} = 1) = \Phi(\gamma_k' \tilde{D}_i) \quad (5)$$

with inference again by a paired bootstrap over the full three-equation chain (3)–(5). The difference between the Step 1 and Step 2 coefficients ($\gamma_k - \gamma_k'$) isolates how much of the 1900-era Domar effect on institution k is attributable to the slave trades' direct imprint on population density.

Step 3: The independent effect of slave-trade exposure.

Steps 1 and 2 route all slave-trade influence through population density. Step 3 instead returns to the full 1900 cross-section and asks what predicts each institution directly. Each institution is regressed by probit on (1) log population density; (2) the other two institutional indicators

[3] I thank Claude for pointing this out.

(allowing the three institutions to be jointly, contemporaneously correlated); (3) a dedicated slave-trade block consisting of proximity to slave ports (defined as the negative of the corresponding travel time) and slave-export intensity; (4) a dedicated block of environmental effects that includes the Tsetse Fly Suitability Index, the other geographic controls, the date of observation and broad region fixed effects (North, South, East, and Central, with West Africa as the reference category).[4]

Formally, for each k ∈ {Polygyny, Slavery, Centralization}:

$$\Pr(Institution_{k,i} = 1) = \Phi(\theta_{k,D}\ln(Density_i) + \Sigma_{j\neq k}\theta_{k,j}Institution_{j,I} + \theta_{k,port}PortProx_i + \theta_{k,sahel}SahelProx_i + \theta_{k,exp}Exports_i + \theta_{k,TSI}TSI_i + \boldsymbol{X}_i'\theta_{k,X} + \theta_{k,yr}Year_i + \boldsymbol{R}_i'\theta_{k,region}) \quad (6)$$

where $PortProx_i$ and $SahelProx_i$ are the negatives of travel time to the nearest coastal and Sahel slave ports; X_i collects the remaining geographic controls; and R_i is the vector of region-of-Africa indicators. Standard errors are clustered by ethnolinguistic affiliation.

Notation Summary

$Density_i$	1900 log population density for ethnic group i (Murdock).
$Institution_{k,i}$	Binary indicator for institution k ∈{Polygyny, Slavery, Centralization} for group i, circa 1900.
TSI_i	Tsetse Fly Suitability Index for group i's territory.
$PortTime_i$ / $SahelTime_i$	Average travel time to the nearest coastal slave port / nearest Sahel slave port.
$Exports_i$	log(1 + slave exports per square kilometer) attributable to group i's territory.
D_i	Environmentally predicted density (eq. 1), still slave-trade-contaminated.
$\tilde{D}_i$	Pre-slave-trade predicted density (eq. 4), purged of slave-trade exposure.
γ_k , γ_k'	Step 1 / Step 2 coefficients on predicted density; $\gamma_k - \gamma_k'$ is the slave-trade-attributable share of the raw Domar effect.
$\Phi(\cdot)$	Standard normal CDF (probit link function).

[4] State Fixed Effects were tried, but some state had no variation and reduced the sample size. The smaller State Fixed Effects model did not change the results in any way.

6. Results

6.1 Domar Effects Before and After the Slave Trades

Table 1 reports tests of the Domar Hypothesis before and after the slave trades. The result for slavery in the counterfactual baseline is exactly the Domar prediction. Land abundance predicts

Table 1. Domar Effects Before and After the Slave Trade Era

	Polygyny	**Indigenous Slavery**	**Political Centralization**
Predicted density, post-slave-trade (circa 1900)	0.603*** (0.147)	-0.197 (0.175)	-0.163 (0.121)
Average marginal effect	0.148	-0.048	-0.059
Predicted density, pre-slave-trade (net of slave-trade exposure)	0.430* (0.226)	-0.549** (0.235)	0.080 (0.154)
Average marginal effect	0.115	-0.130	0.029
N	307 / 310	277 / 280	312 / 315

*Notes: Probit coefficients; bootstrap standard errors in parentheses (1,500 replications, both estimation stages re-sampled together). *** p<0.01, ** p<0.05, * p<0.10, bootstrap p-values. "Post-slave-trade (raw)" predicts density from TSI and geography alone. "Pre-slave-trade" additionally removes the component of density explained by travel time to the nearest slave port, travel time to the Sahel, and slave-export intensity. Political Centralization coefficients are the sign-reversed complement of the underlying political-decentralization regressions (Centralization = 1 − Decentralization). N reported as post-slave-trade / pre-slave-trade sample size.*

indigenous slavery before the slave-trade era, consistent with Nieboer (1900), Domar (1970), and the view that slavery has deep historical roots in Africa caused by environmental constraints that produced land-abundance. That the effect is invisible after being exposed to slave trades, indicates that the international slave trades masked rather than created the land-abundance channel, foreshadowing the Table 2 finding that by 1900 the determinants of African slavery lie elsewhere.

Polygyny runs in the opposite direction, and its relationship to population density is stronger, not weaker, after the slave trade era. Read together with Table 2 below, where polygyny is also significantly related to proximity to slave ports, the pattern is more consistent with population growth and access to international slave-trades, rather than land abundance. This is more like the kind of labor-intensive, female-farming intensification described by Boserup, or the spread of bridewealth with productivity and income growth, rather than the kind of labor-scarcity logic that motivates the Domar hypothesis for slavery. Finally, political centralization (and by definition

decentralization) shows no relationship to land-abundance in either the raw or the purged specification, again foreshadowing the Table 2 finding that its determinants lie elsewhere.

6.2 Institutional Choice After the Slave Trades

Table 2 returns to the full 1900 cross-section (step 3). Two results stand out. First, population density, once the other institutions and slave-trade exposure are controlled for, no longer predicts slavery (coefficient 0.014, p=0.86), confirming again that the pre-slave-trade Domar effect documented in Table 1 does not survive intact once the historical record is restored to its full, slave-trade-shaped state. In its place, political centralization becomes overwhelmingly the strongest predictor of slavery (coefficient 1.602, p<0.01), and the relationship runs symmetrically in the other direction: slavery is, in turn, the strongest predictor of political

Table 2. Determinants of African Slavery, Polygyny, and Political Centralization, circa 1900

	Slavery	**Polygyny**	**Political Centralization**
Murdock log population density	0.014 (0.078)	0.170** (0.074)	0.097 (0.086)
Polygyny (0/1)	0.014 (0.296)	—	0.432 (0.272)
Indigenous slavery (0/1)	—	0.168 (0.297)	1.721*** (0.310)
Political centralization (0/1)	1.602*** (0.265)	0.455* (0.257)	—
Tsetse Suitability Index (TSI)	0.733*** (0.187)	0.049 (0.218)	-0.460*** (0.169)
Proximity to port/coast (days)	0.028* (0.016)	0.026** (0.011)	-0.009 (0.010)
Proximity to Sahel (days)	0.018 (0.013)	-0.007 (0.012)	-0.003 (0.009)
Slave exports per km², ln(1+exports/km²)	0.085 (0.130)	-0.319 (0.208)	0.128 (0.151)
Geographic controls, date of observation, region FE	Yes	Yes	Yes
N (ethnolinguistic clusters)	274 (84)	274 (84)	274 (84)
Pseudo R^2	0.362	0.346	0.240

*Notes: Probit coefficients from the full specification (Step 3); standard errors clustered by ethnolinguistic affiliation in parentheses. *** p<0.01, ** p<0.05, * p<0.10. Proximity to port/coast and proximity to Sahel are the negative of weighted travel time in days, so higher values indicate closer proximity. All models also include absolute latitude, mean altitude, coastal access, river access, mean daily humidity and temperature, agricultural suitability, date of observation, and region-of-Africa fixed effects (North, South, East, Central; West = reference), not shown.*

centralization (coefficient 1.721, p<0.01). No other variable in either equation comes close in magnitude or precision. Slavery and centralization are, empirically, co-determined — not two separate institutions independently caused by density or by slave-trade exposure, but a single institutional complex that moves together.

Second, the slave-trade block itself retains some direct explanatory power, but selectively. Proximity to coastal ports is a significant, positive predictor of both slavery (p<0.10) and polygyny (p<0.05): societies located closer to slave ports were more likely to practice both institutions, independent of population density and of each other, a result found in Whatley (2022). Proximity to Sahel slave ports and slave-export-intensity are not significant in the full specification for any of the three institutions, suggesting that whatever direct effect exposure had on institutional choice operated mainly through access to coastal slave ports rather than through the volume of its people captured and exported. The Tsetse Fly Suitability Index remains a robust, independent positive predictor of both slavery and political decentralization, echoing Alsan (2015).

7. Robustness Check: Different Types of Slavery

The results reported above collapse the Ethnographic Atlas's four-point slavery variable into a single "any slavery" binary (incipient, non-hereditary, or hereditary vs. absence). This section asks whether the results hold for the narrower, classically "coercive" institution of hereditary slavery, the kind that Whatley (2026: Ch. 3) argues distinguishes slave cultures from slaveholding. Table 3 reruns the setup, replacing "any slavery" with a stricter "hereditary slavery only" binary (hereditary vs. everything else).

Table 3. Domar Effects on Hereditary Slavery

	Any Slavery	**Hereditary Slavery Only**
Predicted density, post-slave-trade (raw)	-0.213 (0.178)	-0.368*** (0.128)
Average marginal effect	-0.052	-0.127
Predicted density, pre-slave-trade (net of slave-trade exposure)	-0.549** (0.232)	-0.500*** (0.179)
Average marginal effect	-0.130	-0.171
N	277	277

*Notes: Probit coefficients; pairs-bootstrap standard errors in parentheses (1,500 replications, re-estimating both stages jointly). AME = average marginal effect. *** p<0.01, ** p<0.05, * p<0.10.*

Table 3 shows that the Domar effect is not an artifact of the broader slavery category. If anything, it is sharper for hereditary slavery. For "any slavery" the coefficient post-slave-trades is statistically indistinguishable from zero (p=0.233), but is significant at the 1% level for "hereditary slavery" (p=0.004), and both coefficients are larger in magnitude. Land abundance predicts the strict, classically coercive form of bondage both before and after the slave trade era.

For the full 1900 specification, three findings stand out. First, the co-determination of hereditary slavery and political centralization survives but weakens considerably: centralization's coefficient in the slavery equation falls from 1.602 ($p<0.001$, pseudo $R^2=0.362$) to 0.655 ($p=0.001$, pseudo $R^2=0.154$) -- still highly significant, but less than half the magnitude, and explaining much less of the variation. Second, polygyny's relationship to slavery reverses. Polygyny and "any slavery" are unrelated in either direction, but the relationship between polygyny and hereditary slavery is significantly negative in both directions. This is consistent with hereditary slavery and polygyny functioning as substitute institutions, rather than as

Table 4. Determinants of Slavery, Polygyny, and Political Centralization: Any Slavery vs. Hereditary Slavery Only

	Any Slavery			Hereditary Slavery Only		
	Slavery	**Polygyny**	**Centralization**	**Slavery**	**Polygyny**	**Centralization**
Murdock log population density	0.0135 (p=0.8615)	0.1700** (p=0.0222)	0.0965 (p=0.2608)	0.0034 (p=0.9650)	0.1631** (p=0.0319)	0.1167 (p=0.1434)
Slavery (0/1)	—	0.1684 (p=0.5714)	1.7206*** (p=0.0000)	—	-0.4379* (p=0.0615)	0.6511*** (p=0.0025)
Polygyny (0/1)	0.0137 (p=0.9632)	—	0.4318 (p=0.1126)	-0.5054** (p=0.0489)	—	0.6695** (p=0.0205)
Political centralization (0/1)	1.6024*** (p=0.0000)	0.4552* (p=0.0770)	—	0.6551*** (p=0.0012)	0.6081** (p=0.0156)	—
Tsetse Suitability Index (TSI)	0.7327*** (p=0.0001)	0.0489 (p=0.8219)	-0.4599*** (p=0.0066)	0.3362** (p=0.0240)	0.1137 (p=0.5939)	-0.2543 (p=0.1210)
Proximity to port/coast (days)	0.0278* (p=0.0826)	0.0259** (p=0.0193)	-0.0086 (p=0.4112)	0.0217** (p=0.0430)	0.0278** (p=0.0142)	-0.0091 (p=0.4120)
Proximity to Sahel (days)	0.0178 (p=0.1723)	-0.0071 (p=0.5655)	-0.0032 (p=0.7342)	0.0164 (p=0.1251)	-0.0045 (p=0.7245)	-0.0042 (p=0.6615)
Slave exports per km², ln(1+exports/km²)	0.0849 (p=0.5146)	-0.3186 (p=0.1248)	0.1281 (p=0.3957)	0.0855 (p=0.4815)	-0.2859 (p=0.1566)	0.1120 (p=0.4488)
Geographic controls, date of observation, region FE	Yes	Yes	Yes	Yes	Yes	Yes
N (ethnolinguistic clusters)	274 (84)	274 (84)	274 (84)	274 (84)	274 (84)	274 (84)
Pseudo R^2	0.362	0.346	0.240	0.154	0.356	0.200

*Notes: Probit coefficients (p-values in parentheses), clustered by ethnolinguistic affiliation. — indicates the variable is the equation's own dependent variable. *** p<0.01, ** p<0.05, * p<0.10.*

complementary, the reverse of Whatley (2022). I treat this as a tentative result because it does not survive several robustness checks reported in the Appendix.

Third, population density remains an insignificant predictor of slavery in the full model no matter how slavery is defined. By 1900, population density carries no additional explanatory power once centralization and slave-trade exposure are held constant.

A supplementary OLS regression of the severity of slavery (0=absence, 1=incipient, 2=non-hereditary, 3=hereditary) on the same determinants (n=274, 84 clusters, R^2=0.293) confirms the pattern in Table 4 and adds some nuance. Political centralization (β=0.557, $p<0.001$) and TSI (β=0.384, $p<0.001$) increase the frequency and intensity of slavery, while proximity to Sahel ports increased intensity (β=0.0148, $p=0.025$) but not its frequency.

8. *Reframing "Africanisms"*

8.1 The Density-Centralization Null, Reconsidered

This paper's three-step empirical strategy is not only a fix for its own identification problem; it speaks directly to a null result that a substantial literature has treated as settled and consequential: the finding that in Africa population density does not robustly predict political centralization. Osafo-Kwaako and Robinson (2013) and Fenske (2013b) report that contrary to the kind of land-abundance logic long applied to state formation in Eurasia, population density does not predict political centralization in Africa. Henn and Robinson (2025) treat this null as one strand of evidence that Eurasian theories of state formation, applied in reverse, simply do not travel to Africa — consistent with their broader argument that African decentralization requires an entirely different kind of explanation, rooted in a deliberate preference for local autonomy, rather than factor endowments.

This paper's results are consistent with that null: political centralization is never well explained by density at any point in the analysis, before or after purging slave-trade effects. But the paper's results also show that this null should not be read as evidence that factor-endowment logic is absent from Africa's institutional history. Domar applies robustly to slavery before the slave-trade era, which is entirely displaced by a centralization-slavery complex after the slave trades. A researcher working only with raw population data circa-1900 or circa-1880 — including the

HYDE-based 1880 density estimates that Henn and Robinson (2025) plot — will not recover a Domar effect for slavery because more than three and a half centuries of slave-trade exposure had already overwritten it.

8.2 Political Decentralization as Choice

Henn and Robinson's (2025) account of political decentralization treats slavery, and by extension the international slave trades, as an unintended downstream consequence of Africa's decentralized equilibrium: non-cooperative competition among thousands of small, independent polities made supplying the export trade individually rational and collectively disastrous. This paper's co-determination finding suggests a complementary but distinct causal story, running substantially in the other direction: where exposure to slave trades was low, the kind of decentralized and free equilibrium that Vansina (1990) and Henn and Robinson (2025) describe persisted largely intact, but where exposure was high, it was substantially displaced by a different equilibrium built around captive-taking and the kind of centralized authority needed to organize, retain, and defend captives

This has a bearing on a broader interpretive tendency, both within this literature and in the wider Africanist scholarship: the tendency to treat institutional patterns observed at the moment of colonial contact as expressions of a stable, essentially African cultural preference — what one might call, echoing an older usage in African diaspora scholarship, an “Africanism.” The evidence in this paper points the other way. Slavery, political centralization, and polygyny each responded in economically legible ways to specific, exogenous and unevenly distributed, environmental conditions and historical shocks. None of this denies African agency in constructing decentralized, autonomy-preserving institutions where the shock was weak enough to leave them intact. It does mean that the geography of decentralization visible in the Ethnographic Atlas is a joint product of slave trade exposure, not simply a direct window onto a timeless institutional choice.

8.3 Slavery and Freedom in the Two Worlds

For example, Figure 1 shows that the decentralized world was not void of slavery. Non-Hereditary slavery is its modal feature, and the closest in frequency to the centralized world. The sharper contrasts are at the extremes: freedom from slavery is more than twice as common among decentralized societies (19 percent versus 8 percent), while hereditary slavery is almost twice as frequent among centralized societies (46% versus 26%). While different, this is not the kind of stark difference on the slavery/freedom spectrum that Henn and Robinson (2025) would lead us to believe.

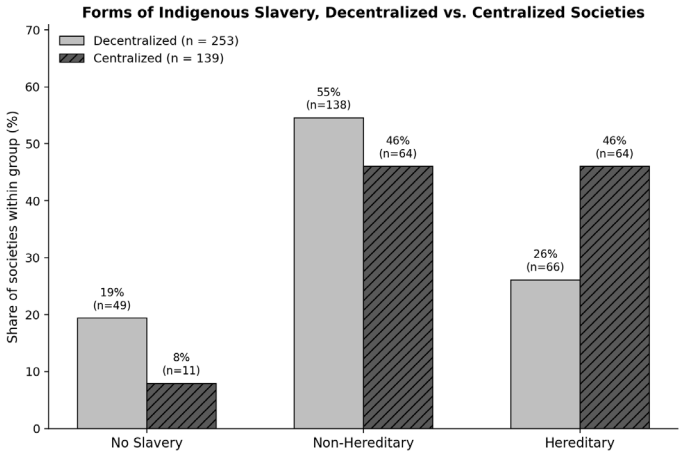

Figure 1. Forms of indigenous slavery, decentralized vs. centralized societies (counts and shares).

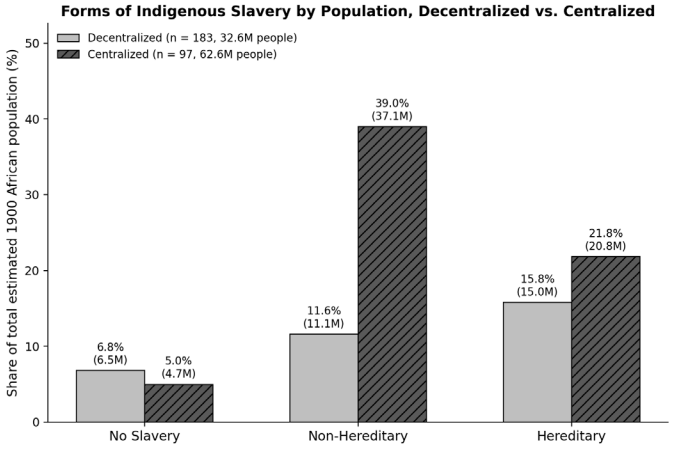

Figure 2. Forms of indigenous slavery by estimated 1900 population, as a share of total African population in the sample (n = 280 societies with population data; grand total ≈ 95.2 million).

After weighting these distributions by their 1900 populations, the picture changes substantially, as Figure 2 shows, but not in favor of Henn and Robinson (2025). Centralized societies are fewer in number (139 versus 253) but because they include several very large kingdoms, they account

for nearly two thirds of the estimated 1900 population (62.6 million versus 32.6 million). The single largest bin for all of Africa is centralized non-hereditary slavery, at 39.0 percent of the total population (37.1 million people), so the near symmetry in the count of polities does not survive in the population shares. The comparisons at the extremes do survive: freedom from slavery remains more prevalent among decentralized populations but by a smaller difference, and hereditary slavery remains more prevalent among centralized population, again, by a smaller difference. The overall implication is that centralized Africa, while a demographic minority of societies, was not a demographic minority of people, and its large states weighed heavily as non-hereditary slave holders.

8.4 Mapping the Decentralized World

Figures 3–6 map the geography of slavery and freedom from slavery in politically decentralized Africa, moving from the broadest category to the most specific. Read together, they show that some kind of slavery was the norm among decentralized societies (81 percent), but its form clusters geographically in a way that a single combined map obscures. Hereditary slavery is concentrated in two distinct pockets: a Sahelian fringe running from Senegal through Mali into Chad, and the Central African interior spanning the Congo Basin, plus an outlying cluster on Madagascar. Non-hereditary slavery, the modal category, is densest exactly where hereditary bondage is thin — the Guinea Coast forest belt and Niger Delta, and the Congo Basin core — indicating that where decentralized societies in this area held slaves, it was overwhelmingly of the incipient, less permanent type rather than a hereditary caste.

The absence of slavery (Figure 6) clusters still differently: overwhelmingly in the East African Rift and Great Lakes highlands (the Kenya–Tanzania–Uganda corridor), with only a thin scattering along the Atlantic coast and in southern Africa. This is the specific geography in which decentralization's "success," in Henn and Robinson's (2025) sense, is most visible on the slavery/freedom margin: a compact East African zone where small-scale, non-centralized communities avoided slaveholding of any kind. The mean Tsetse Fly Suitability Index (TSI) reported on each map confirms the same gradient ecologically: the TSI averages 0.35 among

decentralized hereditary-slaveholding societies and 0.48 among decentralized non-hereditary-slaveholding ones, but falls to -0.38 among decentralized societies free of slavery.

Figures 3–6. Decentralized societies: geography of slavery status (any slavery; hereditary; non-hereditary; no recorded slavery).

Figure 3. Decentralized societies practicing any form of indigenous slavery (n = 204 of 253, 81 percent).

Figure 4. Decentralized societies with hereditary slavery (n = 66 of 253, 26 percent).

Figure 5. Decentralized societies with non-hereditary (incipient) slavery (n = 138 of 253, 55 percent).

Figure 6. Decentralized societies with no recorded slavery (n = 49 of 253, 19 percent).

8.5 Mapping the Centralized World

Figures 7–10 repeat the sequence for centralized societies. The contrast with the decentralized world is sharp on three counts. First, the overall incidence of slavery is higher and more uniform: 92 percent of centralized societies practiced some form of slavery (Figure 7), essentially wall-to-

Figures 7–10. Centralized societies: geography of slavery status (any slavery; hereditary; non-hereditary; no recorded slavery).

Figure 7. Centralized societies practicing any form of indigenous slavery (n = 128 of 139, 92 percent).

Figure 8. Centralized societies with hereditary slavery (n = 64 of 139, 46 percent).

Figure 9. Centralized societies with non-hereditary (incipient) slavery (n = 64 of 139, 46 percent).

Figure 10. Centralized societies with no recorded slavery (n = 11 of 139, 8 percent).

wall across the Sahelian belt, the Great Lakes kingdoms, the Horn, Madagascar, and southern Africa.

Second, the 11 politically centralized societies that were free of any form of slavery were clustered almost entirely in the Sotho–Tswana zone of southern Africa. The mean TSI scores show the same ecological gradient among centralized and decentralized societies, shifted downward throughout: TSI averages -0.36 for hereditary and -0.00 for non-hereditary slaveholders, but falls sharply to -1.47 for the 11 centralized societies with no slavery. This cluster is concentrated in the tsetse-free southern African highveld, where the ecological precondition for the Domar mechanism for slavery was weakest to begin with. This finding feeds directly into the Mfecane aftermath debate about South Africa's origin story, and how the Boer Trek pushed against the free and centralized indigenous population that resided there (Hamilton 1995, Fourie 2021: 83-87, Axelson 1998: 32-36).

Third, the East African Rift corridor, decentralization's clearest "free" zone, was also one of the two densest concentrations of centralized slaveholding (non-hereditary). The same neighborhood produced sharply different norms of freedom and slavery depending on whether the local polity was centralized or decentralized. A spatial clustering analysis reported in Appendix A5 finds

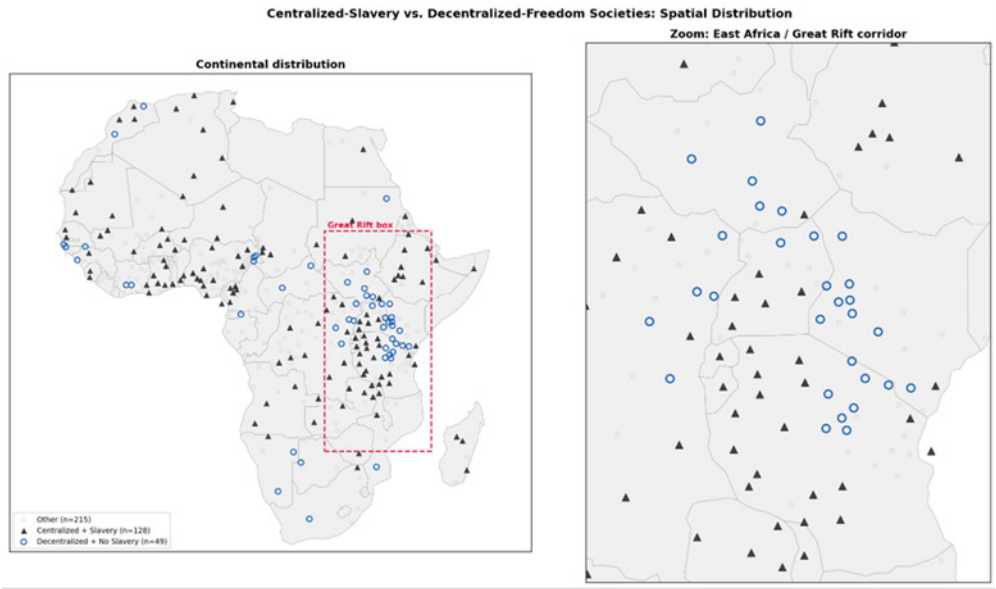

Figure 11. Centralized + slavery (black triangles, n = 128) versus decentralized + no-slavery (blue circles, n = 49) societies. Left: continental distribution, with the Great Rift bounding box outlined. Right: zoom on the East Africa / Great Rift corridor, showing the two types forming distinct, spatially separated pockets rather than an intermingled border zone.

evidence consistent with the free decentralized societies here being closer to each other than to a centralized society. The pattern, seen clearly in Figure 11, is more like a clustering of free refugee societies at a distance from centralized slave societies than like a border between centralized slavery and decentralized freedom.

By the end of the slave trade era, politically decentralized free societies concentrated in the Kenyan and Tanzanian highlands, the South Sudanese Sudd, and the eastern Congo forest zone — terrain long identified in the historical and anthropological literature as defensible ground to which populations withdrew from surrounding predation. This has been framed as a blessing of ruggedness (Nunn and Puga 2012), an "African frontier" of voluntary resettlement (Kopytoff 1987) or, in James C. Scott's (2009) terms, a deliberate retreat from states and slave-raiders' reach into stateless, hard-to-govern terrain. The statistical signature here — tight self-clustering combined with significant distance from centralized-slavery — is what that kind of retreat would be expected to produce. Populations did not simply hold their ground next to expanding slaving states, they moved toward a specific, shared set of locations that put distance between themselves and predators.

Read together, the evidence presented in this section supports a qualified version of Henn and Robinson's (2025) claim. On the slave/free margin, decentralization was a comparatively successful strategy, and the geography of that success is specific and legible -- concentrated in the East African Rift and Great Lakes highlands -- and it operated primarily by keeping the incidence of slavery and hereditary slavery down, not by eliminating it outright. The "success" that Henn and Robinson (2025) document is real, but its geography tracks a specific historical shock and a specific regional pattern rather than being a uniform African achievement.

9. Conclusion and Future Research Directions

This paper develops a three-step empirical strategy to separate the effects of land-abundance and slave trade exposure on institutional choice circa 1900. Population density is predicted from ecology alone and purged of its slave-trade-driven component. The resulting pre- and post-slave-trade population densities are compared as predictors of indigenous slavery, polygyny, and political centralization. A full model then restores direct slave-trade exposure alongside cross-institutional correlations and region fixed effects. The results show that population scarcity

predicts slavery before the slave trade era, but is entirely displaced by a strong, direct, and nearly symmetric co-determination of slavery and political centralization after slave trade exposure. Polygyny is linked to population growth and coastal access rather than to land abundance directly, and political centralization is never well explained by density at any point.

These results have specific implications for the literatures and issues discussed at the outset. The Domar effect was real in the pre-slave trade counterfactual world, implying that slavery in Africa predated the arrival of European and Muslim slave trades, but it was transformed into a co-determination of political centralization and slavery by the end of the slave trade era. This co-determination complicates views of pre-colonial political centralization as a sub-national path to post-colonial economic development.

On the other side of the political divide, political decentralization was found to be a successful strategy for constraining incentives to recognize slavery, but it was not universally successful and followed discernible responses to ecological constraints and historical shocks. Polygyny never followed population scarcity. Its spread followed population increases, access to coastal trade and presumably bridewealth and female farming. The colonial encounter with this kind of institutional diversity across Africa produced the diverse outcomes we call customary authority.

Future research could proceed along econometric lines, but more-informative and more useful results would probably come from focused historical research and randomized controlled trials (RCT). Econometrically, the finding of mutually reinforced slavery-and-centralization is best read as a description of a 1900 institutional equilibrium, not as an estimate of a specific causal effect. The cross-sectional structure of the Ethnographic Atlas data cannot fully adjudicate which institution drove the move to the new equilibrium, even though slavery has environmental roots and centralization does not. The results in Table 2 are consistent with either sequencing. Slave-trade exposure could have driven captive-taking that then required centralized authority to organize, retain, and defend slaves (Whatley 2014, 2022, 2026; Sharman 2023, 2026), or preexisting centralized states could have been better positioned to capture and retain slaves in the first place (Fage 1969, Thornton 2001, Whatley and Gillezeau 2011). The observed correlation is consistent with both channels operating together.

The measures of slave port proximity are natural candidates for instrumenting slavery in the centralization equation, but the exclusion restriction is doubtful: trade access plausibly built centralized military capacity directly, through imported firearms and commercial wealth (Whatley 2018), independent of any effect running through domestic slaveholding. A cleaner test would require either an instrument that affects centralization without a direct slavery channel, like geographic determinants of defensibility, or distance to pre-slave trade state cores (or cities), or genuine time-series variation in the onset of each institution, neither of which a single 1900 cross-section can provide. The co-determination reported here should therefore be read as a robust feature of the data, corroborated qualitatively by centralized slave-raiding states such as Kongo, Asante, Oyo and Dahomey, rather than as a resolved causal estimate in either direction.

A second research direction could deploy randomized controlled trials (RCT) or focused historical reconstructions designed to test for the cultural legacies of freedom from slavery in Africa. The fact that decentralized freedom survived centuries of exposure to international slave trades makes the achievement all the more remarkable. As an example of how this research might proceed, Table 5 presents a list of free-slave pairs: decentralized societies, one slave and one free, but close to each other. An actual deployment of RCTs or historical case studies would require a more precise locating mechanism than the map in the Ethnographic Atlas, and better matching on geographic features like terrain and ruggedness, in addition to TSI. A well-designed project could produce a test of the legacy of African freedom on contemporary measures like trust, similar to the tests of catchment on mistrust found in Nunn and Wantchekon (2011). And as a general test of culture as "the persistence of norms," the normative difference between hereditary slavery and freedom from slavery of any kind is probably one of the cleanest cultural distinctions available to researchers (Whatley 2026: Ch. 2; Graeber and Wengrow 2021: Ch. 5).

Table 5. Slave-Free pairs

Candidate RCT Site Pairs — Nearest Match

Decentralized + No Slavery site paired with its nearest Decentralized + Slavery neighbor (49 pairs, sorted by distance)

Free Society	Free Country	Free TSI	Slave Society	Slave Country	Slave Type	Slave TSI	Distance (km)
Matakam	Cameroon	-0.246	Kapsiki	Cameroon	Non-hereditary	-0.246	36.1
Gude	Cameroon	-0.246	Kapsiki	Cameroon	Non-hereditary	-0.246	37.3
Goroa	Tanzania	-1.425	Rangi	Tanzania	Non-hereditary	-1.425	46.4
Sabei	Kenya		Gisu	Uganda	Non-hereditary	0.582	49.8
Banyun	Guinea-Bissau	-0.378	Balante	Guinea-Bissau	Non-hereditary	-0.378	69.4
Tatoga	Tanzania	-1.425	Rangi	Tanzania	Non-hereditary	-1.425	73.3
Podokwo	Cameroon	-0.795	Kapsiki	Cameroon	Non-hereditary	-0.246	78.1
Sandawe	Tanzania	-0.989	Rangi	Tanzania	Non-hereditary	-1.425	82.7
Logo	Dem. Rep. Congo	0.911	Lugbara	Uganda	Non-hereditary	0.494	92.8
Bari	S. Sudan	0.494	Kuku	S. Sudan	Non-hereditary	0.494	92.9
Teita	Kenya	-0.802	Pare	Tanzania	Hereditary	-0.802	104.4
Chaga	Tanzania		Pare	Tanzania	Hereditary	-0.802	110.7
Gagu	Côte d'Ivoire	1.259	Guro	Côte d'Ivoire	Hereditary	1.259	113
Acholi	Uganda	0.755	Lango	Uganda	Non-hereditary	0.755	115.5
Kumu	Dem. Rep. Congo	1.299	Rumbi	Dem. Rep. Congo	Non-hereditary	-0.701	117.5
Bete	Côte d'Ivoire	1.259	Guro	Côte d'Ivoire	Hereditary	1.259	118
Diola	Senegal	-0.931	Balante	Guinea-Bissau	Non-hereditary	-0.378	118.6
Baga	Guinea	0.205	Susu	Guinea	Non-hereditary	0.205	120.5
Nandi	Kenya	-1.482	Gisu	Uganda	Non-hereditary	0.582	141.4
Lotuko	S. Sudan	0.494	Kuku	S. Sudan	Non-hereditary	0.494	145.8
Nuer	S. Sudan	-0.656	Shilluk	S. Sudan	Hereditary	-0.663	152.6
Bongo	S. Sudan	-0.41	Jur	S. Sudan	Non-hereditary	-0.41	155.2
Kikuyu	Kenya	-0.673	Kamba	Kenya	Non-hereditary	-1.049	156.1
Turu	Tanzania	-0.759	Gogo	Tanzania	Non-hereditary	-0.989	159.1
Bira	Dem. Rep. Congo	-0.701	Budu	Dem. Rep. Congo	Non-hereditary	1.23	159.5
Suk	Kenya		Gisu	Uganda	Non-hereditary	0.582	161.3
Jie	Uganda	0.053	Teso	Uganda	Non-hereditary	0.582	161.8
Fang	Gabon	1.412	Ngumba	Cameroon	Non-hereditary	1.48	168.7
Luo	Kenya	-0.807	Gisu	Uganda	Non-hereditary	0.582	177.8
Kindiga	Tanzania		Rangi	Tanzania	Non-hereditary	-1.425	179.9
Rega	Dem. Rep. Congo	-0.861	Songola	Dem. Rep. Congo	Hereditary	1.301	181.5
Keyu	Kenya	-1.482	Gisu	Uganda	Non-hereditary	0.582	183.5
Masai	Tanzania	-2.136	Rangi	Tanzania	Non-hereditary	-1.425	194.7
Amba	Dem. Rep. Congo	-0.788	Chiga	Uganda	Non-hereditary	-0.875	207.4
Mondari	S. Sudan	-0.237	Kuku	S. Sudan	Non-hereditary	0.494	214.8
Turkana	Kenya	0.622	Teso	Uganda	Non-hereditary	0.582	227.2
Dorobo	Kenya	-1.482	Gisu	Uganda	Non-hereditary	0.582	229.9
Tenda	Senegal		Susu	Guinea	Non-hereditary	0.205	250.7
Sonjo	Tanzania	-2.136	Kamba	Kenya	Non-hereditary	-1.049	266.8
Mandja	Central African Rep	0.523	Baya	Central African Rep.	Hereditary	0.876	284.3
Kara	Central African Rep	-0.798	Banda	Central African Rep.	Non-hereditary	0.608	316
Shluh	Morocco	-1.008	Tekna	Morocco	Hereditary	-0.3	378.7
Bisharin	Sudan	-0.362	Barabra	Sudan	Hereditary	-0.377	385.7
Hlengwe	Mozambique		Sena	Mozambique	Non-hereditary	0.893	509.7
Kung	Namibia	0.206	Herero	Namibia	Hereditary	0.152	510.3
Naron	Botswana	0.262	Herero	Namibia	Hereditary	0.152	663
Rif	Morocco		Mzab	Algeria	Non-hereditary	0.071	729.6
Nama	Namibia	-1.13	Herero	Namibia	Hereditary	0.152	756.1
Xam	South Africa		Herero	Namibia	Hereditary	0.152	1412.1

Appendix: Robustness Checks

This appendix summarizes five additional robustness and validity checks conducted after the main results in Sections 6 and 7 and the map-based analysis in Section 8. Checks A.1–A.4 are documented in full in a companion memo available on request. A.5 reports a spatial-statistics check on the Section 8 map sequence. None of the checks alters the paper's central finding—that slave-trade exposure jointly predicts political centralization and hereditary slavery, net of predicted population density. One secondary claim from Section 7 (a polygyny–hereditary-slavery substitution pattern) does not survive the corrected specification in A.3, and is accordingly presented in the main text as tentative. All Appendix specifications and calculations are performed by/in Claude's Sonnet 5 model.

A.1 Endogeneity Check: Control Function (Rivers–Vuong) Estimator

The paper's plug-in two-step estimator (predicted density net of slave-trade effects, entered as a regressor) assumes the first-stage residual is uncorrelated with the second-stage error conditional on covariates. A Rivers–Vuong control-function estimator relaxes this by including the first-stage residual directly as a regressor and testing its significance. A significant residual indicates the plug-in approach understates standard errors but does not necessarily overturn the point estimate; an insignificant residual indicates the plug-in and control-function estimates should coincide.

Outcome / sample	**OLS-style coef.**	**CF-adjusted coef.**	**CF residual p-value**	**Verdict**
Polygyny (raw density)	0.603***	0.632***	0.000***	Confirmed
Polygyny (purged density)	0.430*	0.172	0.965	Not confirmed
Indigenous slavery (raw density)	-0.197	-0.205	0.020**	Confirmed
Indigenous slavery (purged density)	-0.549**	-0.316**	0.006***	Confirmed
Centralization (raw density)	-0.163	-0.190	0.007***	Confirmed
Centralization (purged density)	0.080	0.021	0.495	Not confirmed

The purged-density effect on indigenous slavery—this paper's central result—survives the control-function check essentially unchanged. The purged-density effects on polygyny and centralization, which the main text already treats cautiously, do not survive; this is consistent with Section 6's treatment of polygyny and centralization as more-weakly and indirectly linked to the Domar channel than slavery itself.

A.2 Adding Malaria Ecology as a Step-1 Control

Because tsetse suitability and malaria ecology both vary with rainfall and vegetation, Step 1's density-prediction equation could be confounding a broader disease-ecology effect with the tsetse-specific mechanism the paper emphasizes. Adding a Malaria Ecology Index to Step 1 increases the first-stage fit (R^2 rises from 0.218 to 0.236, n = 315) while leaving the tsetse-suitability coefficient intact. Malaria itself is only marginally significant ($\beta = 0.027$, $p = 0.073$).

Purged-density effect on	**Baseline (no malaria control)**	**With malaria control**
Polygyny	0.430*	0.596*** ($p < 0.01$)
Indigenous slavery	-0.549**	-0.402** ($p = 0.049$)
Centralization	0.080 (n.s.)	essentially unchanged (n.s.)

Adding the malaria control leaves the paper's core slavery result intact, though it weakens slightly (still significant at 5 percent). It strengthens, rather than weakens, the polygyny result. Tsetse suitability's role as the paper's key geographic instrument is unaffected by controlling separately for malaria ecology.

A.3 Corrected Hereditary-Slavery Specification

The Ethnographic Atlas codes roughly 39 percent of slaveholding societies in the estimation sample as slavery of an unspecified type ("S"), which the paper's main hereditary/non-hereditary indicator originally placed at an intermediate severity level. A corrected specification drops these unspecified cases and defines hereditary slavery using only confirmed hereditary versus confirmed absent/incipient cases, shrinking the sample from 274 to 167 ethnic groups.

Relationship	**Original coding (n = 274)**	**Corrected coding (n = 167)**
Purged density → hereditary slavery	significant at 1%	-0.453** (significant at 5%)
Slavery equation: centralization coefficient	0.655***	0.813*** (strengthens)
Centralization equation: hereditary-slavery coefficient	0.651***	0.801** (strengthens, less precise)
Polygyny–hereditary substitution (slavery eq.)	-0.505**	-0.339 (n.s., p = 0.358)
Polygyny–hereditary substitution (centralization eq.)	-0.438*	-0.315 (n.s., p = 0.383)

The paper's central Domar-mediation result and the centralization–hereditary-slavery co-determination result both survive this correction, and the latter strengthens. The polygyny–hereditary "substitution" pattern, however, does not survive: it is not statistically distinguishable from zero once unspecified-type cases are excluded. That claim is accordingly presented as suggestive rather than robust.

A.4 Corrected Slavery-Severity Scale

A related coding issue affects the paper's 0–3 ordinal severity scale used in Section 6's robustness checks: unspecified-type cases were placed at level 2, between incipient (1) and hereditary (3), despite representing the largest single category in the sample (39 percent of n = 274) and having no documented ordinal relationship to the other categories. A corrected 0–2 scale uses only the 167 groups with confirmed slavery type.

Coefficient	**Original 0–3 scale (n = 274, miscoded)**	**Corrected 0–2 scale (n = 167)**
Centralization	0.557	0.499
Tsetse suitability index (TSI)	0.384	0.388
Model R^2	0.293	0.417
Proximity to Atlantic/Indian Ocean port	weaker, less precise	stronger, more significant
Proximity to trans-Saharan routes	weaker, less precise	stronger, more significant

Both the centralization and tsetse-suitability effects are essentially confirmed under the corrected scale, and overall model fit improves substantially. A diagnostic regression on which groups were originally miscoded as "unspecified type" finds that this coding gap correlates with agricultural suitability ($p = 0.036$) and slave-export intensity ($p = 0.036$), but not with population density, centralization, tsetse suitability, or proximity to trade routes—reassuring evidence that the correction does not introduce a new, systematic bias along the dimensions this paper's identification strategy relies on.

A.5 Spatial Clustering: Is the Refugee Pattern Random?

Section 8's map sequence shows decentralized free societies and centralized slaveholding societies concentrated in the same general East African corridor along the Great Rift. This raises a natural question: does that corridor reflect two institutional types occupying genuinely

adjacent, intermingled territory — consistent with a buffer or border-zone story — or does it instead reflect two institutional types that happen to co-occur within the same region while remaining spatially segregated from one another? This appendix reports a formal spatial test of that question.

Each of the 392 ethnic groups with complete centralization and slavery-type data is classified into one of three categories: "centralized + any slavery" (n = 128), "decentralized + no slavery" (n = 49), or all other combinations (n = 215, mostly decentralized societies that did hold slaves). For each pair of categories, the mean great-circle distance from every society in one category to its nearest neighbor in the other category is computed, along with each category's mean nearest-neighbor distance to itself, a measure of self-clustering. Because the 392 society locations are fixed by history and geography rather than by random sampling, statistical significance is assessed by permutation: category labels are reshuffled across the fixed set of locations 9,999 times, holding the three group sizes constant, and the observed statistic is compared to this null distribution of what the same statistic would look like under purely random institutional assignment. The test is run twice — once for the full continental sample, and once restricted to a Great Rift bounding box (20°S–15°N, 25°E–42°E, n = 128) covering Ethiopia through Malawi and Mozambique.

Sample	Comparison	Observed (km)	Null mean (km)	Null SD	z	p (one-tail)
Continental (n=392)	Decentralized-free → nearest Centralized-slave	292.3	231.8	28.5	2.13	0.9701
Continental (n=392)	Centralized-slave → nearest Decentralized-free	606.2	365.8	35.1	6.85	1.0000
Continental (n=392)	Centralized-slave → nearest Centralized-slave (self)	235.0	232.3	12.9	0.21	0.5761
Continental (n=392)	Decentralized-free → nearest Decentralized-free (self)	259.0	370.1	39.0	−2.85	0.0023
Great Rift box (n=128)	Decentralized-free → nearest Centralized-slave	228.0	194.7	22.4	1.49	0.9208
Great Rift box (n=128)	Centralized-slave → nearest Decentralized-free	440.3	238.9	25.1	8.01	0.9999
Great Rift box (n=128)	Centralized-slave → nearest Centralized-slave (self)	197.6	196.3	14.6	0.09	0.5322
Great Rift box (n=128)	Decentralized-free → nearest Decentralized-free (self)	134.9	242.5	25.9	−4.15	0.0001

p is the one-tailed probability, under 9,999 label permutations, that the null statistic is at least as small as the observed statistic. A low p indicates the observed pattern is more clustered, or closer, than random; a high p indicates it is more dispersed, or farther, than random.

Two results hold in both samples. First, decentralized-free societies are significantly self-clustered — their nearest neighbor is typically another decentralized-free society, at a distance well below what random relabeling of the same locations would produce ($p = 0.0023$ continent-wide; $p = 0.0001$ within the Rift box). Centralized-slave societies show no comparable self-clustering; their nearest-neighbor distances are statistically indistinguishable from a random spatial pattern. Second, and contrary to a simple adjacency or buffer-zone hypothesis, decentralized-free societies are not closer to centralized-slave societies than chance would predict — if anything, they are significantly farther away ($p \approx 0.92$–0.97 and $p \approx 1.00$ on the two directional cross-category tests). The apparent overlap visible in the maps is a same-country effect: Kenya, Tanzania, South Sudan, and the Democratic Republic of Congo each contain sizable numbers of both institutional types, but the specific decentralized-free and centralized-slave locations within those countries form two distinct, non-overlapping pockets rather than an intermingled border zone, as Figure 11 illustrates directly.

This is a descriptive spatial pattern, not a causal test, and two limitations apply. The refuge locations identified here — highlands, swamp, dense forest — are also ecologically distinctive: hard to farm, hard to raid, and hard to build a centralized state around. The same terrain that offered refuge from slaving pressure may therefore independently explain the low population density and the absence of centralized authority, apart from any specific proximity to a slaving neighbor. Distinguishing a “refuge from a nearby threat” mechanism from a “geography independently disfavors both centralization and slavery” mechanism would require terrain-ruggedness and market-access controls this appendix does not yet include, and is flagged as a natural extension of this test. A.5 refines, rather than undermines, the map-based interpretation by showing that the apparent adjacency of decentralized and centralized societies in East Africa is a same-country artifact rather than genuine geographic intermingling.

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