On the Persistent Effects of the Slave Trade on Postcolonial Politics in Africa*

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Abstract

How does the disruption of traditional institutions shape subsequent political outcomes? I argue that the demographic shock to indigenous societies induced by Africa's slave trade influences postcolonial politics by tragically improving ethnic institutions and leadership, thereby affecting the coup-civil war trap and the underlying commitment problems. The empirical analysis leverages the soil suitability for cassava as an instrument to exploit plausibly exogenous variation in the ethnic group-level exposure to the slave trade. The findings are four-fold: Ethnic groups with severer slave trade exposure are (1) less likely to experience battle incidents within their traditional homelands, (2) less likely to fight civil wars against the central government, (3) more likely to be included in state power-sharing schemes, and (4) more likely to stage coups in postcolonial states. Falsification tests exploiting the timing of cassava's arrival in Africa and the regional variation in non-cassava crop suitability lend further credibility to the findings. (150 words)

Keywords
armed conflict, coups, historical legacies, power sharing, slave trade

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Introduction

How do historical events and institutions persistently affect subsequent political outcomes? Previous literature on historical legacies highlights three distinct pathways. First, institutions impose constraints on individual behavior and interactions. Historical institutions and community ties shape contemporary economic performance and armed conflicts by influencing institutional constraints, collective action capacity, and bargaining leverages (e.g., Acemoglu et al., 2001, 2014; Michalopoulos & Papaioannou, 2013; Wig, 2016). Second, the breakdown of historical institutions also matters. Existing literature demonstrates how the breakdown of traditional institutions and community ties alters future economic development, altruistic tendencies, and political attitudes and behavior. Such legacies can be generated and transmitted across generations by the fall of traditional polities (e.g., Lowes et al., 2017) as well as the damages inflicted by forms of political violence (e.g., Bellows & Miguel, 2006; Rozenas & Zhukov, 2019). Third, and relatedly, exogenous shocks facilitate institutional change. For example, the rise of the Atlantic Trade and the technological innovations contributed to the economic growth of West European states after the sixteenth century through direct channels of increased economic profits and indirect pathways of institutional change to assure property rights (Acemoglu et al., 2005).

This article expands the third pathway by exploring the long-run effects of the transatlantic and Indian Ocean slave trades on power sharing, rebellion, and coups d'état in postcolonial Africa. I argue that descendant ethnic groups with severer exposure to the slave trade are less likely to engage in armed conflicts while more likely to be included in the state power-sharing schemes in postcolonial politics due to the tragically improved group-level institutions and leadership authority, with a by-product of the increased risks of insider coups. Existing literature suggests that the slavery shock improved the political authority of local chiefs (Whatley, 2014). Somewhat in parallel with the logic of war and state-making (Tilly, 1975), the intensified competition for survival and the benefits from slave raids also facilitate local chiefs' investments into ethnic institutions to better mobilize group members and prevent free-riding, if not into large-scale territorial states. The similarly increased group members' demands for protection and institutional constraints on strengthened leadership are also attuned to enhanced leadership authority and ethnic institutions. The improved institutions and leadership authority enable an ethnic group to make credible threats and promises, which in turn alleviates the war-causing commitment problems.

Empirically, this article focuses on the later half of the slave trade during the eighteenth to the nineteenth centuries and leverages the soil agricultural suitability for cassava (manioc) as an instrument to exploit plausibly exogenous variation in the ethnic group-level exposure to the slave trade. Although known as a major staple crop today, cassava did not exist in Africa before the Columbian Exchange and was introduced to the continent by the Portuguese in the middle of the slave trade period (Crosby, 1972). The ecological features of cassava include its tolerance to stressful environments, high energy yields, and less input-demanding nature (El-Sharkawy, 2004), as well as the suitability for lengthy travels. The introduction of cassava increased land potential to sustain a larger population and additional food surplus in regions with both higher and lower suitability for other crops while magnifying slave raiders' incentives and capabilities for slave capture in the cassava-suitable regions.

The first-stage results confirm the ability of cassava suitability to predict group-level exposure to the transatlantic and Indian Ocean slave trades during the 1700–1900 (post-cassava arrival) period. The second-stage estimates reveal four empirical patterns consistent with the argument: Ethnic groups with severer historical exposure to the slave trade are (1) less likely to experience battle incidents between armed forces within their traditional homelands, (2) less likely to fight civil wars against the central government, (3) more likely to be included in state power-sharing schemes, and (4) more likely to stage coups in postcolonial states. The main falsification test exploits the arbitrary timing of cassava's arrival in Africa in the middle of the slave trade, and lends further credibility to the findings by demonstrating a systematic first-stage association *after*, but not *before*, the arrival of the New World crop.

This article contributes to broader literature in three ways. First, it speaks to the growing literature on the persistent effects of historical events on modern outcomes, particularly the studies on the historical roots of contemporary civil conflicts. Some historical events such as ethnic partitioning by modern border design escalate postcolonial conflicts (Michalopoulos & Papaioannou, 2016), while other institutions including precolonial political centralization have a pacifying effect (Wig, 2016). In addition to highlighting the long-run effects, this article traces out how traditional ethnic institutions have been affected by prior historical shocks to persistently influence subsequent political outcomes. Second, the findings deepen our understanding of the persistent legacies of the slave trade. Previous literature demonstrates the impacts of the slave trade on, for example, economic growth (Nunn, 2008), traditional political institutions (Whatley, 2014), trust attitudes (Nunn & Wantchekon, 2011), and precolonial conflicts in Africa (Fenske & Kala, 2017). What remains less clear is the impact of the slave trade on postcolonial politics, which this article investigates. Finally, this article carries implications to broader political science literature. Revealing the deep historical roots, the findings directly speak to the ongoing debate over the causes and consequences of power sharing (e.g., Cederman et al., 2010; Roessler, 2011, 2016; Roessler & Ohls, 2018). The long-run political consequences of the slave trade shock also provide another piece of insight into how violence and institutions interact to generate political outcomes.

Historical Legacies and the Coup-Civil War Trap

Scholars increasingly investigate the legacies of Africa's slave trade as well as the institutional determinants of postcolonial politics. This section reviews the insights of related literature, followed by the mechanisms that might link the slave trade and postcolonial politics.

Slave Trade Legacies

One of the most prominent aspects of Africa's slave trades is the demographic shock to local societies. During the fifteenth to nineteenth centuries, more than 10 million Africans were enslaved by the transatlantic slave trade alone, inflicting historically rare demographic shocks to the exposed communities (Curtin, 1969; Eltis et al., 1999; Manning, 1990).

The slave trade generated "tragically interconnected transformations" into African society (Manning, 1990, 147) and thereby left lasting legacies. Since the seminal data construction by Eltis et al. (1999), Nunn (2008), and Nunn & Wantchekon (2011), the empirical literature has increasingly explored the persistent effects of the shock-induced transformations on economic, political, and cultural outcomes. Nunn & Wantchekon (2011) demonstrate how the historical exposure to the slave trade undermined the interpersonal trust such that individuals with ancestor ethnic groups heavily exposed to the slave raids are less trusting of others and political authority. Dalton & Leung (2014) and Teso (2019) leverage the abnormal distortions in sex ratios induced by the slave traders' preference for males in the transatlantic slave trade. The shortage of males facilitated the spread of polygyny and gender equality, or norms and informal institutions that have persisted to the present day.

This article is closely related to Whatley (2014) on the impact of the slave trade on indigenous political authority in West Africa. Utilizing the port-level records of slave exports, Whatley (2014) highlights a slave trade-induced transformation of political authority by demonstrating that regions with greater exposure to the transatlantic slave trade see an increased proportion of ethnic groups with absolutist authority structure in the succession of local headman or chiefs, measured as the patrilineal heir or matrilineal heir of local political authority. As in conflict situations, "individuals subject to slave capture will pay more for protection, including relinquishing freedoms and rights that might otherwise be cherished in times of peace" (Whatley, 2014, 471). Whatley (2014) further uncovers that absolutist authority survived the colonial periods, as the colonial authorities relied on the existing local authority structure to govern and extract resources in West Africa.

Although several studies explore the consequences of the slave trade on *historical* conflicts (e.g., Fenske & Kala, 2017), the slave trade legacies on *postcolonial* politics remain relatively under-studied. As discussed below, the primary methodological challenge stems from the nonrandom assignment of the slave trade exposure and the correlates of political and economic outcomes in Africa. Since the work of Nunn & Wantchekon (2011), previous studies often employ coast distance to instrument the slave trade exposure. However, the established correlations between coast distance and economic development and between economic development and armed conflict in Africa discourage the use of the coast distance-based identification strategy to examine the slave trade legacies on, for example, contemporary conflicts due to the concern for the probable exclusion restriction violations.

Bargaining in the Shadow of History and Violence

Another body of literature related to this article focuses on the link between ethnic institutions and postcolonial politics. For example, Depetris-Chauvin (2016) and Wig (2016), respectively, demonstrate how historical statehood experiences and centralized traditional institutions enable ethnic groups to make credible commitments and decrease the risks of armed conflicts in postcolonial Africa. In contrast, Paine (2019) highlights the conflict-escalating effect of traditional institutions, such that groups with precolonial state-like institutions exacerbate civil war and coup risks by increasing interethnic tensions in host countries.

Behind these claims is the now-dominant view of postcolonial politics in Africa as the outcomes of strategic interactions between distinct ethnic groups in the shadow of violence (Francois et al., 2015; Roessler, 2011, 2016; Roessler & Ohls, 2018).[1] Rather than a single "big man" dominating the political realm, distinct ethnic groups with differing interests and access to state power often compete for the state power with the threat of armed uprising from outside; rulers and ruling groups strategically allocate rents and access to state power to buy off potential rivals and thereby prevent an outside rebellion at the increased cost of insider coups. Both sustained power sharing and failure of such peace arrangements into inefficient fighting emerge as equilibrium outcomes from these strategic interactions.

[1] Admittedly, ethnic cleavages are a major but not the sole driver of Africa's postcolonial politics and do not necessarily matter for politics in all the circumstances. Yet rebel and political entrepreneurs often have strategic incentives to exploit observable and less-manipulable ethnic traits to facilitate mobilization under informational asymmetry (Hale, 2008; Roessler, 2011) while limiting access to the spoils (Fearon, 1999).

Existing literature conceives the strategic situation as the coup-civil war trap (Francois et al., 2015; Paine, 2021; Roessler, 2011, 2016). On the one hand, rulers need to acquire support from a broader population by assuring rewards to consolidate their regimes. Excluding relevant groups from state power risks an outsider rebellion, while granting power-sharing spoils not only mitigates the imminent threats but also contributes to the government's counterinsurgency capacities. On the other hand, having potential rivals within the regime entails increased risks of coups d'état from inside. Rival groups make demands of political power with the twofold threats of inside coups and outside rebellions, and the ever-present threats incentivize rulers to offer rents and power-sharing spoils. Given weak state institutions, however, each side's capability and incentives to acquire more power at the expense of others generate persistent commitment problems. In the absence of commitment devices or the presence of power shifts, the underlying commitment problems can in turn invite a breakdown of power-sharing deals into inefficient fighting (Fearon, 1995; Powell, 2006).

Although suggestive, it remains less clear how group-level institutions affect the general coup-civil war trade-off. If traditional institutions shape groups' abilities to make credible commitments, group-level institutions and the transformations thereof should also influence the severity of the general trade-off and the prospects for domestic peace.

Slave Trade, Institutions, and Postcolonial Politics

This article argues that descendant ethnic groups with severe exposure to the slave trade are less likely to engage in armed conflicts while more likely to be included in the state power sharing schemes in postcolonial politics due to the ironically strengthened, group-level institutions and leadership authority. Yet the increased chance of power-sharing deals is coupled with the increased risks of insider coups. This section lays out the two building-blocks of the argument: The first component is the slavery-shock induced institutional change, and second is how the institutional change and constraints shape domestic bargaining.

Slave Trade and Institutional Change

As Whatley (2014) demonstrates, one dimension of the slave trade-induced institutional change is absolutist authority structure in the succession of local headmen and chiefs. Another key dimension is the increased investments in institutional devices or political centralization, with the resultant institutional change involving enhanced leadership authority combined with improved institutional constraints.[2] To illustrate, some ethnic groups such as the Yoruba in Nigeria and the Bisa in Zambia, having severely suffered from the slave trade, have absolutist authority and court- and legislature-like institutions. Other groups such as the Teke in DR Congo and the Goroa in Tanzania suffered less from the slave trade while lacking absolutist political structure and developed institutions.

The institutional change emerges from the incentives and strategic interactions of chiefs and group members. A major source of the incentives for absolutist chiefs to make costly investments is their longer time horizons and the ways of leadership appointments. Once established, chiefs in the communities with a less competitive, absolutist political structure would have, somewhat paradoxically, increased incentives to invest in group-level institutions to organize collective actions and deliver local public goods (Baldwin, 2016). Expecting to "rule for life," chiefs with longer time horizons have "more incentive than elected politicians to make up-front investments in institutions that will improve the ability of their communities to act collectively over the long run" (Baldwin, 2016, 10). *Because* they are unelected, chiefs have incentives to invest in institutions to tie their own hands to assure group members not to exploit their power at the expense of group members, thereby consolidating their authority and securing their long-run payoffs. Consistent with the reasoning, a Ghanaian local expresses, traditional chiefs often lack formal political power and thus "have to earn trust" of the local population to sustain their authority.[3]

[2] Using the instrumental variable design below, Appendix C reveals a positive association between the group-level slave trade exposure and the prevalence of absolutist authority and institutional constraints.

[3] Quoted in "Chiefs in command: Africa's chiefs are more trusted than its politicians." *Economist*, December 19, 2017. Available at: https://amp.economist.com/middle-east-and-africa/2017/12/19/africas-chiefs-are-more-trusted-than-its-politicians, accessed September 30, 2020.

Another set of institution-building incentives for local chiefs stems from the competition for survival of polities in the face of external insecurity. On the one hand, the logic of war and state-making failed to contribute to the emergence of territorial states in Africa (Herbst, 2000; Tilly, 1975). On the other hand, warfare to capture scarce people, rather than abundant lands, and the competition for survival have been a key driver of the continent's history (Dincecco et al., 2019; Herbst, 2000). The slave trade played an important role in such historical trajectories by reducing the incentives to build large-scale territorial states while intensifying the incentives to raid outsiders for slaves (Whatley & Gillezeau, 2011a).

The intensified insecurity and the increased profits from raiding outsiders can generate institution-building incentives for local leaders to better mobilize group members for group survival and collective benefits. As defense from outside threats and the benefits from raiding outsiders constitute public goods, group members have the incentives for free-riding on others' efforts. Institutional devices combined with uncontested authority mitigate the collective action challenge by improving the credibility of selective rewards and punishments. Survival and securing group benefits in an increasingly competitive environment also contributes to chiefs' own payoffs, and generates incentives for local chiefs for to invest in ethnic institutions. Turning to empirical patterns, Bates (2014) documents suggestive covariation of war, state formation-destruction, and institutional development in Africa during the 1400–1900 period. Acemoglu et al. (2014) and Bellows & Miguel (2006) uncover positive associations between internally less-competitive chiefdoms and enhanced local collective action capacities, and between exposure to wartime violence and political mobilization in Sierra Leone.

Group members' incentives and fear for enslavement are also attuned to the development of ethnic institutions. The threats of enslavement stem not only from outside but also from inside of their community. Indeed, intra-group kidnapping and trickery, along with inter-group warfare and raids, accounted for a major volume of enslavement in Africa (Lovejoy, 2000, 3–4; Nunn, 2017, 36–37). The increased demands for protection from external threats incentivize group members to support authority to coordinate group defense (Whatley, 2014, 471). Yet, the strategy itself does not necessarily prevent the authority from predating group members for private advantages. Even if the leadership commits to group protection, group members have reasons to expect the same leadership to renege on prior commitments once the external threats are mitigated or given the increasing benefits from raiding group members.[4] Uncontested, absolutist authority might fail to resolve the intra-group commitment and agency problems in the absence of institutional constraints. Institutional devices help the leadership to tie their hands and assure group members to survive the local competition.

Ethnic Institutions and the Coup-Civil War Trap

The improved group-level institutions and political authority, or centralized absolutist political structure, influence the general coup-civil war trade-off by altering the credibility of two distinct forms of commitments: *threats* and *promises*. First, institutional constraints enable an ethnic group to make a credible threat of an outside rebellion against political exclusion from state power. As Roessler (2016) and Roessler & Ohls (2018) argue, a key determinant of the ruler's decision to offer a power-sharing deal is the capabilities of outsider groups to credibly threaten and violently overthrow the regime. Political exclusion and inclusion constitute rulers' strategies of consolidating state power; and rulers have little incentive to grant power-sharing spoils to outsider groups lacking the coercive capacity to challenge and recapture sovereign authority.

In emphasizing coercive capacity, this article is not suggesting that the demographic size of an outsider group is the sole determinant of the credibility of civil war threats. Besides mobilization potential, the insights of Olson (1965) suggest that collective action problems also play a key role in altering the credibility of the threat of an outsider rebellion. In principle, higher mobilizational *potential* boost a group's ability to challenge the central government with force (Roessler, 2016; Roessler & Ohls, 2018). At the same time, however, severer col-

[4] The slave trade-induced, deteriorated interpersonal trust (Nunn & Wantchekon, 2011) does not necessarily contradict with improved ethnic institutions. Rather, the undermined trust and informal institutional constraints can facilitate investments in costly and otherwise unnecessary institutional devices.

lective action problems for a demographically larger group simultaneously impede its *actual* mobilizational capacity and threat credibility (Ito, 2021). Here, institutional devices and uncontested leadership, as well as resource endowments to offer selective (dis)incentives, serve as another key determinant of a group's ability to make credible threats of outside attacks against political exclusion by mitigating the collective action challenge. When coupled with improved collective action capacity, mobilizational potential permits a credible threat of an outside rebellion, which in turn gives rise to rulers' incentives to grant power-sharing spoils.

Second, institutions and effective group authority allow an ethnic group to make credible promises to follow through power-sharing deals and not to stage an insider coup once granted power-sharing spoils. The inclusion of a potential rival in the state power-sharing schemes entails an increased risk of insider coups, and the coup risk is partly a function of the rival's coercive capacity and leadership effectiveness. Indeed, the coercive capacity to stage an outside armed uprising also enhances the feasibility of an insider coup (Paine, 2021, 512). To circumvent the side effect of coercive capacities, therefore, an ethnic group needs to credibly promise to follow through the power-sharing deals and reassure the ruling groups. Yet the absence of uncontested leadership to monitor and control group members undercuts the group's ability to make credible commitments. Even if the current leadership promises not to stage an insider coup, such commitments remain incredible in the absence of effective group authority or the presence of internal fractionalization (Cunningham, 2013). Just as ethnic institutions (Wig, 2016), absolutist authority creates leadership continuity and mitigates the challenge of internal fractionalization and credible promises.

For a power-sharing deal to be self-enforcing, a ruler, as well as an outsider group, must credibly commit to the political bargain. Here, rulers not only decide whether or not to include an outsider group but also strategically choose the amount of the power-sharing spoils distributed to the group (Paine, 2021, 511–512). A possible strategy for a ruler to make credible commitments to share power is to grant valuable pieces of state power that allow the outsider groups for effective resistance against the rulers, such as military organizations.

Table 1: Testable Predictions

	Location level	Group level		
Mechanism	Battle	Outsider Rebellion	Power Sharing	Insider Coups
Institutional change	−	−	+	+
Geographic sorting	+	+	−	−

Nonetheless, the spoils of key state institutions also contribute to the coup technologies, and the strategy can in turn result in higher risks of insider coups by making coup attempts more feasible (Paine, 2021; Roessler, 2016; Roessler & Ohls, 2018).

In sum, the ironically improved institutions and leadership shape postcolonial politics by influencing the coup-civil war trade-off. Institutional constraints contribute to the group's ability to make credible threats for improved collective action capacity, and less-contested leadership to monitor and control group members allows for making credible promises to follow through power-sharing deals. If ethnic groups with severer exposure to the slave trade are better-equipped with such institutional devices and leadership authority, the historical treatment leads to not only *less* armed conflicts but also *higher* chances of political inclusion in postcolonial states. A probable by-product of the increased power-sharing chance is the *increased* risks of coup attempts, as higher power-sharing spoils enhance coup technology.

Alternative Explanation: Geographic Sorting

The advanced argument emphasizes institutional pathways to explain the political legacies of the slave trade. A possible alternative explanation is geographic sorting by migrations and the influx of new inhabitants. Group members may relocate across generations, and their historical homelands do not necessarily correspond to the current settlements. In particular, the short-term legacies of the slave trade create land lots by forcibly removing local inhabitants and could also facilitate subsequent population influx into the cleared lots. In a long run, Whatley & Gillezeau (2011b) empirically reveal a positive association between the exposure to the transatlantic slave trade increased ethnic stratification in West Africa.

Closely related to such population reshuffling is the "sons of the soil" dynamics highlighted in the civil war literature. Migration and population influx can increase the opportunities for riots and forms of communal conflicts between indigenous groups and recent migrants, which can escalate into large-scale armed conflicts with the incumbent forces often siding against the indigenous group (Fearon & Laitin, 2011). Although previous studies focus on postcolonial politics, a similar dynamic can facilitate historical clashes between local inhabitants by opening up otherwise absent opportunities for group interactions. To some extent, the reshuffling of the local population can remove the preexisting inter-group rivalries and hatreds. Yet even large-scale resettlements would remain incapable of replacing the whole pre-existing population, and the conflict-escalating legacies of increased conflict opportunities may outweigh the potential pacifying effect of hatred and rivalry removal (cf. Besley & Reynal-Querol, 2014). Consequently, the induced migrations and the legacies of historical conflicts would lead to *increased* incidents of armed conflicts at the location level.

The testable predictions for group-level rebellion, power sharing, and coup attempts also contrast with the advanced institutional mechanism. Population reshuffling does not necessarily contribute to institution building and enhanced leadership authority. Rather, the failure to protect group members and the loss of members might delegitimize and weaken preexisting ethnic institutions and group leadership. The resultant predictions include *higher* likelihood of armed conflicts and *decreased* chances of power sharing deals coupled with *decreased* coup risks, because the incumbent has little reason to buy off ethnic groups lacking institutional devices and leadership authority. Table 1 summarizes the testable predictions.

Research Design

The key features of the empirical strategy are twofold. First, to unpack the causal effects, it adopts an instrumental variable (IV) approach leveraging the soil suitability for cassava cultivation along with the arbitrary timing of its arrival in Africa. Second, to investigate the underlying mechanisms, it employs location- and group-level outcome variables.

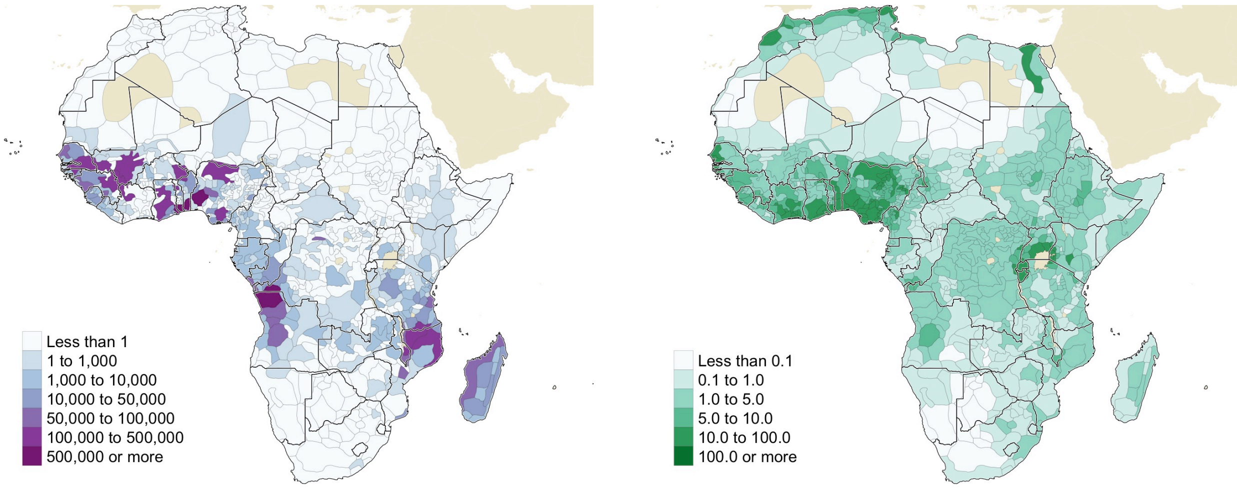
Data and Measurement

The unit of analysis is ethnic groups nested by host countries, which follows previous studies (e.g., Michalopoulos & Papaioannou, 2013, 2016). The sample ethnolinguistic groups and settlement areas build upon the map of Murdock (1959) digitized by Nunn (2008). As described in detail below, the primary treatment indicator of slavery exposure is measured at Murdock ethnic group level. The list-wise deletion due to missing values in the variables below leaves 824 unique groups and 1,282 country-group observations nested by 48 host countries. Figure 1 depicts the group settlement pattern along with the key variables.

The group-level records of slavery exposure come from Nunn & Wantchekon (2011). The dataset covers the group-level number of slave trade exports in the transatlantic and Indian Ocean slave trades, but not the trans-Saharan and Red Sea slave trades. The scope of the following analysis is thus limited to the former two slave trades.[5] The key treatment variable, $Slave_i^{\text{pc}} = \ln\left(0.01 + \frac{Slave\ Export_i}{Population_{i,1500}}\right)$, measures population-normalized slave trade exposure, with $Slave\ Export_i$ reflecting the number of slave exports of group i during the 1700–1900 (post-cassava arrival) period and $Population_{i,1500}$ measuring the estimated group population in 1500 (HYDE data, Kees et al., 2011). As an initial robustness check, I also construct an area-normalized measure, $Slave_i^{\text{Area}} = \ln\left(0.01 + \frac{Slave\ Export_i}{Area_i}\right)$, with $Area_i$ indicating the area in km^2 of a group's historical homeland.

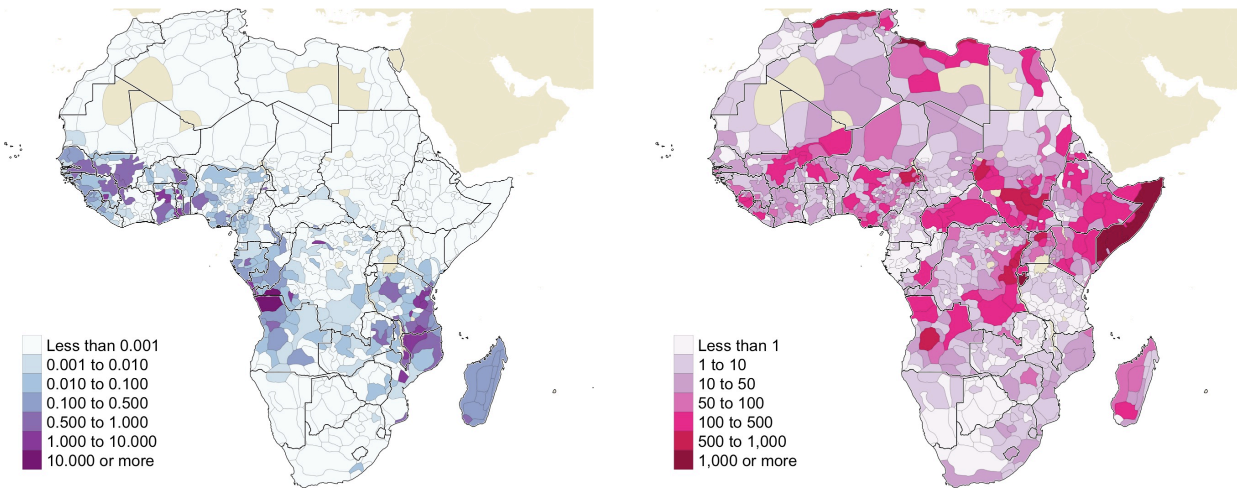
The analysis employs two series of outcome variables, each capturing distinct aspects of the slave trade legacies. First, to capture the local dynamics of armed conflicts, I utilize the Armed Conflict Location and Event Dataset (ACLED, Raleigh et al., 2010). Its African subset contains 216,808 geocoded violent and non-violent events from January 1997 to July

[5] One might wonder how historical group boundaries in Murdock (1959) overlap contemporary boundaries. Nunn & Wantchekon (2011) reveal a systematic association between the two slave export measures, one based on reported ethnicities in the 2005 Afrobarometer survey (ethnicity-based measure) and another based on the locations where the respondents reside today (geography-based measure). The two measures take the same values for 55 percent of the respondents in their sample (Nunn & Wantchekon, 2011, 3248).



(a) Slave Trade Exports, 1700–1900

(b) Population Density, 1500



(c) Per Capita Slave Exports, 1700–1900

(d) Battle Incidents, 1997–2020

Figure 1: Slave Trade Exposure, Precolonial Demography, and Postcolonial Battles
Notes: Thin (solid) segments represent group boundaries (international borders as of 2000).

2020 (version August 1, 2020). The current version of the dataset distinguishes three major event categories: "Violent events," "demonstrations," and "non-violent actions." As the analysis primarily concerns armed conflicts, I use the 59,121 records of battle events (a subcategory of "violent events" category). Of the 59,121 records, 2,684 entries (4.5%) are coded with provincial capitals (without precise locations) and excluded from the analysis. I then overlay the battle locations onto Murdock's (1959) map to count the number of events falling into each group's settlement area to construct *Battle* (Figure 1(d)).

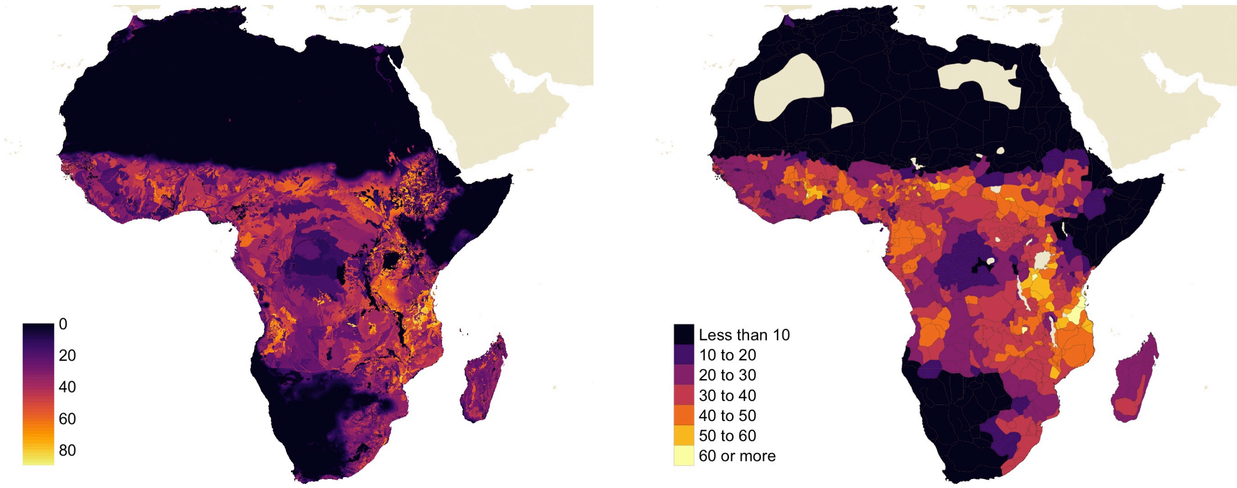
Second, to construct the group-level outcome variables, I first match Murdock ethnic groups to the ethnic groups in the Ethnic Power Relations (EPR) dataset (Cederman et al., 2010; Vogt et al., 2015), using the Linking Ethnic Data from Africa (LEDA) database and algorithm (Müller-Crepon et al., 2021). The LEDA algorithm identifies links between ethnic groups in different datasets based on the linguistic tree of the *Ethnologue* database and linguistic distances between distinct groups.[6] The generated correspondence table effectively connects 939 out of 1,282 country-group observations to the EPR groups.

The coding of the group-level outcomes follows previous studies on the coup-civil war trade-off (e.g., Roessler, 2011, 2016; Roessler & Ohls, 2018). *Power Sharing* measures the fraction of years in which an ethnic group is included in the national power sharing schemes or coded as "monopoly," "dominant," "senior partner," or "junior partner" in the EPR dataset. *Power Sharing* is normalized by the total years during which a group is available in the dataset because groups differ in the period in which they are available in the EPR data, and ranges from 0 (totally excluded) to 1 (totally included). *Rebel* and *Coup* are constructed from the EPR-compatible dataset of Roessler & Ohls (2018). *Rebel* (*Coup*) takes a value of 1 if members of an ethnic group launched a rebellion (coup attempt) and 0 otherwise, during the 1946–2013 period covered by the dataset.

As explained below, this article leverages soil suitability for cassava cultivation to instrument historical exposure to the slave trade at the ethnic group level. To construct the instrument, *Cassava*, I rely on the suitability index developed by the Global Assessment of Land Use Dynamics, Greenhouse Gas Emissions and Ecosystem Services (GLUES) Project (Zabel et al., 2014).[7] The GLUES data provide suitability indexes for 16 different crops and general agriculture, ranging from 0 (least suitable) to 100 (most suitable), based on the climatic, soil, and topographic conditions at a resolution of 30 arc-second ($\approx$ 1 km).

[6] I employ the linguistic distance-based matching algorithm of R-package LEDA, with a linguistic distance (ranging from 0 to 1) threshold of 0.2 to generate the Murdock-EPR links. The partial correlation estimates in Appendix A reveal little covariate imbalance between matched and non-matched group across the covariates below. As reported in Appendix B, alternative threshold values produce qualitatively similar results.

[7] I use the baseline estimates for the 1961–1990 period to construct soil suitability measures. The GLUES data provide geographically finer information compared to a 10 km-resolution of the Food and Agriculture Organization Global Agro-Ecological Zones database. See Zabel et al. (2014) for a focused comparison.



(a) Cassava Suitability Index (b) Group-Level Mean Suitability

Figure 2: Soil Suitability for Cassava Cultivation

As in Figure 2, I measure group-level average suitability measures for cassava and overall agriculture based on the estimates for the baseline 1961–1990 period.

Broadly following previous studies (e.g., Nunn & Wantchekon, 2011), I measure three sets of covariates to facilitate the analysis. To proxy precolonial political and economic geographies, the first set of covariates includes logged population density in 1500, logged percentages of cropland and grassland in 1500 (Kees et al., 2011), and ecological diversity index (Fenske, 2014). The first set also includes dummy variables for the presence of cities with more than 20,000 inhabitants in 1500 (Reba et al., 2016), node cities in the trans-Saharan and North African trade networks (Ciolek, 1999), precolonial kingdoms, and precolonial conflicts in a settlement area (1400–1700, Besley & Reynal-Querol, 2014; taken from the dataset of Michalopoulos & Papaioannou, 2016).

The second set measures geographic attributes, including logged mean elevation and ruggedness (Shaver et al., 2019; USGS 1996), the presence of capital cities and water body, the average temperature during the 1901–1910 period (Harris et al., 2020), logged mean malaria suitability index (Kiszewski et al., 2004), geographical areas of total homelands in logged km^2, the share of country-group settlement areas relative to total traditional homelands, population density in 1960, and a dummy variable for partitioning by international borders. I also include the distances in logged kilometers from settlement centroids to capitals, borders, equator, and coastlines, and the GLUES overall agricultural suitability index.

The third set includes three dummy variables that proxy European influence during the colonial period. The dummies are equal to 1 if a settlement area contains colonial railways, European explorer routes, or missions (Nunn & Wantchekon, 2011). To mitigate posttreatment bias, these covariates are included for a robustness check purpose.

Soil Suitability for Cassava Cultivation as an Instrument

Any investigation into long-run causal effects encounters the challenges of omitted variable bias and measurement error. For example, ethnic groups less able to resist outside violence might have suffered more from the slave trade and experience fewer conflicts today, generating a spurious negative correlation. The probable measurement error in slave export records invites another concern for attenuation bias and further discourages naïve comparisons.

To address these methodological challenges, I rely on an IV approach. Motivated by previous studies (Cherniwchan & Moreno-Cruz, 2019; Lowes & Montero, 2020; Nunn & Qian, 2011), the IV design leverages the variation in land soil suitability for cassava cultivation, rather than overall agricultural suitability or crop yields, as an instrument. In addition to (conditional) independence, a valid instrument in the current context needs to fulfill two conditions: A valid instrument (1) must be strongly correlated with the group-level exposure to the slave trade (instrument relevance) while (2) not affecting contemporary political outcomes through any path other than the slave trade (exclusion restriction).

Besides the rich group-level variation in Figure 2, there are three reasons to leverage cassava suitability as an instrument. First, unlike crop yields, soil suitability is mainly a function of the time-constant or slow-moving climatic and topographic conditions exogenous to human activities, which alleviates the concerns for instrument independence. Second, as reported below, cassava suitability is strongly correlated with group-level slavery exposure. Cassava did not exist in Africa before its travel from the New World in the sixteenth to seventeenth centuries and gradually spread across the continent toward the nineteenth century (Alpern, 1992, 24–26; Crosby, 1972, 185–188). With its tolerance to adverse environments and high energy yields, cassava improved land potential to provide food supplies and sustain a larger population. While other New World crops such as maize also contributed to population growth (Cherniwchan & Moreno-Cruz, 2019), another distinctive feature of cassava is its less input-demanding and capital-intensive nature (El-Sharkawy, 2004, 482).[8] As such, cassava contributes to both intercropping in regions with higher agricultural suitability *and* the production of otherwise absent food surplus in less suitable regions. The improved land potential raised the abilities of groups in cassava-suitable areas to engage in warfare while amplifying the slave raid incentives due to increased supplies of slaves and foodstuff to sustain their travel. These pathways also mirror the insights of historical studies, for example, that "the exports of an individual region responded far more to local *supply* conditions than they did to the *demand* of European traders" (Curtin, 1969, 226, emphasis added).

Third, cassava suitability circumvents the concern for the exclusion restriction, and its timing of arrival permits a unique falsification test. A common strategy in previous literature is to instrument slave exports by the geodesic distance between group locations and coastlines (e.g., Nunn & Wantchekon, 2011). While the coast-distance design is valid in some circumstances, the exclusion restriction is less likely to hold in the current context. For example, the established correlation between economic performance and conflict risks (e.g., Fearon & Laitin, 2003), combined with the poor economic performance of landlocked regions, would violate the assumption by opening up a path from the distance instrument to postcolonial conflicts. As reported later, cassava suitability is not systematically associated with established correlates of armed conflicts and power sharing, regional development and population size (e.g., Cederman et al., 2010; Fearon & Laitin, 2003; Francois et al., 2015).[9]

[8] Appendix D.1 presents a focused analysis and discussion on cassava and non-cassava crop suitability.

[9] Another potential concern for the exclusion restriction is that cassava suitability affects modern political outcomes by directly improving the group institutions and leadership rather than exclusively through the slave trade. Mayshar et al. (2017), however, reveal the role of cereal cultivation (e.g., maize), rather than perennial root cultivation (e.g., cassava), in facilitating institution building. The finding also discourages the use of maize to instrument the slave trade exposure due to the probable exclusion restriction violation.

Turning to falsification, the current IV strategy implies that *before* cassava's arrival, which altered the role of soil suitability for the crop in shaping the slave trade, we have little reason to see a systematic association between cassava suitability and slavery exposure. Unlike maize, which traveled from the New World in the early sixteenth century and diffused rapidly, cassava's delayed arrival left the earlier parts of the slave trade unaffected while opening up the cassava suitability-slavery pathway in the later periods. A fake first-stage regression with the pre-arrival slave exports as the outcome thus serves as a plausible falsification test for the suitability-based IV design.

Model Specification

The IV estimation builds on the following two-stage specification:

$$Slave_i = \alpha_c + \gamma Cassava_i + \boldsymbol{X}_i'\boldsymbol{\beta} + \boldsymbol{X}_{ic}'\boldsymbol{\eta} + \boldsymbol{M}_{ic}'\boldsymbol{\theta} + f_1(\boldsymbol{s}_{ic}) + e_{ic}, \tag{1}$$

$$Y_{ic} = \mu_c + \tau_{\text{IV}}\widehat{Slave_i} + \boldsymbol{X}_i'\boldsymbol{\eta} + \boldsymbol{X}_{ic}'\boldsymbol{\phi} + \boldsymbol{M}_{ic}'\boldsymbol{\lambda} + f_2(\boldsymbol{s}_{ic}) + u_{ic}, \tag{2}$$

where i indexes an ethnic group and c denotes host countries. Y_{ic} is one of the outcome variables, $Slave_i$ is population- or area-normalized slave trade exposure, $\widehat{Slave_i}$ is the corresponding fitted values from the first stage, and $Cassava_i$ is logged cassava suitability index. $\boldsymbol{X}_i$ and $\boldsymbol{X}_{ic}$ denote the vectors of group-level and country group-level covariates, and $f_1(\boldsymbol{s}_{ic})$ and $f_2(\boldsymbol{s}_{ic})$, with $\boldsymbol{s}_{ic} = (\text{Longitude}_{ic}, \text{Latitude}_{ic})$, are two-dimensional cubic polynomials of settlement centroids to screen out regional heterogeneity.[10] $\boldsymbol{M}_{ic}$ is a vector of synthetic covariates representing the Moran eigenvectors to absorb residual spatial autocorrelations (spatial filtering, Griffith & Peres-Neto, 2006).[11] Country fixed effects α_c and μ_c subsume cross-country heterogeneity such as colonizer policies and (post-)colonial institutions.

[10] $\boldsymbol{X}_{ic}$ includes share of contemporary settlement areas relative to traditional homelands, border distance, capital distance, capital presence, border partition, and population density (1960). $\boldsymbol{X}_i$ includes the remaining covariates. In the sample, 42.4% of the country-group observations are partitioned by modern borders.

[11] The spatial filtering approach follows Rozenas & Zhukov (2019). I rely on a distance-based spatial weight matrix with a $5°(\approx 550$ km$)$ threshold to construct the synthetic variables.

The parameter of interest is τ_{IV}, which captures the local average treatment effect (LATE) of the group-level exposure to the slave trade on the outcome variables, with the expected signs summarized in Table 1. Following the recommendations of Angrist & Pischke (2008, 197–205), I rely on two-stage least square (2SLS) models throughout the analysis, rather than nonlinear models with additional estimation assumptions. To account for possible error dependence, I report Kelly's (2020) data-driven, heteroskedasticity and autocorrelation consistent (HAC) standard errors adjusted for spatial clustering with an exponential kernel.

Table A.1 in the Appendix reports descriptive statistics of the variables, and Figures A.1 to A.3 present partial correlations to examine covariate balance. The covariates are roughly balanced, with exceptions including overall agricultural suitability and precolonial grassland proportion. To prevent the remaining imbalance from plaguing the estimates, the regression models reported below always adjust for these two covariates along with precolonial population density and settlement area as a restricted set of controls.

Results

This section reports the main empirical results in three steps. First, I present the first-stage estimates to examine the relevance of the cassava suitability instrument. Second, I report the second-stage results on the slave trade-battle association with the location-level outcome. I then turn to the group-level outcomes to further investigate the underlying causal pathways, followed by several additional investigations into the mechanisms and robustness checks.

Instrument Relevance

Table 2 reports the first-stage estimates, along with the coefficients on coast distance, an established predictor of the slave trade exposure. Consistent with the current IV strategy, cassava suitability is positively associated with group-level slave exports, with F-statistics passing Stock & Yogo's (2005) critical value of 16.38 against weak instruments, regardless of model specifications and (sub)samples. Panels A and B display the first-stage results with the population-normalized (columns 1–3) and the area-normalized slave export measures (columns 4–6) for the full 1,282 country-group observations and the subsample of the EPR-connected observations, respectively. Columns 1 and 4 adjust for the two covariates with relative imbalance, along with precolonial population density and settlement area (restricted controls). Columns 2–3 and 5–6 further adjust for the remaining geographic and precolonial controls and the dummies for European influence. The Moran's I statistics for regression residuals fail to retain statistical significance, suggesting that the covariate adjustments and the spatial filtering approach effectively address spatial autocorrelations.

Results I: Battle Exposure

Table 3 reports the IV-2SLS estimates for battle incidents (columns 2–3 and 5–6), along with the uninstrumented ordinary least squares (OLS) results (columns 1 and 4). Despite the negligible coefficient estimates in the OLS models, the coefficients on the slave export intensity retains statistical significance once instrumented, and remain stable across model specifications, which provides initial support for the advanced argument. In addition to its persistence, the negative association is sizable. Given the log-log specification, the coefficients can readily be interpreted as elasticity. Substantively, even after centuries, the coefficient of -0.955 (column 2) translates into that a 1 percent increase in the slavery exposure is followed by a roughly 0.96 percent decrease in battles in a group's traditional homeland.

Besides the difference between the average treatment effect (ATE) and the LATE, the OLS-IV discrepancy may reflect the bias remaining in the OLS estimates, IV-2SLS estimates, or both. First, the OLS estimates might suffer from confounding bias in addition to a classical form of measurement error, such that unobserved group-level propensity to engage in fighting is positively associated with the slave trade exposure and modern battle events. For example, war-prone groups might have been heavily exposed to slave exports due to the increased prisoners of war. A probable result is a bias toward zero, which masks the underlying

Table 2: First-Stage Estimates for the Group-Level Slave Trade Exports

	Panel A: All Observations					
	Slave^{pc} (population-normalized slave exports, 1700–1900)			$\text{Slave}^{\text{Area}}$ (area-normalized slave exports, 1700–1900)		
	(1)	(2)	(3)	(4)	(5)	(6)
Cassava Suitability	0.161**	0.194**	0.197**	0.197**	0.245**	0.247**
	(0.034)	(0.037)	(0.038)	(0.047)	(0.049)	(0.050)
Coast Distance		−0.308**	−0.293**		−0.520**	−0.503**
		(0.081)	(0.083)		(0.111)	(0.111)
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.400	0.420	0.421	0.420	0.449	0.451
F-statistic (weak instrument)	22.227	27.223	26.673	17.634	24.642	24.679
Residual Moran's I (std. deviate)	−1.184	−1.416	−1.079	−0.398	−0.875	−0.853
	Panel B: LEDA-Matched Observations					
	(1)	(2)	(3)	(4)	(5)	(6)
Cassava Suitability	0.199**	0.198**	0.201**	0.236**	0.239**	0.245**
	(0.041)	(0.044)	(0.044)	(0.055)	(0.058)	(0.059)
Coast Distance		−0.294**	−0.279**		−0.499**	−0.486**
		(0.088)	(0.090)		(0.116)	(0.118)
Observations	939	939	939	939	939	939
Adjusted R^2	0.416	0.441	0.444	0.430	0.468	0.470
F-statistic (weak instrument)	23.241	20.468	21.015	18.302	16.725	17.5
Residual Moran's I (std. deviate)	−1.27	−1.129	−1.054	−1.182	−0.564	−0.84
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓	✓	✓
Precolonial controls		✓	✓		✓	✓
Geographic controls		✓	✓		✓	✓
European influence indicators			✓			✓

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) spatial HAC standard errors are in parentheses. Moran eigenvectors in Panel B are based on the second-stage estimates with *Power Sharing* as the outcome.

negative slave trade-battle association. Second, despite the instrument relevance, the IV-2SLS estimates might suffer from the exclusion restriction violations and unadjusted spatial trends. The falsification tests reported below, however, fail to invalidate the current IV strategy and thereby support the interpretation of the IV-2SLS estimates as causal quantities.

Results II: Coup-Civil War Trap

Turning to the group-level outcomes, the results again lend empirical support to the advanced institutional change mechanism. Table 4 reports the IV-2SLS and uninstrumented OLS

Table 3: Slave Export Intensity and Battle Events, 1997–2020

	Dependent variable: $\ln(1 + \text{Battle})$					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave^{pc}	0.001	-0.955^{*}	-0.991^{*}			
	(0.032)	(0.459)	(0.436)			
$\text{Slave}^{\text{Area}}$				0.019	-0.753^{*}	-0.788^{*}
				(0.026)	(0.372)	(0.365)
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.559			0.559		
F-statistic (weak instrument)		27.223	26.673		24.642	24.679
Residual Moran's I (std. deviate)	−0.422	−0.313	−0.512	−0.7	−0.677	−0.557
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
European influence indicators			✓			✓

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) spatial HAC standard errors are in parentheses.

estimates for *Power Sharing*, *Rebel*, and *Coup*. The IV-2SLS results in columns (2)–(3) and (5)–(6) indicate that groups with higher exposure to the slave trade are more likely to be included in national power-sharing schemes (panel A), less likely to fight civil wars against the central government (panel B), and more likely to stage coup attempts in postcolonial states (panel C). Substantively, the estimates in column (2) indicate that a 10 percent increase in the slave trade exposure is followed by a $\tau_{\text{IV}} \times \ln(1.1) \approx 0.018$ increase in power sharing prevalence, a 1.87 percent point decrease in the rebel risk, and a 3.83 percent point increase in the coup risk, respectively, highlighting the political legacy of the slave trade.

Figure 3 displays the partial correlations between the (instrumented) slave exports and each of the four outcomes, with 100 randomly chosen observations. Rather than the variations driven by outliers, the estimates capture the overall patterns.

Robustness Checks and a Closer Look at the Mechanisms

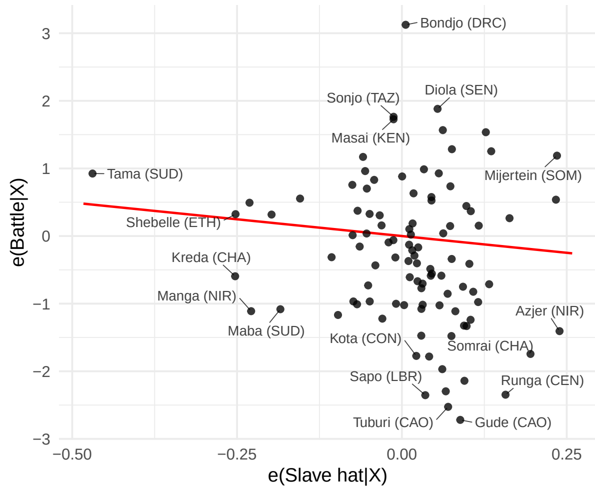
Appendix B addresses robustness concerns by employing (1) the post-cassava arrival slave trade intensity relative to the pre-arrival intensity, (2) an alternative battle intensity mea-

Table 4: Slave Export Intensity and Power Sharing, Rebel, and Coup, 1946–2013

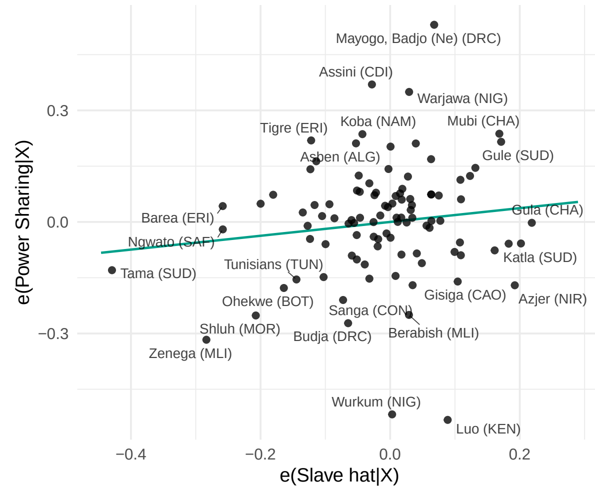
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: Power Sharing						
Slave^{pc}	−0.003	0.192*	0.186*			
	(0.005)	(0.077)	(0.076)			
$\text{Slave}^{\text{Area}}$				−0.005	0.159*	0.152*
				(0.004)	(0.067)	(0.064)
Observations	939	939	939	939	939	939
Adjusted R^2	0.721			0.719		
F-statistic (weak instrument)		20.468	21.015		16.725	17.5
Residual Moran's I (std. deviate)	−0.239	−0.561	−0.657	−0.266	−0.567	−0.568
Panel B. Dependent variable: Rebel						
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave^{pc}	0.007	−0.196*	−0.195*			
	(0.005)	(0.077)	(0.077)			
$\text{Slave}^{\text{Area}}$				0.008+	−0.163*	−0.160*
				(0.004)	(0.066)	(0.066)
Observations	939	939	939	939	939	939
Adjusted R^2	0.721			0.719		
F-statistic (weak instrument)		20.676	20.647		16.96	17.041
Residual Moran's I (std. deviate)	−1.035	−0.574	−0.393	−1.205	−0.66	−0.558
Panel C. Dependent variable: Coup						
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave^{pc}	0.008	0.402**	0.395**			
	(0.007)	(0.137)	(0.138)			
$\text{Slave}^{\text{Area}}$				0.006	0.333**	0.324**
				(0.007)	(0.118)	(0.115)
Observations	939	939	939	939	939	939
Adjusted R^2	0.721			0.719		
F-statistic (weak instrument)		20.496	21.474		17.208	17.525
Residual Moran's I (std. deviate)	0.301	0.802	0.528	0.068	0.633	0.338
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
European influence indicators			✓			✓

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) spatial HAC standard errors are in parentheses.

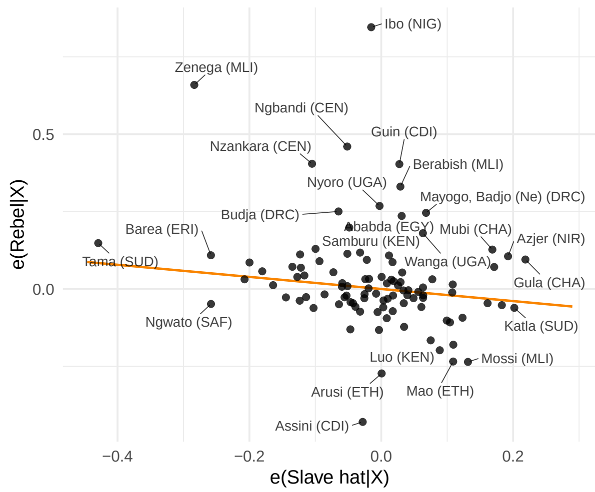
sure, (3) alternative linguistic distance thresholds for the LEDA algorithm, and (4) the LEDA-generated matching pair counts as regression weights. The results remain qualitatively unchanged in these alternative specifications. Appendix C further explores the un-



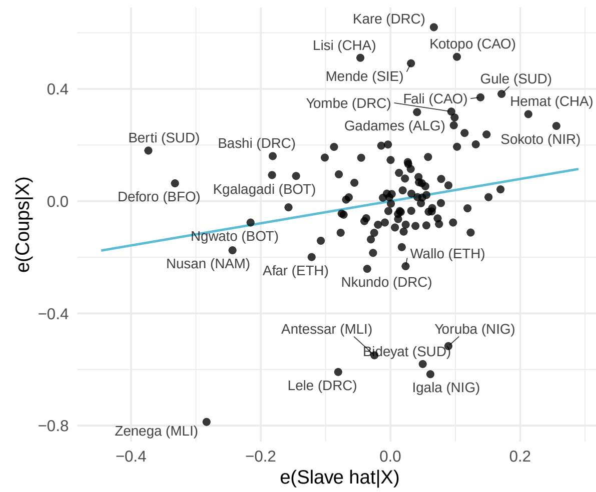
(a) Battle ($\hat{\tau}_{\text{IV}} = -0.955$)



(b) Power Sharing ($\hat{\tau}_{\text{IV}} = 0.192$)



(c) Rebel ($\hat{\tau}_{\text{IV}} = -0.196$)



(d) Coups ($\hat{\tau}_{\text{IV}} = 0.402$)

Figure 3: Partial Correlations Between Per Capita Slave Exports and the Main Outcomes

Notes: The slopes correspond to the coefficient estimates in Column (2) in Tables 3 and 4 for the outcome variable on the vertical axis. Dots and labels represent 100 randomly chosen observations, with labels denoting group names with host country name abbreviations in parentheses.

derlying mechanism in two ways and lends further confidence in the advanced argument. Directly, it reveals a positive association between the slave trade exposure and absolutist authority with enhanced institutions, an intermediate force in the presented argument. Indirectly, a mediation analysis utilizing the sequential g-estimator (Acharya et al., 2016) reveals limited roles of established correlates of coup-civil war trap, contemporary population size and regional development, in generating the political legacies of the slave trade.

Falsification Tests

Potential threats to the presented IV-based inference include the exclusion restriction violation along with unadjusted spatial trends. This section and Appendix D present multifaceted falsification tests to address the identification concerns.

Placebo Treatment: Pre-Cassava Slave Trade Exposure

The first test exploits the timing of cassava's arrival in Africa in the sixteenth to seventeenth centuries. Recall that the current design hinges on the ideas that cassava's arrival generated exogenous fluctuations in the slave trade exposure, and that the cassava suitability affects the modern political outcomes exclusively through the slave trade *after* cassava's arrival. A systematic first-stage association in the *pre*-arrival period would invalidate the IV design by implying unblocked IV-outcome pathways. For this exercise, I construct (placebo) treatment variables using the 1400–1599 period records, and then reestimate the first-stage regressions with the pre-arrival measures.[12] I drop the seventeenth-century records as the slave exports during this period may have been partly (un)affected by cassava suitability.

Table 5 reports the "false" first-stage estimates. Consistent with the current IV design, the cassava-slavery association turns out to be substantially and statistically insignificant once the treatment variables are replaced by the pre-arrival measures. By contrast, the coefficients on coast distance remain negative and statistically significant in the pre-arrival estimates. The discernible negative association suggests that the exercise is not just picking up the demographically less severe nature of the slave trade during the pre-cassava period.

Further Falsification Tests

Appendix D reports four additional falsification tests. The first additional exercise is motivated by the "false experiment" specification of Miguel et al. (2004, 736) and leverages soil

[12] I use the 1400–1599 (instead of the 1400–1500) period records due to dataset availability. The slave export dataset of Nunn & Wantchekon (2011) aggregates the export records during the first 200 years.

Table 5: False First-Stage Estimates for Group-Level Slave Exports, 1400–1599

	$\text{Slave}^{\text{pc}}_{1400\text{–}1599}$ (population-normalized slave exports, 1400–1599)		$\text{Slave}^{\text{Area}}_{1400\text{–}1599}$ (area-normalized slave exports, 1400–1599)	
	(1)	(2)	(3)	(4)
Cassava Suitability	0.017	0.018	0.026	0.028
	(0.015)	(0.019)	(0.018)	(0.023)
Coast Distance	-0.138^{**}	-0.105^{+}	-0.172^{**}	-0.131^{**}
	(0.049)	(0.056)	(0.050)	(0.048)
Observations	1,282	939	1,282	939
Adjusted R^2	0.406	0.409	0.438	0.451
F-statistic (weak instrument)	1.222	0.858	2.029	1.499
Residual Moran's I (standard deviate)	−0.437	−0.631	−0.269	−0.337
Country FE	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓
LEDA-connected sample		✓		✓

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) spatial HAC standard errors are in parentheses.

suitability for non-cassava crops as placebo instruments. If the current IV design is valid, we have little reason to see systematic first-stage and second-stage associations once cassava suitability is replaced with placebo instruments (see also, Lowes & Montero, 2020). The second exercise examines reduced-form associations with contemporary nightlight intensity and population density as auxiliary outcomes. Not to invite the concerns for the exclusion restriction violations, the cassava instrument should remain unassociated with these correlates of domestic fighting and power sharing. The third exercise utilizes the groups in today's North and South African states that have barely been exposed to the slave trade as a placebo sample. Because the slave trade pathway is absent in the subsample, we should not see systematic reduced-form associations between cassava suitability and the outcomes. The last exercise follows the diagnostic procedure proposed by Kelly (2019) and uses spatially autocorrelated artificial noise as placebo treatment and outcome. Any systematic association between the artificial noise and the treatment (outcome) warns spatial curve-fitting plaguing the analysis. Reassuringly, these additional tests also fail to invalidate the proposed IV design and thereby lend further credibility to the main findings.

Conclusion

Historical institutions matter in shaping modern political outcomes. This article exploits African slave trades to unpack the persistent effects of the external shock to local societies on postcolonial politics in Africa. The historically rare shock to local communities induced by the slave trade left lasting legacies on postcolonial politics by enhancing ethnic institutions and leadership authority and thereby easing the general coup-civil war trap and commitment problems. The empirical analysis reveals that ethnic groups with greater historical exposure to the slave trade are (1) more likely to be included in state power-sharing schemes, (2) less likely to experience battle incidents within their traditional homelands, and (3) less likely to fight civil wars, while (4) more likely to stage coups in postcolonial states.

These findings carry implications for broader literature. First, the persistent effects of the slave trade on postcolonial politics highlight the importance of historical confounders for investigations into the coup-civil war trap. Although limited to the context of postcolonial Africa, the findings suggest that the historical exposure to the slave trade and induced shock systematically alter the chances of power sharing, civil wars, and coups d'état in the modern era. A related implication follows that when omitted, the group-level slave trade exposure would invite confounding bias in the investigations into the power sharing-conflict link. Related literature now pays careful attention to the underlying selection process and strategic interactions, yet the historically deeper roots and historical confounders also warrant further attention when unpacking the determinants of contemporary political outcomes.

Second, the limitations of the presented analysis open up pathways for future research. The scope of the current analysis is confined in two aspects. First, the IV strategy identifies the local average treatment effect of Africa's slave trades. The remaining parts of the causal effects, instrumented or not, worth further investigations to fully unpack the persistent legacies of the slave trade. Second, the analysis is limited to the effect of the transatlantic and Indian Ocean slave trades. An investigation into the legacies of the two other major slave trades, trans-Saharan and Red Sea slave trades, offers a rare opportunity to explore how historical demographic shocks persistently affect subsequent political and related outcomes.

Finally, the findings underline how the interactions between historical events and institutional change shape subsequent political outcomes. As first suggested by Whatley (2014), the slave trade shock facilitated institutional change and empowered local chiefs. The externally-invited institutional change, in turn, influences power sharing, outsider rebellion, and insider coups in postcolonial African states by altering the institutional constraints on the strategic interactions. More generally, this article traces out how outside-in shocks to local societies foster institutional transformations and thereby influence subsequent trajectories, which in turn generates persistent legacies of historical events.

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Online Appendix for “On the Persistent Effects of the Slave Trade on Postcolonial Politics in Africa”

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Contents

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Introduction

This Online Appendix presents a series of descriptive statistics and regression estimates to supplement the empirical analysis reported in "On the Persistent Effects of the Slave Trade on Postcolonial Politics in Africa." Section A presents summary statistics and examines covariate balance and possible model dependence. Section B presents the details of the robustness checks, and Section C further investigates the underlying mechanisms by examining the shorter-run association between the slave trade and ethnic institutions and the roles of alternative mediating forces. Section D reports the results of the falsification tests.

The empirical analysis was conducted in R 4.1.0 for macOS. I primarily rely on sf and sp packages for geoprocessing (Bivand et al., 2013; Pebesma, 2018; Pebesma & Bivand, 2005), LEDA package for the Murdock-EPR group matching (Müller-Crepon et al., 2021), and lfe and spatialreg packages for regression analysis (Bivand & Piras, 2015; Gaure, 2013). I use Kelly's (2020) method and the accompanying R-scripts (https://github.com/morganwkelly/persistence) to compute the heteroskedasticity and autocorrelation consistent (HAC) standard errors adjusted for spatial clustering reported in the regression tables.

A Covariate Balance and Model Dependence

Table A.1 reports the descriptive statistics for the baseline and additional posttreatment covariates in the empirical analysis. Table A.2 summarizes the sources of the key variables, including the variables for the robustness checks and falsification tests. The remainder of this section examines covariate balance and possible model dependence.

A.1 Covariate Balance Across Cassava Suitability

Figure A.1 displays the partial correlation between the cassava suitability index and each of the main covariates to examine covariate balance. For each of the covariates, the instrument and the covariate on the vertical axis are partialled out by the remaining baseline covariates,

Table A.1: Descriptive Statistics

	Observations	Mean	SD	Median	IQR
	Panel A: Dependent Variables				
Battle	1,282	1.765	1.775	1.386	3.045
Power Sharing	939	0.434	0.355	0.310	0.571
Rebel	939	0.371	0.483	0.000	1.000
Coup	939	0.477	0.500	0.000	1.000
	Panel B: Treatment and Instrument				
Slave^{pc} (per capita slave exports)	1,282	−3.958	1.334	−4.605	0.453
$\text{Slave}^{\text{Area}}$ (per area slave exports)	1,282	−3.698	1.737	−4.605	0.975
Cassava Suitability	1,282	2.849	1.264	3.411	0.843
	Panel C: Covariates				
Restricted controls					
Area	1,282	9.801	1.321	9.716	1.904
Grassland in 1500	1,282	1.314	0.647	1.366	0.859
Overall Suitability	1,282	3.497	1.033	3.820	0.451
Population Density in 1500	1,282	0.578	1.539	0.659	1.789
Precolonial controls					
Coast Distance	1,282	5.978	1.159	6.300	1.456
Cropland in 1500	1,282	0.708	0.724	0.473	0.804
Ecological Diversity Index	1,282	0.279	0.229	0.292	0.474
Malaria Suitability Index	1,282	2.320	1.053	2.731	1.247
Trade Route Cities	1,282	0.055	0.227	0.000	0.000
Cities in 1500	1,282	0.024	0.154	0.000	0.000
Precolonial Conflict	1,282	0.054	0.226	0.000	0.000
Precolonial Kingdom	1,282	0.414	0.493	0.000	1.000
Geographic controls					
Area Share	1,282	0.642	0.390	0.837	0.766
Avg. Temperature, 1901–1910	1,282	24.220	3.257	24.918	4.865
Equator Distance	1,282	6.817	0.928	6.980	0.929
Elevation	1,282	6.160	0.859	6.205	1.163
Ruggedness	1,282	3.438	1.062	3.478	1.507
Partition	1,282	0.424	0.494	0.000	1.000
Capital (dummy)	1,282	0.034	0.180	0.000	0.000
Capital Distance	1,282	5.970	0.811	6.029	0.929
Border Distance	1,282	3.928	1.410	4.067	2.156
Population Density in 1960	1,282	1.984	1.737	2.165	1.858
Water Body	1,282	0.596	0.491	1.000	1.000
European influence					
Colonial Railway	1,282	0.145	0.352	0.000	0.000
Explore Routes	1,282	0.408	0.492	0.000	1.000
Missions	1,282	0.379	0.485	0.000	1.000

Notes: SD = standard deviation, IQR = interquartile range.

group homeland location polynomial, and country fixed effects. Solid segments represent linear regression fits, and the texts denote the partial correlation estimates.

Specifically, for each of the baseline covariates, I estimate the following linear regressions

Table A.2: Sources of the Key Variables

Variable	Source and Description
Dependent Variables	
Battle	ACLED data (Raleigh et al., 2010)
Power Sharing	Roessler & Ohls (2018)
Rebel	Roessler & Ohls (2018)
Coup	Roessler & Ohls (2018)
Treatment and Instrument	
Slave^{pc}	Per capita slave exports, 1700–1900 (Nunn & Wantchekon, 2011), group population counts are computed using the HYDE data (Kees et al., 2011)
$\text{Slave}^{\text{Area}}$	Per area slave exports, 1700–1900 (Nunn & Wantchekon, 2011), settlement areas are computed based on Murdock (1959), digitized by Nunn (2008)
Cassava Suitability	GLUES data (Zabel et al., 2014)
Covariates	
Overall Agricultural Suitability	GLUES data (Zabel et al., 2014)
Area	Murdock's (1959) map digitized in shapefile-format by Nunn (2008)
Population Density in 1500	HYDE data, version 3.1 (Kees et al., 2011)
Cropland in 1500	HYDE data, version 3.1 (Kees et al., 2011)
Grassland in 1500	HYDE data, version 3.1 (Kees et al., 2011)
Average Temperature, 1901–1910	CRU TS, version 4 (Harris et al., 2020)
Trade Route Cities	OWTRAD data (Ciolek, 1999)
Elevation	USGS (1996)
Ruggedness	Shaver et al. (2019)
Equator Distance	Natural Earth (https://www.naturalearthdata.com/110m-physical-vectors/)
Ecological Diversity Index	White's (1983) map provided in shapefile-format by Fenske (2014)
Malaria Suitability Index	Kiszewski et al. (2004)
Water Body	Natural Earth (https://www.naturalearthdata.com/10m-physical-vectors/)
Precolonial Kingdom	Besley & Persson (2011), taken from the replication data of Michalopoulos & Papaioannou (2016)
Precolonial Conflict	Besley & Persson (2011), taken from the replication data of Michalopoulos & Papaioannou (2016)
Cities in 1500	Historical Urban Population data (Reba et al., 2016)
Area Share	Murdock's (1959) map digitized in shapefile-format by Nunn (2008) and cshapes data (Weidmann et al., 2010)
Border Distance	cshapes data (Weidmann et al., 2010)
Capital (dummy)	cshapes data (Weidmann et al., 2010)
Capital Distance	cshapes data (Weidmann et al., 2010)
Coast Distance	cshapes data (Weidmann et al., 2010)
Partition	Murdock's (1959) map digitized in shapefile-format by Nunn (2008) and cshapes data (Weidmann et al., 2010). Following Michalopoulos & Papaioannou (2016), *Partition* is coded 0 if more than 90% of a historical homeland falls into a single country.
Population Density in 1960	HYDE data, version 3.1 (Kees et al., 2011)
Colonial Railway	Nunn & Wantchekon (2011)
Explore Routes	Nunn & Wantchekon (2011)
Missions	Nunn & Wantchekon (2011)
Robustness checks and Falsification Tests	
$\Delta\text{Slave}^{\text{pc}}$	Per capita increase in slave exports in 1700–1900 relative to the 1400–1599 slave exports (Nunn & Wantchekon, 2011)
$\Delta\text{Slave}^{\text{Area}}$	Per area increase in slave exports in 1700–1900 relative to the 1400–1599 slave exports (Nunn & Wantchekon, 2011).
xSub Battle	xSub data (Zhukov et al., 2019)
Nightlight in 2010	DMSP-OLS (NSDC 2014)
Population Density in 2010	WorldPop (2016)
Non-Cassava Crop Suitability	GLUES data (Zabel et al., 2014)

with the instrument (cassava suitability) and the kth covariate as the dependent variables:

$$Cassava_i = \alpha_c + \boldsymbol{X}'_{ic,-k}\boldsymbol{\beta} + g(\boldsymbol{s}_{ic}) + e_{ic}, \tag{A.1}$$

$$X_{ic,k} = \mu_c + \boldsymbol{X}'_{ic,-k}\boldsymbol{\eta} + h(\boldsymbol{s}_{ic}) + u_{ic}, \tag{A.2}$$

where the right-hand-side variables and coefficients are defined analogously to the two-stage specification in the main text.[1] Since the cassava-covariate association attributable to other covariates, $\boldsymbol{X}_{-k}$, is partialled out, the remaining covariance in each panel represents the conditional correlation between the cassava suitability index on the horizontal axis and the covariate on the vertical axis. All variables are standardized for a comparative purpose.

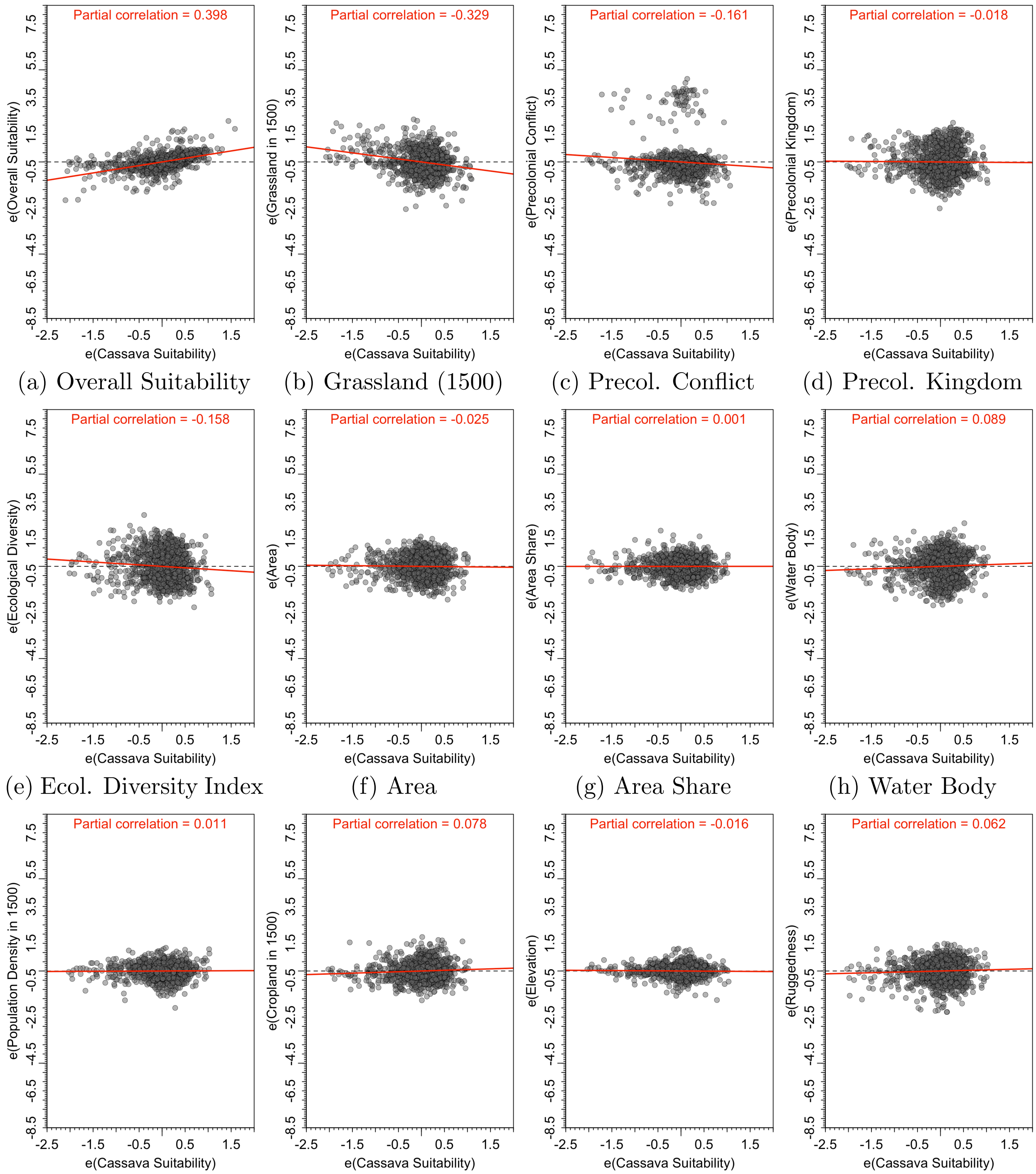
Reassuringly, the broadly flat regression slopes in Figure A.1 indicate a fair balance for most of the covariates. Exceptions include overall agricultural suitability and grassland proportion (panels (a) and (b)) out of the 23 baseline covariates, and to a lesser extent, ecological diversity index and precolonial conflict presence (panels (c) and (e)). The absence of systematic associations in the remaining panels suggests that, as far as other covariates are adjusted for, these covariates remain incapable of inducing confounding bias. Adjusting for these covariates increases the accuracy of the model if they are associated with the outcomes. To address the remaining imbalance, the estimations in the main text always adjust for these covariates with relative imbalance across the levels of cassava suitability.

Figure A.2 displays the corresponding partial correlations for the observations connected to the EPR groups by the LEDA algorithm, and the results remain qualitatively similar to the full sample estimates in Figure A.1. An exception is the stronger partial correlation between cassava suitability and water body presence in the LEDA-connected sample (panel (h)).

A.2 Covariate Balance Across Group Links

One might wonder if the group matching procedure with the LEDA algorithm induces sample selection (collider) bias in the group-level outcome results by omitting the groups without the Murdock-EPR links. To address the concern, Figure A.3 examines the partial correlation estimations for the covariate imbalance between the LEDA-matched and unmatched observations. Specifically, I replace the dependent variable in equation (A.1) with a dummy variable that takes the value of 1 if a country-group (Murdock group partitioned by contem-

[1] The baseline covariates refer to the covariates excepting the three indicator variables for European influence. The results remain qualitatively unchanged regardless of the adjustments for the indicator variables.



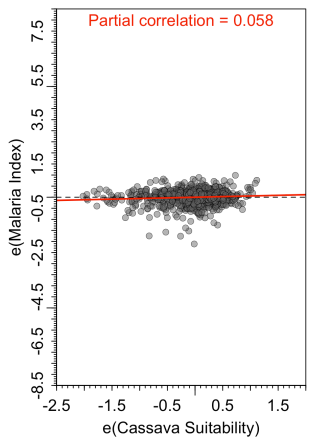
(a) Overall Suitability (b) Grassland (1500) (c) Precol. Conflict (d) Precol. Kingdom

(e) Ecol. Diversity Index (f) Area (g) Area Share (h) Water Body

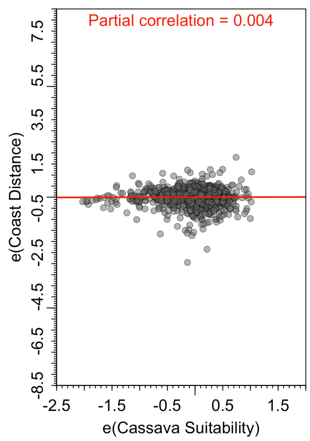
(i) Pop. Density (1500) (j) Cropland (1500) (k) Elevation (l) Ruggedness

Figure A.1: Partial Correlations between Cassava Suitability and the Baseline Covariates, All Observations ($N_{\text{obs}} = 1,282$). *Continued on next page.*

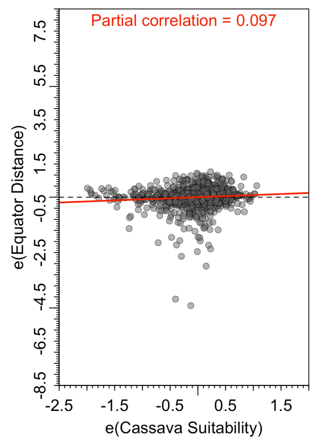
Notes: Each panel displays standardized partial correlation between the cassava suitability index (instrument) and one of the baseline covariates. The cassava suitability index on the horizontal axis is partialled out by the baseline covariates excepting for the vertical axis variable, location polynomial, and country fixed effects, while the covariate on the vertical axis is partialled out by the baseline covariates excepting itself, location polynomial, and country fixed effects, as in equations (A.1) and (A.2). Solid segments represent linear regression fits, and the text denotes the corresponding regression slope estimates.



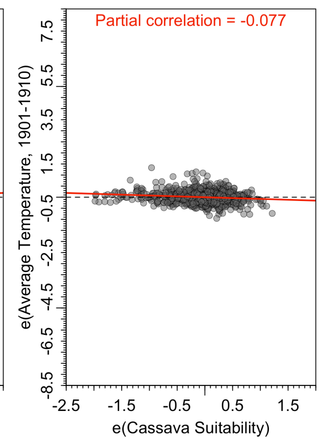
(m) Malaria Suit. Index



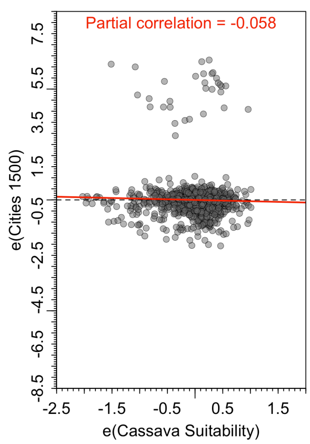
(n) Coast Distance



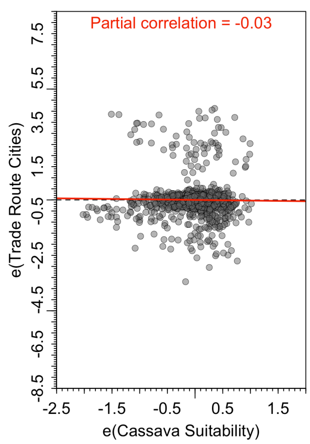
(o) Equator Distance



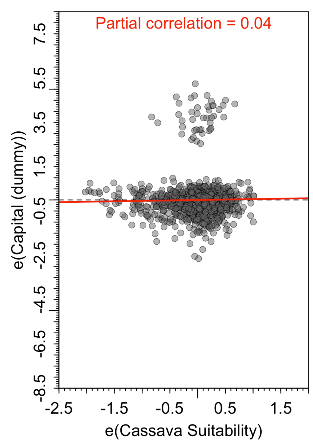
(p) Temp. (1901–1910)



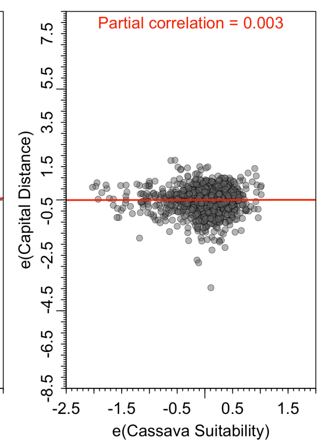
(q) Cities (1500)



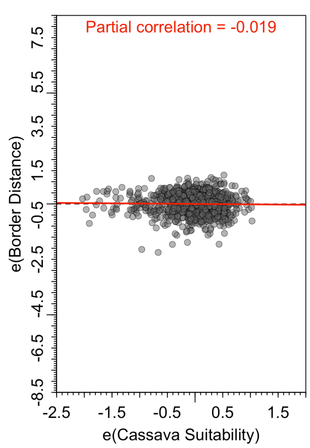
(r) Trade Route Cities



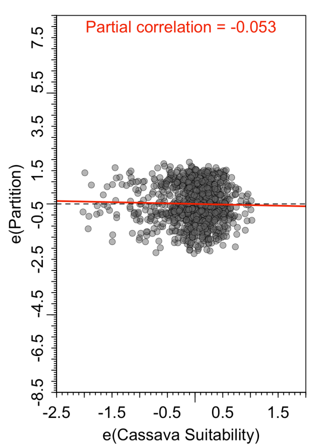
(s) Capital (dummy)



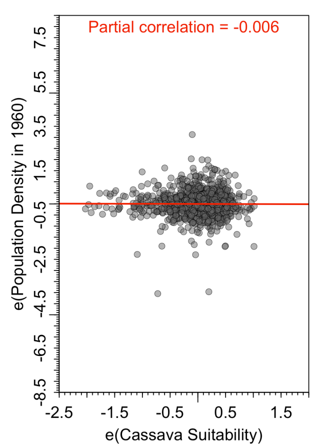
(t) Capital Distance



(u) Border Distance

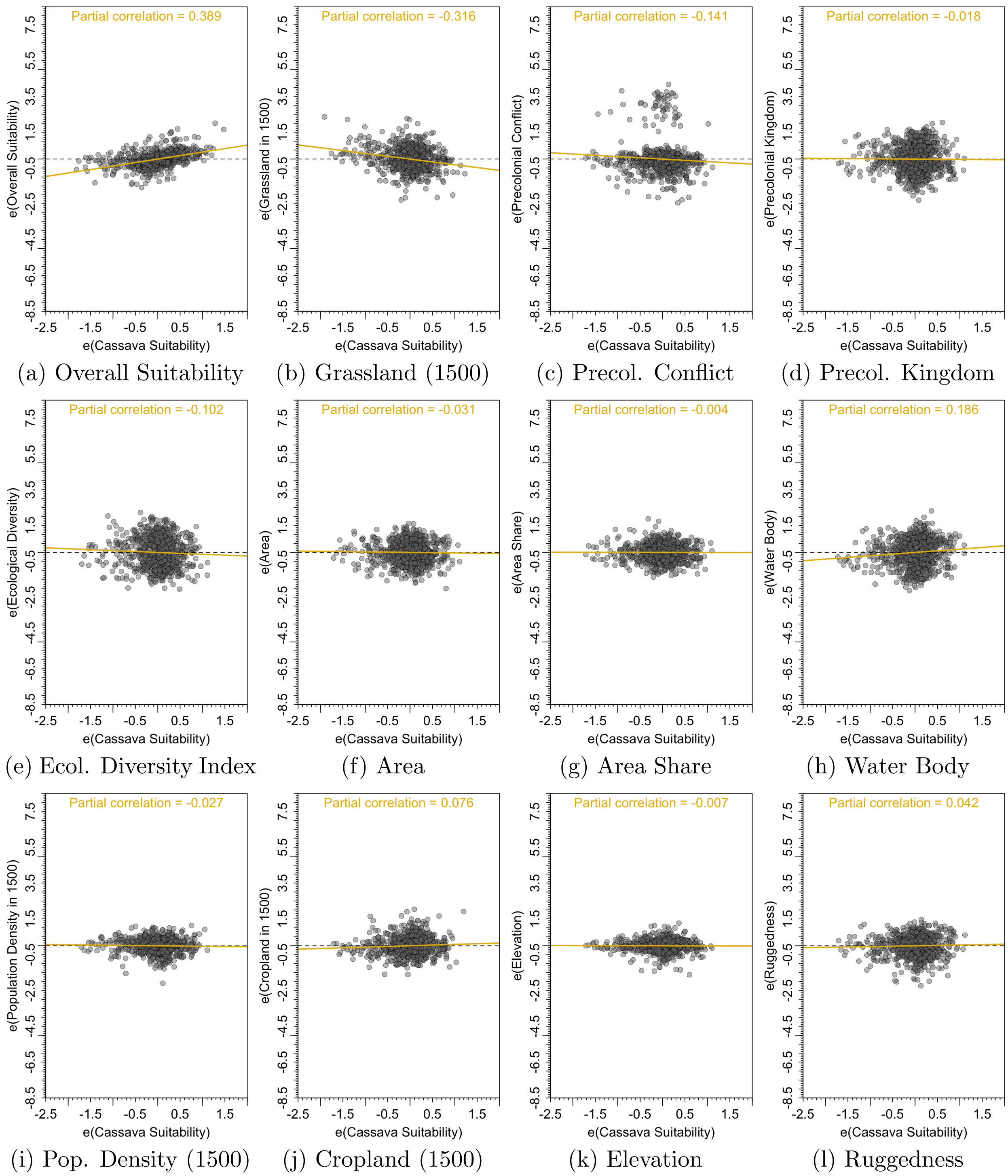


(v) Partition



(w) Pop. Density (1960)

Figure A.1 (contd.): Partial Correlations between Cassava Suitability and the Baseline Covariates, All Observations ($N_{\text{obs}} = 1,282$)



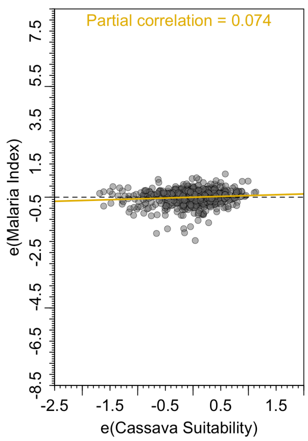
(a) Overall Suitability (b) Grassland (1500) (c) Precol. Conflict (d) Precol. Kingdom

(e) Ecol. Diversity Index (f) Area (g) Area Share (h) Water Body

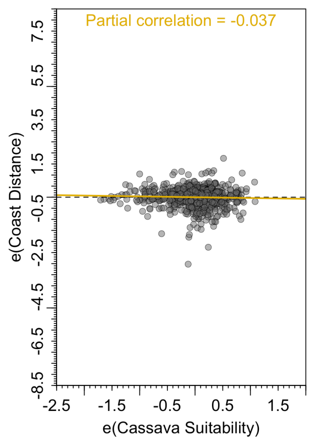
(i) Pop. Density (1500) (j) Cropland (1500) (k) Elevation (l) Ruggedness

Figure A.2: Partial Correlations between Cassava Suitability and the Baseline Covariates, LEDA-Connected Observations ($N_{\text{obs}} = 939$). *Continued on next page.*

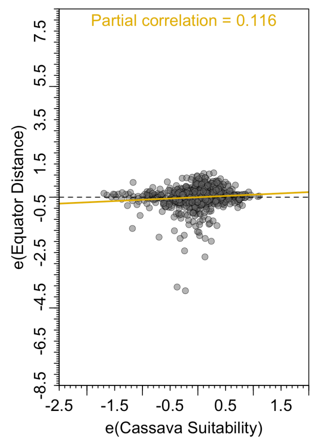
Notes: See notes in Figure A.1.



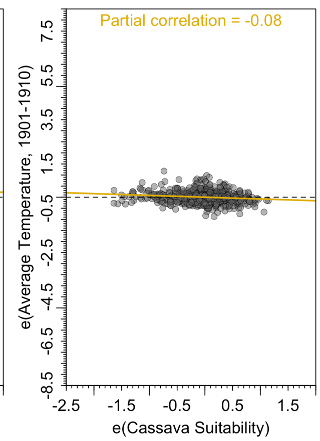
(m) Malaria Suit. Index



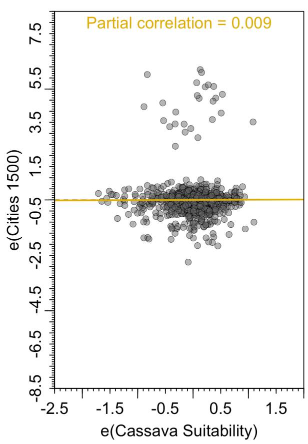
(n) Coast Distance



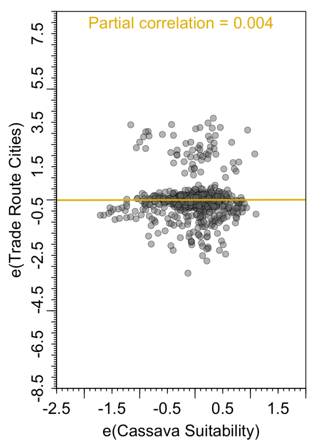
(o) Equator Distance



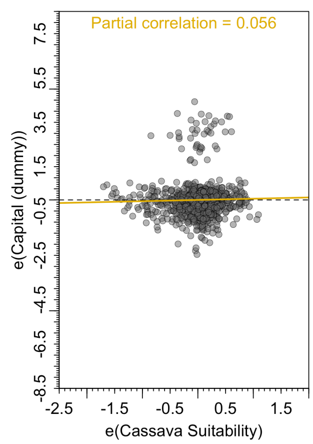
(p) Temp. (1901–1910)



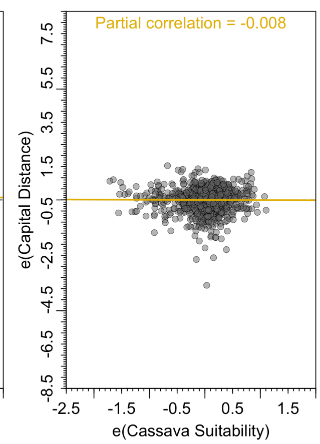
(q) Cities (1500)



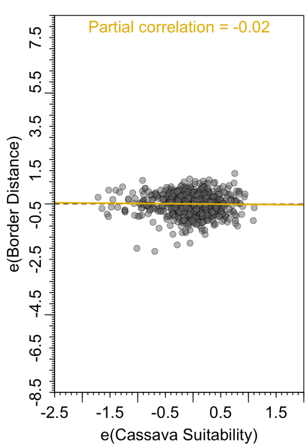
(r) Trade Route Cities



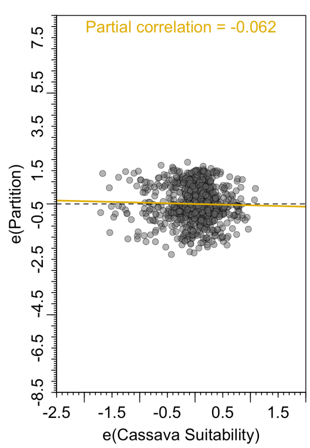
(s) Capital (dummy)



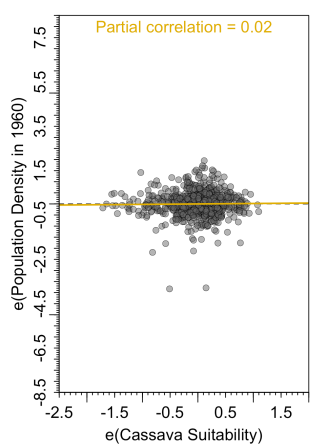
(t) Capital Distance



(u) Border Distance



(v) Partition



(w) Pop. Density (1960)

Figure A.2 (contd.): Partial Correlations between Cassava Suitability and the Baseline Covariates, LEDA-Connected Observations ($N_{\text{obs}} = 939$)

porary borders) observation is linked to EPR groups, and 0 otherwise, and add the cassava suitability index to covariate vector $\boldsymbol{X}$ in equations (A.1) and (A.2). I then subsequently estimate the partial correlations between the LEDA link dummy variable and the covariates (panels (a)–(w)) along with the cassava suitability index (panel (x)). The partial correlation estimates are largely negligible and reveal fair balance across the covariates and, importantly, the cassava instrument, inviting little concern for arbitrary sample selection.

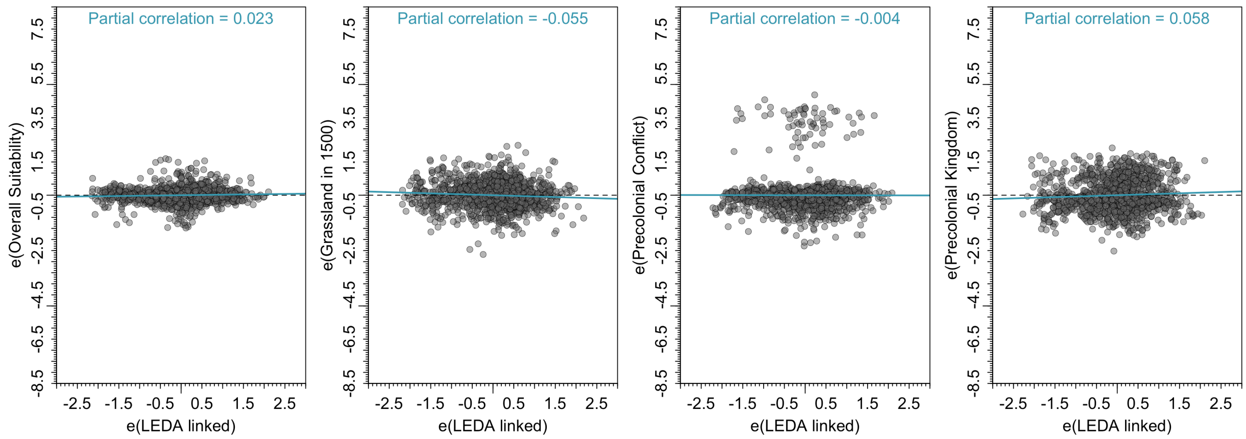
A.3 Model Dependence

One might wonder if covariate imbalance and possible model dependence invite any bias and robustness concerns. To guard against arbitrary picking of model specifications, I follow Ho et al. (2007) to replicate the first-stage estimate for each of $2^{19} = 524,288$ model specifications with different combinations of 19 covariates, along with the two covariates with relative imbalance (overall suitability and grassland proportion), precolonial population density and settlement area, location polynomial, and country fixed effects.

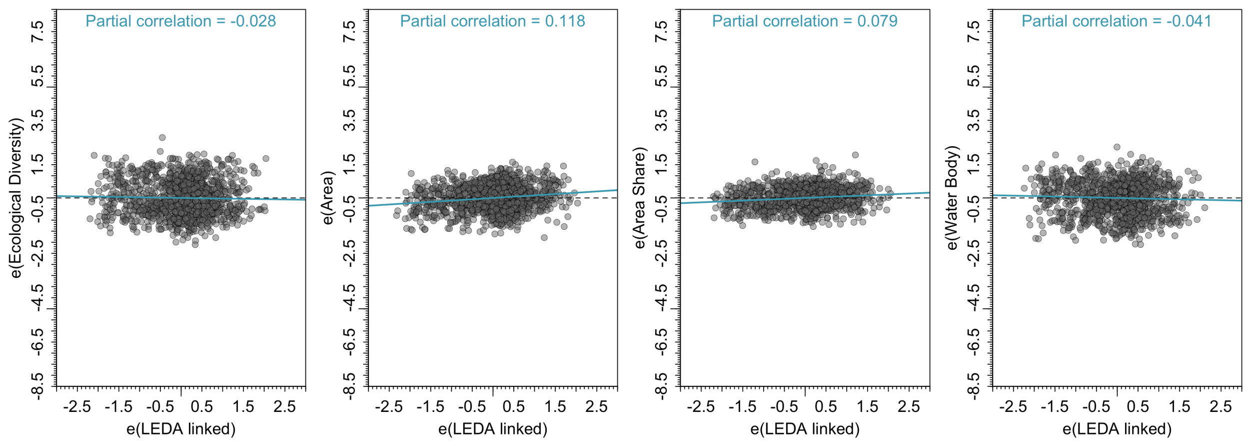
Figure A.4 shows the empirical distribution of the coefficient estimates obtained from the $524,288$ model specifications, for each of the population-normalized and area-normalized slave export index. The baseline point estimates in Table 2 are close to the mean of the respective empirical distribution of coefficient estimates. The roughly normally distributed coefficient estimates suggest that the inclusion or exclusion of the remaining covariates are unlikely to invite systematic bias into the first-stage estimates beyond random noise.

B Robustness Checks

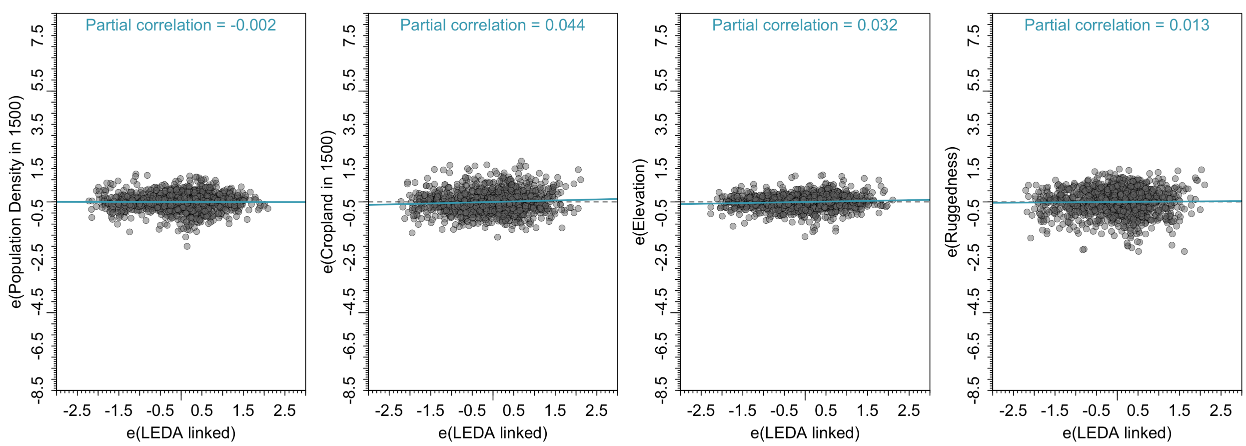
This section addresses the remaining robustness concerns. Specifically, the following sections present a series of robustness checks using (1) an alternative slavery exposure measure, the post-cassava arrival slave trade exposure relative to the pre-arrival intensity, (2) an alternative battle event measure, and (3) alternative linguistic distance thresholds for the



(a) Overall Suitability (b) Grassland (1500) (c) Precol. Conflict (d) Precol. Kingdom



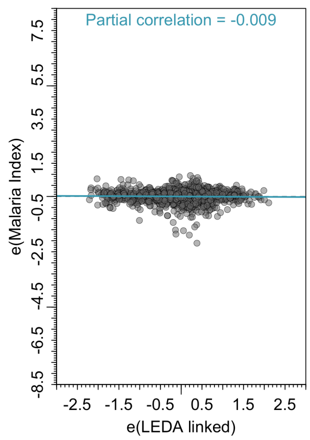
(e) Ecol. Diversity Index (f) Area (g) Area Share (h) Water Body



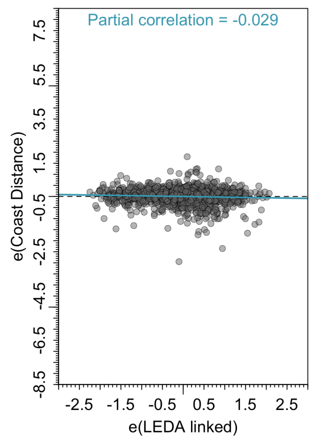
(i) Pop. Density (1500) (j) Cropland (1500) (k) Elevation (l) Ruggedness

Figure A.3: Partial Correlations between the LEDA group link and the Baseline Covariates ($N_{\text{obs}} = 1,282$). *Continued on next page.*

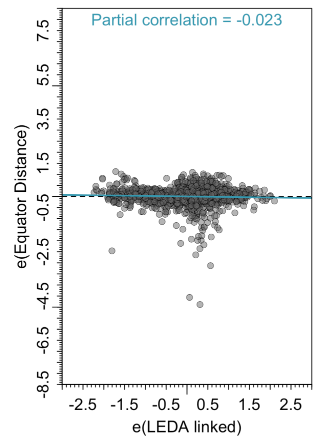
Notes: See notes in Figure A.1. The LEDA link dummy variable is coded using the LEDA-based matching procedure with the linguistic distance threshold of 0.2 as in the main text.



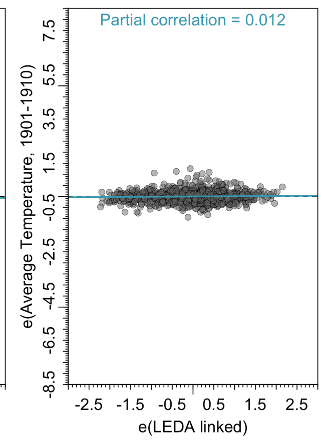
(m) Malaria Suit. Index



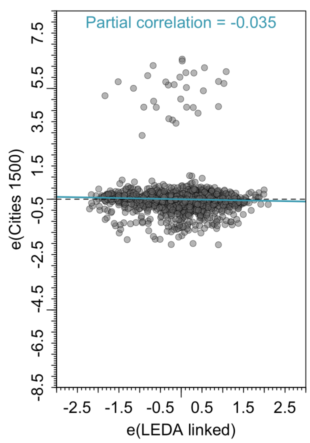
(n) Coast Distance



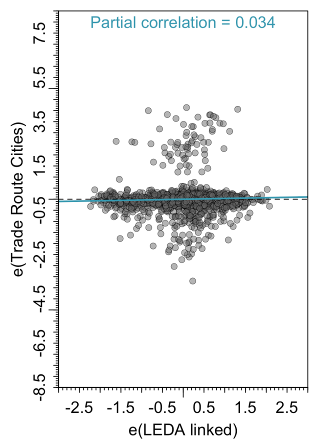
(o) Equator Distance



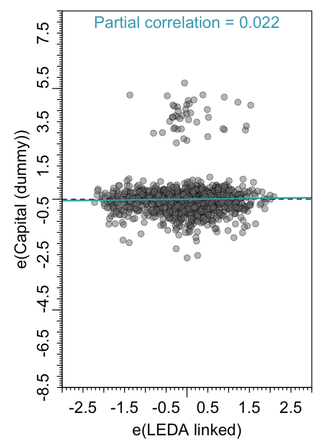
(p) Temp. (1901–1910)



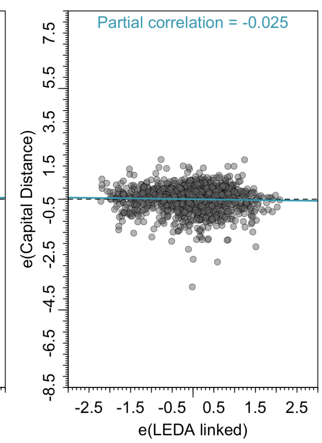
(q) Cities (1500)



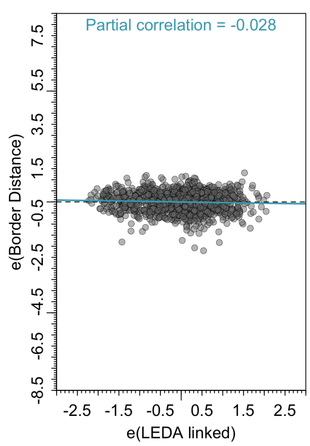
(r) Trade Route Cities



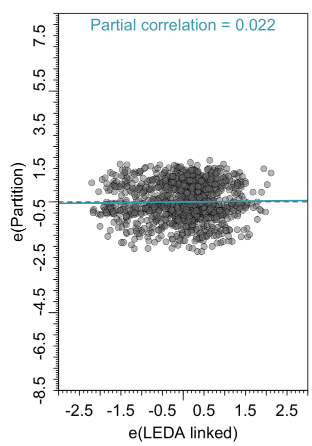
(s) Capital (dummy)



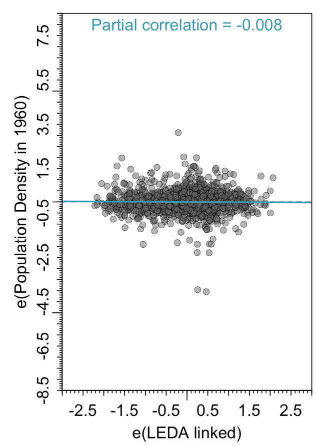
(t) Capital Distance



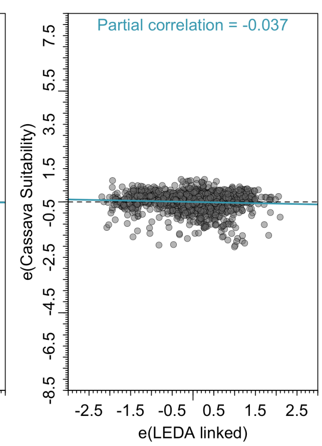
(u) Border Distance



(v) Partition



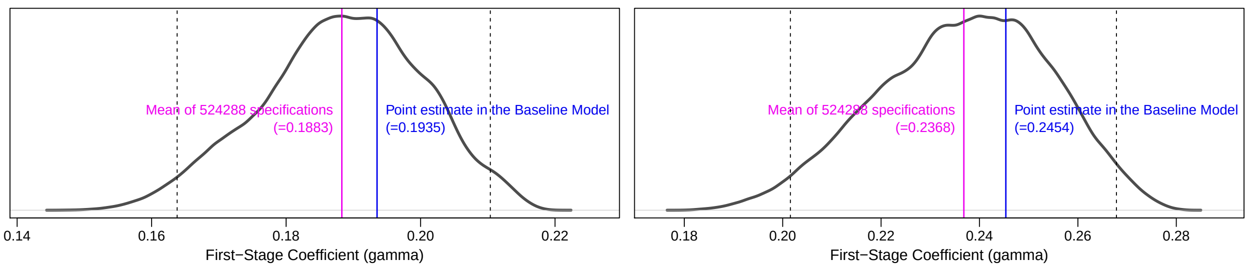
(w) Pop. Density (1960)



(x) Cassava Suitability

Figure A.3 (contd.): Partial Correlations between the LEDA group link and the Baseline Covariates ($N_{\text{obs}} = 1,282$)

Notes: See notes in Figure A.1.



(a) Population-Normalized Slave Trade Export (b) Area-Normalized Slave Trade Export

Figure A.4: Empirical Distributions of the First-Stage Cassava-Slave Trade Association

Notes: Empirical distributions of the first-stage coefficients on cassava suitability across $2^{19} = 524,288$ model specifications, with (a) population-normalized and (b) area-normalized slavery exposure measures as the first-stage outcomes. Blue vertical segments indicate the baseline estimates reported in Table 2 in the main text. Magenta vertical segments represent the mean values of the empirical distributions, and the range between dashed vertical segments cover the corresponding 95% confidence intervals.

LEDA algorithm. I also report (4) the group-level results with regression weights accounting for the multiple matching pairs generated by the LEDA algorithm.

B.1 Alternative Slave Trade Exposure Measures

The current IV strategy implies that ethnic groups with higher cassava soil suitability should have experienced increased slave trade exposure after cassava's arrival in Africa (1700–1900) relative to the pre-arrival exposure (1400–1599). A possible alternative treatment index thus measures the *difference* between the slave trade intensity in the pre-cassava and the post-arrival periods. To empirically examine this observable implication, I employ first-difference equivalent slave export measures, $\Delta Slave_i^{\text{pc}} = \ln\left(0.01 + \frac{\Delta Slave_i}{Population_{i,1500}}\right)$ and $\Delta Slave_i^{\text{Area}} = \ln\left(0.01 + \frac{\Delta Slave_i}{Area_i}\right)$, with $\Delta Slave_i = Slave\ Export_{i,1700\text{–}1900} - Slave\ Export_{i,1400\text{–}1599} + s$ and $s = |\min(Slave\ Export_{i,1700\text{–}1900} - Slave\ Export_{i,1400\text{–}1599})|$.

Tables B.1 and B.2 reestimate the baseline regressions in Tables 3 and 4 in the main text with the alternative population-normalized ($\Delta Slave_i^{\text{pc}}$) and area-normalized measures ($\Delta Slave_i^{\text{Area}}$). While the relatively weaker first-stage associations caution against the interpretation of the second-stage estimates as consistent estimates, the coefficient signs remain unchanged for all outcomes and provide additional support for the main findings.

Table B.1: Slave Trade Intensity and Battle Events, 1997–2020, Alternative Slave Trade Measures

	Dependent variable: ln(1 + Battle)					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
ΔSlave$^{\mathbf{pc}}$	0.081	-2.925^{*}	-3.162^{*}			
	(0.078)	(1.445)	(1.379)			
ΔSlave$^{\mathbf{Area}}$				0.078	-3.431^{*}	-3.623^{*}
				(0.104)	(1.645)	(1.689)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
European influence indicators			✓			✓
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.56			0.557		
F-statistic (weak instrument)		27.223	26.673		24.642	24.679
Residual Moran's I (std. deviate)	−0.879	−0.117	−0.339	−0.09	−0.016	−0.774

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) standard errors adjusted for spatial clustering with an exponential kernel are in parentheses. Restricted controls: Area, Grassland in 1500, Overall Suitability, and Population Density in 1500. Precolonial controls: Coast Distance, Cropland in 1500, Ecological Diversity Index, Malaria Suitability Index, Trade Route Cities, Cities in 1500, Precolonial Conflict, and Precolonial Kingdom. Geographic controls: Area Share, Average Temperature (1901–1910), Equator Distance, Elevation, Ruggedness, Partition, Capital (dummy), Capital Distance, Border Distance, Population Density in 1960, and Water Body. European influence indicators: Colonial Railway, Explore Routes, and Missions. See Tables A.1 and A.2 for a detailed description of the covariates.

B.2 Alternative Battle Incident Measure

Another remaining concern is the reliance on the ACLED data to construct the battle exposure in the empirical analysis. Reasonably, the exclusive reliance on a single database invites a robustness concern such that the findings might depend on the use of the ACLED data.

To address this robustness concern, I employ the Cross-National Data on Sub-National Violence (xSub) database to construct an alternative battle incident measure (Zhukov et al., 2019). The xSub database hosts different datasets with different spatio-temporal coverage, including the ACLED data and the UCDP Georeferenced Event Data (GED, Sundberg & Melander, 2013), which are widely used in previous studies. The xSub database also allows for combining the entries in different sources into a single dataset based on the Matching Event Data by Location, Time, and Type (MELTT) algorithm (Donnay et al., 2019). For

Table B.2: Slave Trade Intensity and Power Sharing, Rebel, and Coup, 1946–2013, Alternative Slave Trade Measures

	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
Panel A. Dependent variable: Power Sharing						
$\Delta\mathbf{Slave^{pc}}$	−0.016	0.617*	0.585*			
	(0.012)	(0.269)	(0.255)			
$\Delta\mathbf{Slave^{Area}}$				−0.037*	0.673*	0.647*
				(0.014)	(0.295)	(0.289)
Observations	939	939	939	939	939	939
Adjusted R^2	0.729			0.73		
F-statistic (weak instrument)		14.978	14.551		13.969	14.232
Residual Moran's I (std. deviate)	−0.124	−0.526	−0.556	−0.168	0.108	0.349
Panel B. Dependent variable: Rebel						
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
$\Delta\mathbf{Slave^{pc}}$	0.013	−0.631*	−0.615*			
	(0.011)	(0.252)	(0.250)			
$\Delta\mathbf{Slave^{Area}}$				0.009	−0.687*	−0.680*
				(0.023)	(0.274)	(0.275)
Observations	939	939	939	939	939	939
Adjusted R^2	0.824			0.821		
F-statistic (weak instrument)		15.586	14.938		13.835	14.277
Residual Moran's I (std. deviate)	−0.808	−0.276	−0.624	−0.641	0.007	0.128
Panel C. Dependent variable: Coup						
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
$\Delta\mathbf{Slave^{pc}}$	0.026	1.291**	1.243**			
	(0.016)	(0.480)	(0.459)			
$\Delta\mathbf{Slave^{Area}}$				0.026	1.407**	1.374**
				(0.028)	(0.536)	(0.504)
Observations	939	939	939	939	939	939
Adjusted R^2	0.72			0.718		
F-statistic (weak instrument)		15.511	14.95		14.037	14.544
Residual Moran's I (std. deviate)	0.305	1.232	0.375	0.789	0.969	0.392
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
European influence indicators			✓			✓

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) standard errors adjusted for spatial clustering with an exponential kernel are in parentheses. Restricted controls: Area, Grassland in 1500, Overall Suitability, and Population Density in 1500. Precolonial controls: Coast Distance, Cropland in 1500, Ecological Diversity Index, Malaria Suitability Index, Trade Route Cities, Cities in 1500, Precolonial Conflict, and Precolonial Kingdom. Geographic controls: Area Share, Average Temperature (1901–1910), Equator Distance, Elevation, Ruggedness, Partition, Capital (dummy), Capital Distance, Border Distance, Population Density in 1960, and Water Body. European influence indicators: Colonial Railway, Explore Routes, and Missions. See Tables A.1 and A.2 for a detailed description of the covariates.

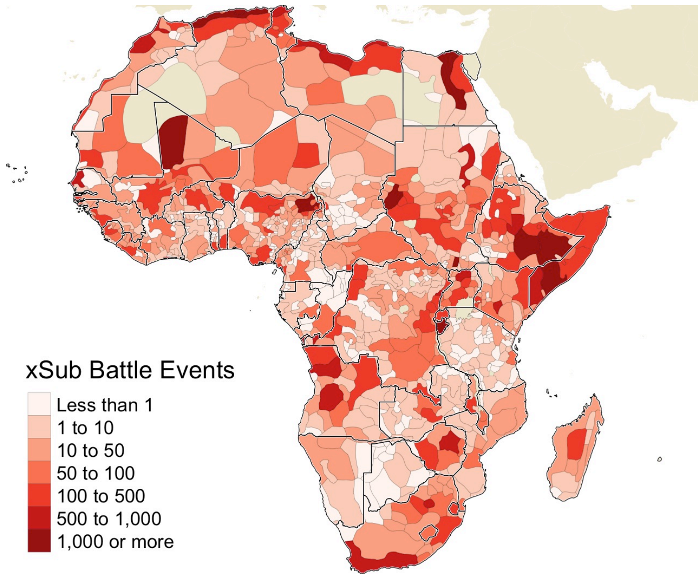


Figure B.1: xSub Battle Events, 1997–2019

Notes: Darker shades represent greater prevalence of battle events. Thin (solid) segments represent group boundaries (international borders as of 2000). Settlement areas with missing values and the areas outside of the study region are left blank.

consistency, the analysis restricts its temporal scope to the 1997–2019 period as the ACLED data, one of the major sources of MELTT-integrated xSub event dataset for the study region, covers the period after 1997 and the temporal coverage of the xSub database is limited to the pre-2019 period. I rely on the xSub-MELTT algorithm with a 1-km-by-1-day spatiotemporal window to extract the multi-source event dataset for each of the 48 countries in the study region. This procedure leaves 79,985 records of battle incidents ("DYAD_A_B," government-opposition interactions, Zhukov et al., 2019) in the study region during the 1997–2019 period. I then aggregate the xSub battle incidents at the country-group level following the geoprocessing procedure described in the main text to construct the alternative battle exposure measure, *xSub Battle*, depicted in Figure B.1.

Table B.3 reestimates the OLS and IV-2SLS regressions in Table 3 in the main text with $\ln(1 + \textit{xSub Battle})$ as the second-stage outcome variable. The IV-2SLS estimates remain remarkably stable with the alternative battle incident measure, with the coefficient signs and sizes remaining almost unchanged compared to the baseline of Table 3. The results suggest that the finding is unlikely to be a product of the reliance on a specific dataset.

Table B.3: Slave Trade Intensity and xSub Battle Events, 1997–2019

	Dependent variable: $\ln(1 + \text{xSub Battle})$					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
Slave$^{\text{pc}}$	0.029	-0.916^{*}	-1.013^{*}			
	(0.035)	(0.413)	(0.417)			
Slave$^{\text{Area}}$				0.049^{+}	-0.723^{*}	-0.806^{*}
				(0.029)	(0.340)	(0.346)
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.567			0.568		
F-statistic (weak instrument)		26.685	26.413		24.497	24.539
Residual Moran's I (std. deviate)	-0.597	-1.661^{+}	-1.47	-1.061	-1.278	-1.67^{+}
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
European influence indicators			✓			✓

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) standard errors adjusted for spatial clustering with an exponential kernel are in parentheses. Restricted controls: Area, Grassland in 1500, Overall Suitability, and Population Density in 1500. Precolonial controls: Coast Distance, Cropland in 1500, Ecological Diversity Index, Malaria Suitability Index, Trade Route Cities, Cities in 1500, Precolonial Conflict, and Precolonial Kingdom. Geographic controls: Area Share, Average Temperature (1901–1910), Equator Distance, Elevation, Ruggedness, Partition, Capital (dummy), Capital Distance, Border Distance, Population Density in 1960, and Water Body. European influence indicators: Colonial Railway, Explore Routes, and Missions. See Tables A.1 and A.2 for a detailed description of the covariates.

B.3 Alternative Murdock-EPR Group-Matching Threshold

The main analysis matches the ethnic groups in Murdock (1959) to the groups in the Ethnic Power Relations (EPR) dataset (Cederman et al., 2010; Vogt et al., 2015) using the Linking Ethnic Data from Africa (LEDA) algorithm (Müller-Crepon et al., 2021). Specifically, it employs the linguistic distance algorithm in LEDA package for R, with the language distance (ranging from 0 to 1) threshold of 0.2. Although the partial correlation estimates in Figure A.3 fail to reveal noticeable covariate imbalance between the LEDA-matched and non-matched groups, the threshold setting drops roughly 26.8% (343) of the 1,282 country-group observations from the analysis and might invite a concern for arbitrary sample selection.

To address this concern, Table B.4 reestimates the analysis of Table 4 in the main text with an alternative threshold value set to the sample median of 0.423 for the LEDA-Murdock

Table B.4: Slave Trade Intensity and Power Sharing, Rebel, and Coup, 1946–2013, with an Alternative LEDA Linguistic Distance Threshold Set to the Sample Median

	Panel A. Dependent variable: Power Sharing					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	0.002	0.167**	0.155**			
	(0.004)	(0.073)	(0.073)			
Slave$^{\mathbf{Area}}$				−0.001	0.135**	0.126**
				(0.003)	(0.060)	(0.061)
Observations	1,155	1,155	1,155	1,155	1,155	1,155
Adjusted R^2	0.763			0.763		
F-statistic (weak instrument)		24.498	23.979		21.104	19.823
Residual Moran's I (std. deviate)	−0.517	−0.882	−0.582	−0.573	−0.822	−0.657
	Panel B. Dependent variable: Rebel					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	−0.003	−0.201**	−0.208**			
	(0.004)	(0.080)	(0.082)			
Slave$^{\mathbf{Area}}$				−0.002	−0.162**	−0.168**
				(0.004)	(0.066)	(0.068)
Observations	1,155	1,155	1,155	1,155	1,155	1,155
Adjusted R^2	0.819			0.818		
F-statistic (weak instrument)		25.312	25.463		20.744	20.639
Residual Moran's I (std. deviate)	−0.311	−0.575	−0.262	−0.205	−0.634	−0.416
	Panel C. Dependent variable: Coup					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	−0.005	0.303**	0.299**			
	(0.005)	(0.124)	(0.124)			
Slave$^{\mathbf{Area}}$				−0.007	0.245**	0.243**
				(0.005)	(0.102)	(0.104)
Observations	1,155	1,155	1,155	1,155	1,155	1,155
Adjusted R^2	0.769			0.770		
F-statistic (weak instrument)		24.492	24.990		20.810	19.716
Residual Moran's I (std. deviate)	0.232	−0.211	−0.194	0.156	−0.229	−0.276
LEDA language dist. threshold	Median	Median	Median	Median	Median	Median
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
European influence indicators			✓			✓

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) standard errors adjusted for spatial clustering with an exponential kernel are in parentheses. Restricted controls: Area, Grassland in 1500, Overall Suitability, and Population Density in 1500. Precolonial controls: Coast Distance, Cropland in 1500, Ecological Diversity Index, Malaria Suitability Index, Trade Route Cities, Cities in 1500, Precolonial Conflict, and Precolonial Kingdom. Geographic controls: Area Share, Average Temperature (1901–1910), Equator Distance, Elevation, Ruggedness, Partition, Capital (dummy), Capital Distance, Border Distance, Population Density in 1960, and Water Body. European influence indicators: Colonial Railway, Explore Routes, and Missions. See Tables A.1 and A.2 for a detailed description of the covariates.

matching. The results remain qualitatively unchanged regardless of the threshold values for the group-matching procedure and the resultant (increased) sample size.

B.4 Group-Level Regressions with the LEDA Matching-Pair Weights

Relatedly, the group-level regressions and the Murdock-EPR matching procedure permit multiple matches such that the LEDA algorithm generates, when available, multiple matching pairs for a single EPR (Murdock) group with equal language distances.[2] For example, the matching rule allows a single instance of outside rebellion (recorded at the EPR-group level) to be counted for more than one time when the corresponding EPR group is matched with more than one Murdock group. This matching rule might invite another concern for the possible inflation in the group-level outcome variables.

To address this concern, Table B.5 reestimates the group-level regressions with the number of matched pairs as regression weights. Although the coefficient sizes vary, the coefficient signs remain unchanged from the baseline estimates reported in Table 4 in the main text. As in the baseline results, the coefficients of the slave trade measures fail to retain substantial and statistical significance in the uninstrumented OLS estimates (columns 1 and 4). By contrast, once instrumented, the slave trade exposure measures are positively associated with *Power Sharing* and *Coup* while negatively associated with *Rebel* (columns 2–3 and 4–5).

C Mechanisms and Alternative Mediating Forces

An untested observable implication of the advanced institutional mechanism is the positive association between slavery exposure and improved ethnic institutions. The argument also implies that traditional institutions play a substantial role in generating the long-run effect, and, indirectly, the treatment effect should remain stable regardless of the adjustments for alternative mediating forces. This section empirically examines these additional implications.

[2] The number of matching pairs aggregated at Murdock-group level ranges from 1 to 10, with the sample median (mean) of 2 (2.959).

Table B.5: Slave Trade Intensity and Power Sharing, Rebel, and Coup, 1946–2013, with the LEDA-Matching Weights

	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: Power Sharing						
Slave^{pc}	-0.001	0.109^{*}	0.104^{*}			
	(0.003)	(0.053)	(0.051)			
$\text{Slave}^{\text{Area}}$				-0.002	0.105^{*}	0.100^{+}
				(0.003)	(0.053)	(0.053)
Observations	939	939	939	939	939	939
Adjusted R^2	0.854			0.854		
F-statistic (weak instrument)		14.756	14.921		11.339	11.032
Residual Moran's I (std. deviate)	1.429	1.183	0.182	1.502	1.431	0.327
Panel B. Dependent variable: Rebel						
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave^{pc}	0.004	-0.114^{*}	-0.095^{+}			
	(0.003)	(0.055)	(0.053)			
$\text{Slave}^{\text{Area}}$				0.005^{+}	-0.115^{*}	-0.102^{*}
				(0.003)	(0.056)	(0.051)
Observations	939	939	939	939	939	939
Adjusted R^2	0.898			0.898		
F-statistic (weak instrument)		14.759	16.369		11.684	13.625
Residual Moran's I (std. deviate)	0.249	-0.502	-0.653	0.323	-0.531	-0.75
Panel C. Dependent variable: Coup						
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave^{pc}	0.001	0.311^{*}	0.277^{**}			
	(0.005)	(0.128)	(0.103)			
$\text{Slave}^{\text{Area}}$				-0.0005	0.271^{*}	0.265^{**}
				(0.005)	(0.110)	(0.103)
Observations	939	939	939	939	939	939
Adjusted R^2	0.832			0.833		
F-statistic (weak instrument)		13.422	18.842		12.452	15.406
Residual Moran's I (std. deviate)	1.4	0.312	0.478	1.164	0.988	0.883
LEDA matched-pair weights	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
European influence indicators			✓			✓

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) standard errors adjusted for spatial clustering with an exponential kernel are in parentheses. Restricted controls: Area, Grassland in 1500, Overall Suitability, and Population Density in 1500. Precolonial controls: Coast Distance, Cropland in 1500, Ecological Diversity Index, Malaria Suitability Index, Trade Route Cities, Cities in 1500, Precolonial Conflict, and Precolonial Kingdom. Geographic controls: Area Share, Average Temperature (1901–1910), Equator Distance, Elevation, Ruggedness, Partition, Capital (dummy), Capital Distance, Border Distance, Population Density in 1960, and Water Body. European influence indicators: Colonial Railway, Explore Routes, and Missions. See Tables A.1 and A.2 for a detailed description of the covariates.

C.1 Intermediate Outcome: Traditional Ethnic Institutions

The advanced institutional change mechanism emphasizes traditional ethnic institutions as a primary mediating force to generate the slave trade legacies. Although Whatley (2014) reveals a positive association between slave trade exposure and absolutist political authority in West Africa, it warrants a focused examination given the proposed IV design, additional emphasis on institutional constraints, and the expanded sample.

Recall that this article emphasizes institutional development or the combination of absolutist political authority and institutional constraints, in addition to the ways of succession or appointing local headmen (Whatley, 2014). I construct the dependent variable for the additional analysis, *Institutionalized Absolutist*, to measure the characteristics of traditional ethnic institutions along with the two aspects: absolutist authority and political institutions. First, to measure the degree of absolutist authority, I follow the absolutism index of Whatley (2014) based on the dataset of Murdock (1967) *Ethnographic Atlas*. Specifically, the absolutism indicator, *Absolutist*, takes the value of 1 if a group is coded as "patrilineal heir" or "matrilineal heir" in "Succession to the Office of Local Headman" variable (Column 72), and 0 otherwise.[3] Second, I use variable "Jurisdictional Hierarchy Beyond Local Community" (Column 33) in *Ethnographic Atlas* to measure group-level institutional development. The variable counts the number of a group's political organizations or jurisdictional levels, and ranges from 0 (no levels of jurisdictional hierarchy, or "stateless societies"), through 1–2 ("petty and larger paramount chiefdoms or their equivalent"), to 3–4 ("large states," Murdock, 1967, 160). Previous studies use the variable to proxy the degree of group-level institutions and political centralization (e.g., Fenske, 2014; Gennaioli & Rainer, 2007; Michalopoulos & Papaioannou, 2013; Wig, 2016). Third, I interact the absolutist dummy with the jurisdictional hierarchy variable to construct the institutionalized absolutism index. The resultant index is coded in a categorical scale, with a higher score indicating an ethnic

[3] I rely on the digitized version of the *Ethnographic Atlas* of Giuliano & Nunn (2018) to construct the variables that measure the aspects of traditional institutions.

Table C.1: Slave Trade Intensity and Traditional Ethnic Institutions

	Dependent Variable:					
	Slave^{pc}	Institutionalized Absolutist		$\text{Slave}^{\text{Area}}$	Institutionalized Absolutist	
	First stage	OLS	IV-2SLS	First stage	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Cassava Suitability	0.598**			0.721**		
	(0.101)			(0.119)		
Slave^{pc}		−0.004	0.464*			
		(0.046)	(0.233)			
$\text{Slave}^{\text{Area}}$					0.003	0.384*
					(0.038)	(0.195)
Observations	429	429	429	429	429	429
Adjusted R^2	0.352	0.104		0.404	0.104	
F-statistic (weak instr.)	35.265		35.265	36.721		36.721
Residual Moran's I (standard deviate)	−0.098	−0.378	−0.257	0.135	−0.381	−0.616
Region FE	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓

Notes: $^+p < 0.1$; $^*p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) spatial HAC standard errors with an exponential kernel are in parentheses. Precolonial controls: Coast Distance, Cropland in 1500, Ecological Diversity Index, Malaria Suitability Index, Trade Route Cities, Cities in 1500, Precolonial Conflict, Precolonial Kingdom, and Population Density in 1500. Geographic controls: Area, Grassland in 1500, Overall Suitability, Average Temperature (1901–1910), Equator Distance, Elevation, Ruggedness, and Water Body.

group with absolutist political authority and developed institutions.

I then regress *Institutionalized Absolutist* on the slave trade exposure measures to examine the intermediate institutional legacies of the slave trade. The model specification follows the baseline two-stage IV specification, with the spatial polynomial term is specified as a linear polynomial given the reduced sample size. As the analysis concerns the pre-independence periods, I use Murdock ethnic group (not split by contemporary borders) as the unit of analysis and replace the country fixed effects with region fixed effects. I also exclude the covariates that reflect modern international borders and capital locations.[4]

Table C.1 reports the slave trade-customary institution association along with the first-stage cassava-slavery association, and Figure C.1 displays the partial correlations between

[4]Excluded covariates are *Area Share*, *Border Distance*, *Capital* (dummy), *Capital Distance*, *Partition*, and *Population Density* (1960). The regression models adjust for the remaining baseline covariates.

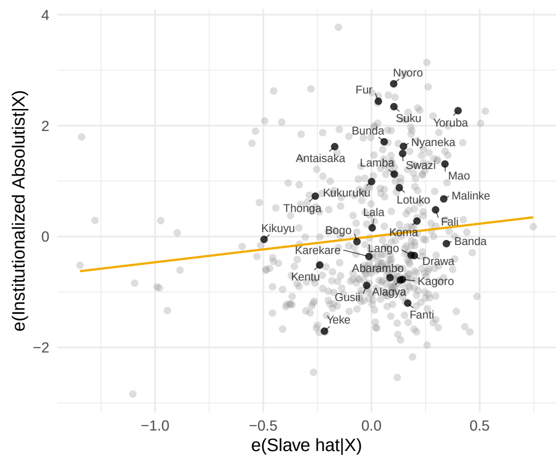


Figure C.1: Partial Correlation Between Per Capita Slave Exports and Ethnic Institutions
Notes: The partial correlation estimate is based on Column (2) in Table C.1. Black dots and labels indicate the names of 30 randomly chosen ethnic groups in Murdock (1967).

the slavery exposure (instrumented by cassava suitability) and the centralized absolutist index. Although the results remain suggestive given possible non-random attrition in the list of Murdock (1967), the intermediate-outcome regressions reveal the patterns consistent with the advanced institutional mechanism. The first-stage estimates in columns (1) and (4) reveal a positive association between cassava suitability and the slave trade exposure, with the F-statistics passing the critical value of 16.38 for both population-normalized and area-normalized slave export measures. The second-stage results in columns (3) and (6) reveal a positive association between slavery exposure and centralized absolutist political structure. The coefficients of the slave trade measures are positive and retain statistical significance at the conventional 5% level in the IV-2SLS estimates (columns 3 and 6) while the uninstrumented OLS estimates remain indeterminate (columns 2 and 5).

Admittedly, the cross-sectional nature of *Ethnographic Atlas* does not allow us to trace the temporal variations of ethnic institutions during the slave trade period. Indeed, *Ethnographic Atlas* is derived from the ethnographic works documented during the colonial period, and the list provides a cross-sectional snapshot of ethnic groups as of the period. In other words, the positive association is obtained from the sample of *survivor* ethnic groups in the colonial age is likely to be driven by the combination of the institutional adoption *and* the sample selection process (see also, Tilly, 1975, 14–15; King et al., 1994, 136). Closely related to this perspective, Bates (2014) points out (431–432, emphasis added):

> "While scanty and highly imperfect, the data suggest that the competition among the states in pre-colonial Africa may have given rise to a cross-section of polities in which state-formation and economic development co-varied. *Either* because they *responded* to external threats by seeking to strengthen their economic base, *or* because interstate competition *winnowed* from the sample polities that were weak and poor, we find that societies that were 'state-like' also appear to have been developmental."

Unfortunately, the limited temporal coverage of the available records does not allow further investigation into the magnitude of possible sample selection bias.

Despite the limitation, the estimates suggest that greater exposure to the slave trade was followed by an increased prevalence of absolutist political authority and improved institutions. These results provide additional evidence in support of the institutional change mechanism by highlighting the shorter-run political legacies of the slave trade.

C.2 Alternative Mediating Forces: Contemporary Population Size and Economic Development

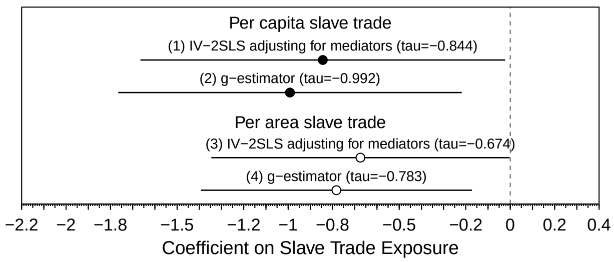
A possible strategy to examine the role of precolonial customary institutions in generating the political legacies of the slave trade is to regress modern political outcomes on indigenous ethnic institutions. Yet such a naive strategy invites another set of methodological concerns including the probable non-random assignment of ethnic institutions as well as the non-random sample attrition in the list of the *Ethnographic Atlas*.

However, it is still possible to further explore the causal pathways indirectly by investigating the roles of alternative mediating forces. The intuition follows that if the slave trade legacies operate through alternative mediating forces rather than precolonial customary institutions, then the claimed treatment effect should be attenuated away once we adjust for the alternative mediators. Possible mediating forces include contemporary population size and regional development, or two of the established correlates of power sharing and domestic fighting (e.g., Cederman et al., 2010; Collier & Hoeffler, 2004; Fearon & Laitin, 2003; Francois et al., 2015). Previous studies also uncover a negative association between the slave trade exposure and contemporary economic development (Nunn, 2008). If, on the one hand, contemporary population size and economic development play a major role in generating the slave trade legacies, then the unblocked causal pathways would undermine the validity of the advanced institutional mechanism. If, on the other hand, the reported treatment effects remain robust to additional adjustments for these alternative mediating forces, then the analysis provides indirect but additional support for the advanced institutional mechanism.

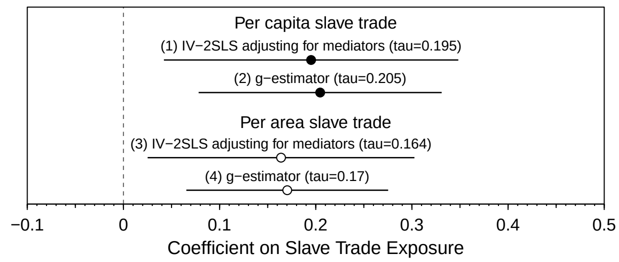
I present two series of estimations to examine how these alternative mediating forces participate in the underlying causal pathways. First, I simply add the group-level logged population density (WorldPop, 2016) and logged nightlight intensity (as a proxy of economic performance, NSDC 2014) in 2010 to the main IV-2SLS specifications (columns 2 and 5 in Tables 3 and 4 in the main text) and reestimate the regressions. If the slave trade legacies primarily operate through the contemporary population and regional development channels, then the LATE estimate should be attenuated away once these pathways are blocked.

Second, to account for possible posttreatment bias, I use the sequential g-estimator to quantify the roles of contemporary population density and regional development in the underlying causal pathways (Acharya et al., 2016a; Vansteelandt, 2009). The quantity of interest in the sequential g-estimator is the average controlled direct effect (ACDE), which measures the direct treatment effect with the mediators fixed at some arbitrary value for all units.[5] The ACDE estimate should be substantially smaller than the baseline treatment effect estimate if contemporary population size and economic development play a major role in the

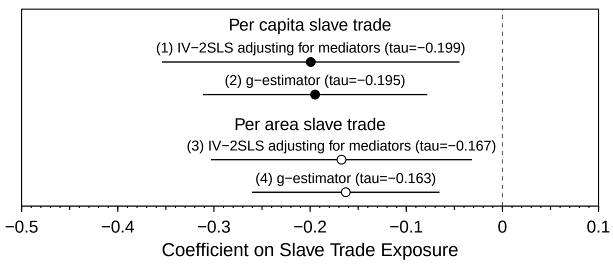
[5] I recenter the two mediators, population density and nightlight intensity, to respective sample means in the sequential g-estimations so that the ACDE estimates measure the direct effect with the mediators fixed at the mean values for all observations. Note that the results of the g-estimation should be interpreted with caution due to potential violations of the sequential unconfoundedness assumption: (1) the treatment (slave trade exposure) is conditionally ignorable given the pretreatment confounders, and (2) the mediators (population density and nightlight intensity) are conditionally ignorable given the treatment, pretreatment confounders, and intermediate confounders (Acharya et al., 2016a, 519).



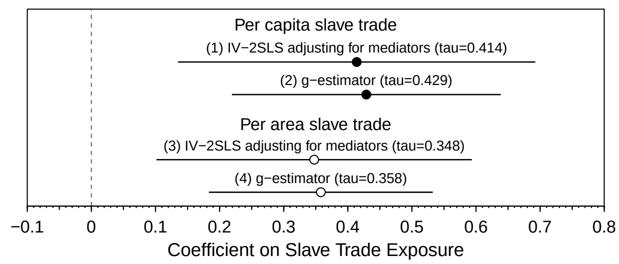
(a) Dependent variable: *Battle*



(b) Dependent variable: *Power Sharing*



(c) Dependent variable: *Rebel*



(d) Dependent variable: *Coup*

Figure C.2: IV-2SLS and Sequential G-Estimates Adjusting for Population Density and Nightlight Intensity in 2010

Notes: Dots indicate the point estimates with per capita (solid) and per area (hollow) slave trade exposure. Segments represent the corresponding 95% confidence intervals based on Kelly's (2020) spatial HAC standard errors with an exponential kernel (IV-2SLS) and Acharya et al.'s (2016a) consistent variance estimator (sequential g-estimator, implemented in DirectEffect package for R).

underlying causal pathways. By contrast, a nonzero ACDE implies the presence of causal channels not captured by the adjusted mediators (Acharya et al., 2016a, 517). To keep the estimates comparable, I plug the fitted values of the slave trade measures (instrumented by the cassava suitability) into the sequential g-estimation. In the sequential g-estimations, some of the group-level covariates are included as intermediate confounders, or posttreatment variables affected by the treatment while confounding the mediator-outcome association.[6]

Figure C.2 reports the results of the IV-2SLS regressions and sequential g-estimations adjusting for contemporary population density and nightlight intensity for each of the main outcome variables, with the per capita and per area slave trade measures. Consistent with the advanced institutional change mechanism, the estimates reveal limited roles of contemporary population size and regional development in linking the slave trade exposure and political

[6] The set of intermediate confounders includes *Border Distance*, *Capital* (dummy), *Capital Distance*, *Partition*, and *Population Density* (1960). The first-stage regressions do not adjust for these covariates to predict the slave trade exposure passed to the sequential g-estimator.

outcomes in postcolonial states. Regardless of model specifications, the treatment effect estimate remains stable and statistically significant, with the mediator-adjusted estimates remaining almost identical to the unadjusted estimates reported in the main text.

Also note that the association between precolonial centralization and contemporary development (e.g., Gennaioli & Rainer, 2007; Michalopoulos & Papaioannou, 2013) does not necessarily undermine the claimed political legacies of the slave trade. The suitability-based IV strategy remains valid as far as cassava suitability affects contemporary political outcomes and their correlates *exclusively* through the slave trade. Admittedly, the exclusion restriction assumption would be violated if cassava suitability instrument affects the main political outcomes through non-slave trade pathways. However, and reassuringly, a series of falsification tests reported in the next section also fail to detect such unblocked direct channels between slave trade exposure and modern political outcomes.

D Falsification Tests

The current IV design yields several testable implications for falsification tests: (1) the treatment take-up attributable to the instrument (first-stage association) should be absent in the pre-cassava arrival period; (2) the treatment take-up should be absent or weaker for non-cassava crop suitability; (3) the cassava instrument should not be associated with the correlates of armed conflicts and power sharing; (4) the cassava instrument should not be associated with the modern political outcomes in the subsample without the slave trade treatment; and (5) the main findings should disappear once we replace the treatment or the outcomes with artificial spatial noise. The first two implications correspond to the key identification idea that cassava's travel from the New World generated exogenous fluctuation in the endogenous treatment; and the third and fourth implications inform us about the validity of the exclusion restriction assumption.[7] The last implication addresses the concern

[7]Note that the first exercise can also inform us about the validity of the exclusion restriction assumption. The current IV design hinges on the assumption that cassava suitability affects the modern political outcomes

for spatial curve-fitting recently highlighted by Kelly (2019, 2020) with a particular focus on persistence studies.

To empirically test these implications, this article leverages (1) the arbitrary timing of cassava's arrival in Africa and the slave exports during the pre-arrival period (placebo treatment), (2) soil suitability measures for non-cassava crops (placebo instrument), (3) auxiliary reduced-form regressions, (4) North and South African observations barely exposed to the slave trade (placebo subsample), and (5) spatially autocorrelated artificial noise (placebo treatment and outcome). The main text reports the first exercise, and the remainder of this section reports the second to fifth tests.

D.1 Placebo Instrument: Soil Suitability for Non-Cassava Crops

Motivated by the approaches of Lowes & Montero (2020) and Miguel et al. (2004), the second falsification test focuses on the first stage and exploits the land soil suitability for non-cassava crops as placebo instruments.[8] The advanced IV design implies that the strong first-stage association should be specific to cassava. If cassava suitability, rather than omitted heterogeneity, influenced the group-level exposure to the slave trade, we have little reason to see systematic first-stage associations for the suitability measures for non-cassava crops.[9] Discernible first-stage associations for the placebo instruments thus invalidate the proposed IV strategy by suggesting that the cassava-slave export association reflects unadjusted heterogeneity correlated with the soil suitability for cassava as well as non-cassava crops.

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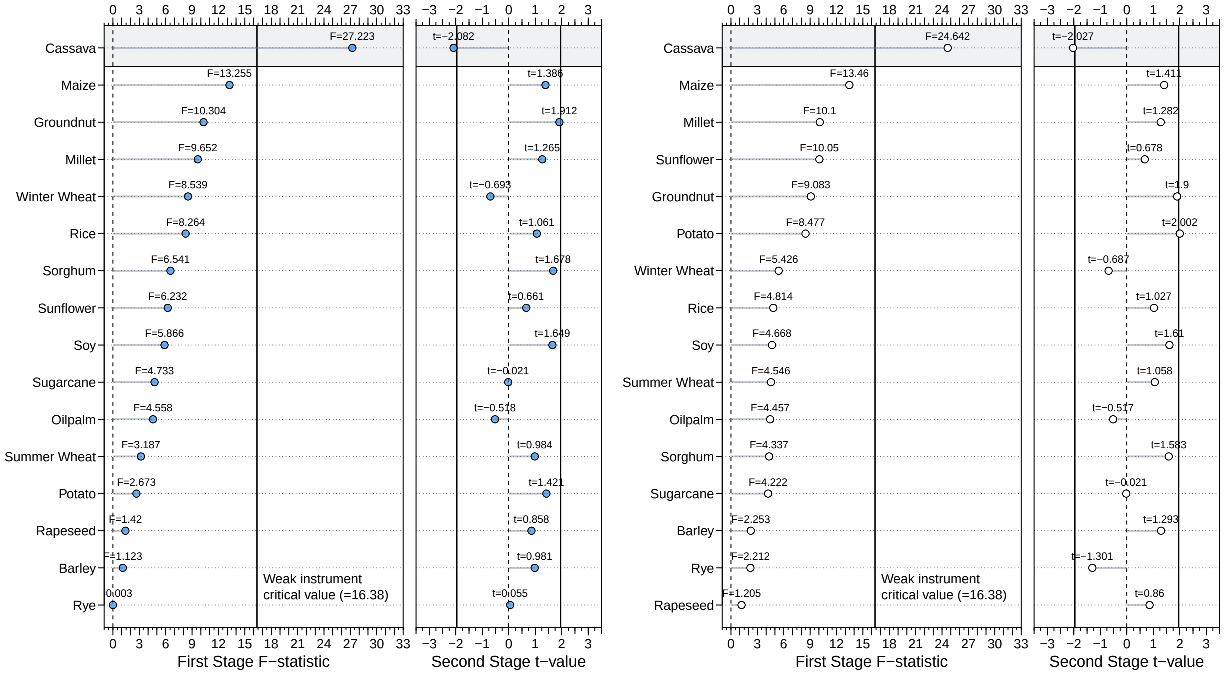
exclusively through the slave trade exposure *after* cassava's arrival in Africa. Therefore, any systematic pre-arrival first-stage association would violate the exclusion restriction assumption by suggesting instrument-outcome pathways other than the treatment, the exposure to the slave trade in the post-arrival period.

[8]Miguel et al. (2004, 736) present an identification check of a "false experiment" by regressing economic growth (endogenous treatment) on future rainfall shock (false instrument) instead of past rainfall shock (true instrument). Lowes & Montero (2020) estimate a series of reduced-form regressions with placebo instruments, non-cassava crop suitability relative to millet suitability, instead of their main instrument, cassava suitability relative to millet suitability.

[9]The analysis focuses on the first-stage associations between non-cassava crop suitability and the slave trade exposure, rather than reduced-form associations. Even if the suitability measure for a non-cassava crop is systematically associated with political outcomes, the lack of first-stage associations undermines the crop's ability to serve as an instrument for the slave trade. Even in the absence of a first-stage association, it is still possible that the non-cassava crop influences political outcomes through *non-slave trade* pathways.

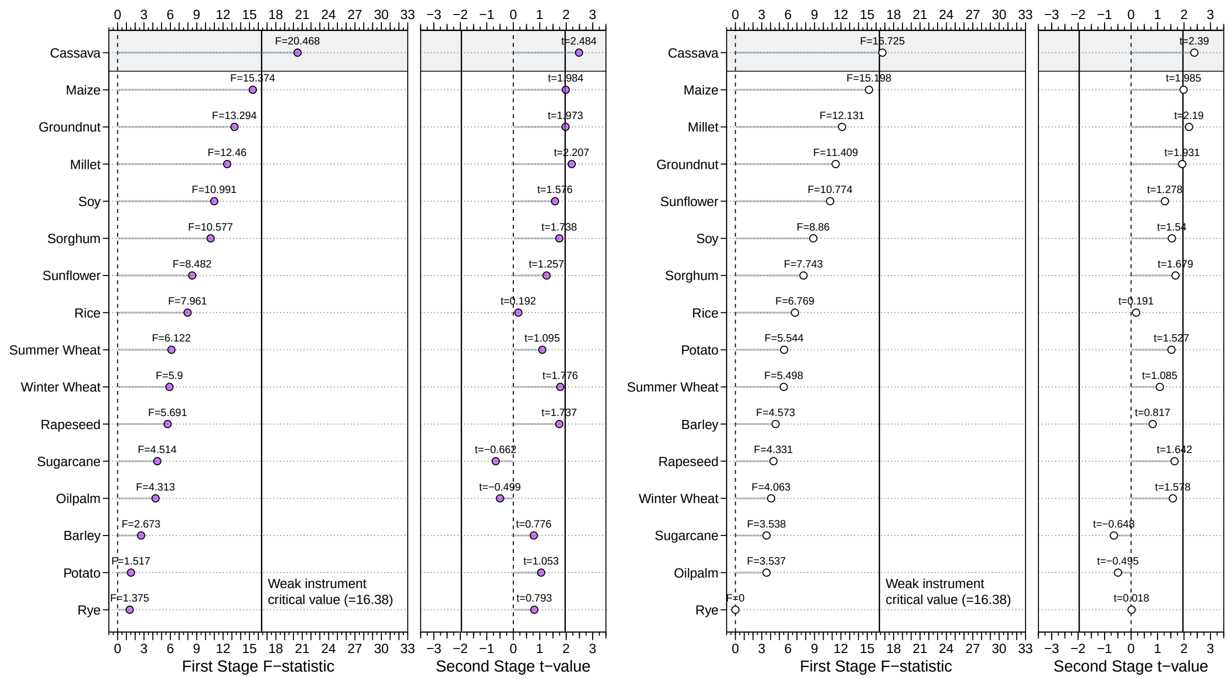


(a) Population-Normalized Slave Trade Export (b) Area-Normalized Slave Trade Export

Figure D.1: IV-2SLS Regressions with Cassava Suitability and Placebo Instruments

Notes: First-stage F-statistics with (a) population-normalized (solid) and (b) area-normalized (hollow) slave trade exposure measures as the first-stage outcomes (in the left panels), along with the corresponding second-stage t-values for (instrumented) per capita or per area slave trade exposure, with the soil suitability measures for cassava and non-cassava crops as (placebo) instruments. The model specification follows column (2) of Table 2 in the main text. Each dot in the left (right) panel indicates the first-stage F-statistic (second-stage t-value) for the crop on the vertical axis. The reported F-statistics and t-values are based on Kelly's (2020) spatial HAC standard errors. Solid vertical segments indicate the critical value of 16.38 (Stock & Yogo, 2005) against weak instruments in the left panels, and the critical values for the statistical significance at the 5% level in the right panels ($|t| = 1.96$).

To investigate this concern, I subsequently replace the cassava suitability index with the placebo suitability measure for one of the 15 crops available in the GLUES data and reestimate the IV-2SLS regressions. The left panes of Figures D.1 and D.2 plot the F-statistics for the placebo instruments and the cassava suitability measure (true instrument), with population-normalized and area-normalized slavery measures as the first-stage outcome for all country-group observations (Figure D.1) and the LEDA-connected observations (Figure D.2). The model specification follows column (2) of Table 3 in the main text. Each dot represents the first-stage F-statistics for each crop suitability, and the solid vertical segments indicate the critical value of 16.38 for weak instruments. The right panels display



(a) Population-Normalized Slave Trade Export (b) Area-Normalized Slave Trade Export

Figure D.2: IV-2SLS Regressions with Cassava Suitability and Placebo Instruments, with the LEDA-Connected Observations

Notes: See notes in Figure D.1. The LEDA-connected sample includes 939 country-group observations with the LEDA linguistic distance threshold of 0.2, as in the baseline setting. The t-values in the right panels are computed based on the second-stage estimates with *Power Sharing* as the dependent variable.

the corresponding second-stage t-values for per capita and per area slave trade measures instrumented by the cassava and non-cassava crop suitability indexes.

For all of the 15 non-cassava crops, the first-stage F-statistics fail to pass the critical value of 16.38 to reject the null hypothesis that the instrument is weak. Somewhat consistent with the country-level findings of Cherniwchan & Moreno-Cruz (2019), among the 15 non-cassava crops, maize suitability is most strongly associated with the slave trade exposure. However, regardless of the slavery measures and (sub)samples, the maize-slave trade association remains weaker at the ethnic-group level than at the country level and fails to pass the critical value of 16.38. These weaker first-stage associations for placebo instruments underline the distinctive role of soil suitability for cassava, rather than other Old World and New World crops, in shaping the group-level exposure to the slave trade.

Similarly, the second-stage t-values of the "fake" IV regressions with non-cassava instruments almost always fail to retain statistical significance at the conventional 5% level, and the corresponding second-stage explanatory power (measured by t-values) are consistently weaker than the cassava-based estimates. Admittedly, it is possible that non-cassava crop suitability affects contemporary political outcomes through pathways other than the slave trade. For example, Mayshar et al. (2017) reveal the effect of cereal cultivation on group-level institution-building by highlighting the "productivity advantage of cereals over roots and tubers, unlike absolute land productivity, is the source of hierarchy." (p.7). An important implication of their earlier findings is that using the suitability for a cereal grain (e.g., maize), rather than a perennial root (e.g., cassava), as an instrument for the slave trade exposure would severely violate the exclusion restriction by opening up *non-slave trade* pathways toward the advanced mediator (i.e., traditional institutions) and outcomes. Although the current analysis tells us little about the plausibility of such non-slave trade pathways, the false IV estimates fail to support the validity of non-cassava crop suitability as an instrument to investigate the political legacies of the slave trade.

D.2 Non-Treatment Pathways: Slave Trade and the Correlates of the Coup-Civil War Trap

Third, reduced-form regressions with additional outcome variables also inform us about potential exclusion restriction violation induced by cassava-outcome pathways not through slave trade exposure. For example, a combination of systematic associations (1) between cassava suitability and current population density in group homelands and (2) between current population density and battle activities opens up a direct instrument-outcome pathway. Such potential pathways are consistent with the insights of previous studies (e.g., Cederman et al., 2010; Collier & Hoeffler, 2004; Fearon & Laitin, 2003; Francois et al., 2015), and can severely violate the instrument exclusion restriction. Not to invite such concerns for the exclusion restriction violations, the instrument should not be associated with the (unadjusted) correlates of civil war or power sharing.

Table D.1: Reduced-Form Regressions with the Main Outcomes and the Correlates of the Coup-Civil War Trap

	Main Outcomes				Potential Correlates	
	$\ln(1+\text{Battle})$	Power Sharing	Rebel	Coup	Nightlight	Population Density
	(1)	(2)	(3)	(4)	(5)	(6)
Cassava Suitability (ζ)	-0.185^{*}	0.038^{**}	-0.039^{**}	0.080^{**}	-0.004	-0.062
	(0.086)	(0.013)	(0.012)	(0.023)	(0.061)	(0.046)
Overall Suitability	0.113	-0.016	0.036^{*}	-0.013	-0.136^{+}	0.347^{**}
	(0.102)	(0.024)	(0.016)	(0.022)	(0.081)	(0.063)
Coast Distance	-0.018	0.042^{*}	0.013	0.089^{**}	-0.457^{**}	-0.038
	(0.100)	(0.018)	(0.015)	(0.030)	(0.081)	(0.064)
Observations	1,282	939	939	939	1,282	1,282
Adjusted R^2	0.561	0.731	0.819	0.723	0.606	0.787
Residual Moran's I (standard deviate)	-0.019	0.109	0.004	0.962	0.693	-1.123
Country FE	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
LEDA-connected sample		✓	✓	✓		

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) standard errors adjusted for spatial clustering with an exponential kernel are in parentheses. Restricted controls: Area, Grassland in 1500, Overall Suitability, and Population Density in 1500. Precolonial controls: Coast Distance, Cropland in 1500, Ecological Diversity Index, Malaria Suitability Index, Trade Route Cities, Cities in 1500, Precolonial Conflict, and Precolonial Kingdom. Geographic controls: Area Share, Average Temperature (1901–1910), Equator Distance, Elevation, Ruggedness, Partition, Capital (dummy), Capital Distance, Border Distance, Population Density in 1960, and Water Body. See Tables A.1 and A.2 for a detailed description of the covariates.

To investigate the concern, I estimate the following reduced-form model:

$$Y_{ic}^{RF} = \kappa_c + \zeta Cassava_i + \boldsymbol{X}_i'\boldsymbol{\lambda} + \boldsymbol{X}_{ic}'\boldsymbol{\phi} + \boldsymbol{E}_{ic}'\boldsymbol{\psi} + f(\boldsymbol{s}_{ic}) + v_{ic}, \tag{D.3}$$

where the right-hand-side variables and parameters are defined analogously to the two-stage specification in the main text, with $\boldsymbol{E}_{ic}$ denoting the Moran eigenvectors. Y_{ic}^{RF} is one of logged nightlight intensity, *Nightlight*, logged population density in 2010, *Population Density*, and the main outcome variables, *Battle*, *Power Sharing*, *Rebel*, and *Coup*.

The reduced-form regressions inform us about the instrument validity in two ways. First, to be a relevant instrument, cassava suitability should predict the main outcomes ($\zeta \neq 0$). Second, not to violate the exclusion restriction, the instrument should *not* be associated with the correlates of the coup-civil war trap, *Nightlight* and *Population Density* ($\zeta = 0$), which would open the instrument-outcome pathways not through the slave trade.

Table D.1 reports auxiliary reduced-form estimates. Reassuringly, we see no systematic associations between cassava suitability and nightlight intensity or population density. Also note that overall agricultural suitability and coastline distance are systematically associated with the key correlates of domestic fighting and power sharing. These results reveal no clear signs of the exclusion restriction violations in the suitability-based IV design and thereby provide additional credibility to the main findings while discouraging the use of alternative instrument candidates in the current analysis.

D.3 Placebo Sample: Groups in North and South African States

The fourth exercise utilizes a negative control (placebo) sample with "a similar confounding structure as the population of interest but was not exposed to the treatment of interest" (Davies et al., 2017, 2069). In the current context, ethnic groups in today's North and South African states have barely been exposed to the slave trade and constitute a plausible negative control subsample. Because the slavery pathway ("cassava suitability $\rightarrow$ slave trade $\rightarrow$ outcome variables") is absent in the subsample, we should not see any reduced-form association between cassava suitability and the outcomes (Davies et al., 2017, 2070).[10] Systematic instrument-outcome associations in the negative control subsample suggest the presence of unblocked direct pathways linking the instrument and the outcomes, which violates the assumption of exclusion restriction. Based on the United Nations region code, 248 out of 1,282 country-group observations (19.34%) for the full sample and 197 out of 939 observations (20.98%) in the LEDA-connected subsample are located in North and South African states. Figure D.2 depicts the distribution of the negative control observations.

Table D.2 reports the subsample reduced-form estimates with the model specification of equation (D.3), with $f(\boldsymbol{s}_{ic})$ specified as a linear polynomial of settlement coordinates given

[10]Note that the IV estimator can be written as the ratio of the reduced-form instrument-outcome association relative to the first-stage instrument-treatment association (Angrist & Pischke, 2008, 120–121). A zero reduced-form association thus immediately indicates a zero second-stage association. Nunn & Wantchekon (2011) adopt a similar reduced-form strategy with a placebo sample as a falsification test by leveraging non-African observations immune to African slave trades. Acharya et al. (2016b) also employ a similar subsample approach in the context of the slavery legacies in the United States.

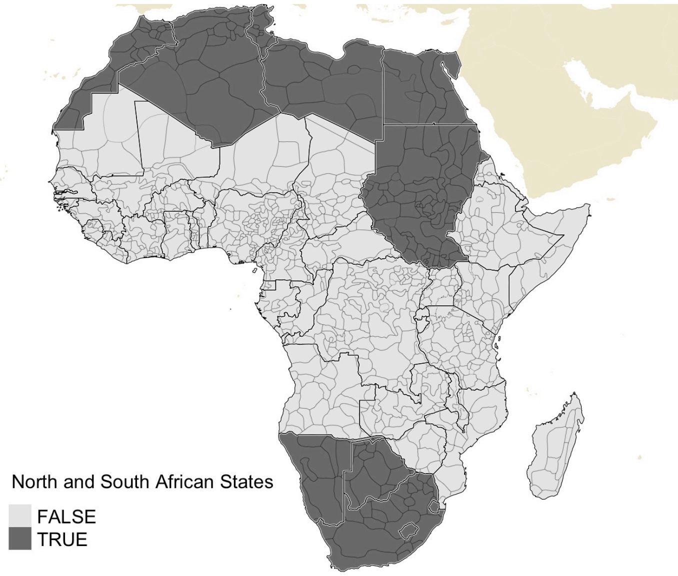


Figure D.3: Country-Group Observations (Not) Falling into North and South African States
Notes: Darker shades represent the settlement areas of the country-group observations in North and South African states based on the United Nations region code.

the reduced sample size. Consistent with the IV strategy, the coefficient estimates remain statistically and substantially indistinguishable from zero. An exception is the marginally significant association between cassava suitability and battle exposure in column (1), with the corresponding t value of $t = \frac{0.234}{0.133} \approx 1.756$. However, the reduced-form coefficient is positive and inconsistent with the main finding of the negative slave trade-battle association, which again fails to invalidate the IV design.

D.4 Spatial Noise Placebo Test

Finally, a remaining but important concern stems from the spatial nature of historical phenomena rather than the IV design, such that the regression estimates reflect spatial trends or heterogeneity rather than the legacies of the slave trade. Kelly (2019) proposes a two-step diagnosis procedure to guard against the methodological concern of spatial curve-fitting. The first diagnosis procedure is to simply report the Moran's I statistic of spatial autocorrelation for regression residuals. A statistically significant Moran's I statistic in regression residuals warns the danger of curve-fitting. The second procedure is to replace the key treatment

Table D.2: Reduced-Form Regressions for the Negative Control Observations

	$\ln(1+\text{Battle})$ (1)	Power Sharing (2)	Rebel (3)	Coup (4)
Cassava Suitability (ζ)	0.234^{+}	0.006	-0.001	-0.011
	(0.133)	(0.026)	(0.013)	(0.027)
Observations	248	197	197	197
Adjusted R^2	0.608	0.381	0.943	0.774
Residual Moran's I (std. deviate)	0.258	-1.724^{+}	0.337	0.387
Country FE	✓	✓	✓	✓
Lon-Lat polynomial (linear)	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓
Restricted controls	✓	✓	✓	✓
Precolonial controls	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓
LEDA-connected sample		✓	✓	✓

Notes: $^{+}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$. Kelly's (2020) standard errors adjusted for spatial clustering with an exponential kernel are in parentheses. Restricted controls: Area, Grassland in 1500, Overall Suitability, and Population Density in 1500. Precolonial controls: Coast Distance, Cropland in 1500, Ecological Diversity Index, Malaria Suitability Index, Trade Route Cities, Cities in 1500, Precolonial Conflict, and Precolonial Kingdom. Geographic controls: Area Share, Average Temperature (1901–1910), Equator Distance, Elevation, Ruggedness, Partition, Capital (dummy), Capital Distance, Border Distance, Population Density in 1960, and Water Body. European influence indicators: Colonial Railway, Explore Routes, and Missions. See Tables A.1 and A.2 for a detailed description of the covariates.

and outcome variables with artificial spatial noise. The empirical findings will be invalidated (1) if spatially autocorrelated noise explains the outcome variable (spatial noise as a placebo treatment) and (2) if the key treatment explains the artificial noise (spatial noise as a placebo outcome). While the Moran's I statistics fail to retain statistical significance at the conventional 5% level in all the regressions reported in this article, the concern for spatial curve-fitting warrants a further investigation through a spatial noise placebo test.

The spatial noise simulation involves two steps. I first generate spatially autocorrelated random variables with the Moran's I statistics similar to the I statistics obtained from the observed slave export and outcome variables.[11] I then reestimate the main IV-2SLS regression specified by equations (1) and (2) in the main text with either the treatment (*Slave*)

[11] For a comparative purpose, I follow the procedure of Bivand et al. (2013, 257–258) to generate spatially autocorrelated random variables measured at the original unit of analysis (country-group) using a distance-based spatial weight matrix (SWM) with a $5^\circ (\approx 555$ km) cutoff as in the main analysis. The univariate distribution of the noise variable follows the standard normal distribution. As reported below, the SWM-based procedure ensures that the artificial variables are measured at the same level as the observed variables.

Table D.3: Spatial Noise Simulation

ρ	Mean Moran's I	Spatial Noise Replaces:	% Statistically Significant at: $p = 0.05$	$p = 0.01$	$p = 0.001$	% Spatial Noise Outperforms
0.8	0.185	Treatment (spatial noise as a placebo treatment)	2.51%	0.00%	0.00%	0.24%
		Outcome (spatial noise as a placebo outcome)	9.24%	1.86%	0.07%	
0.85	0.254	Treatment	3.53%	0.00%	0.00%	0.39%
		Outcome	11.55%	2.66%	0.08%	
0.9	0.368	Treatment	5.33%	0.00%	0.00%	0.81%
		Outcome	14.34%	4.25%	0.21%	
0.925	0.458	Treatment	7.21%	0.00%	0.00%	1.13%
		Outcome	17.61%	5.12%	0.4%	

Notes: Spatial autoregressive parameter ρ (in the first column) controls the degree of spatial autocorrelation in the artificial noise. The fourth to sixth columns report the proportions of simulation runs where the noise variable (as a placebo treatment) or the treatment (with the noise variable as a placebo outcome) retains statistical significance at level p. The last column reports the proportion of simulations where the spatial noise variable outperforms the original treatment ($Slave^{\text{pc}}$) in explanatory power measured by the second-stage t-value, which can roughly be interpreted as the empirical significance of the treatment variable (Kelly, 2019, 16). The Moran's I statistics obtained from the observed treatment and outcome variables are $I^{\text{Slave(pc)}} = 0.33$, $I^{\text{Slave(Area)}} = 0.319$, $I^{\text{Battle}} = 0.195$, $I^{\text{Power Sharing}} = 0.304$, $I^{\text{Rebel}} = 0.477$, and $I^{\text{Coup}} = 0.38$.

or the outcome (Y) replaced with the noise variable. I repeat the two-step procedure 10,000 times for each of the spatial autocorrelation settings parameterized by spatial autoregressive parameter ρ to obtain the empirical distributions of the key statistics.[12]

Table D.3 summarizes the simulation results with different levels of spatial autocorrelation. The second column reports the mean value of the Moran's I statistics of spatial correlation obtained from simulations with different values of ρ. The fourth to sixth columns summarize the proportions of artificial regressions out of 10,000 simulation runs in which (1) the spatial noise variable (as a placebo treatment) retains statistical significance in the second stage (with *Battle* as the second-stage outcome variable) and (2) the treatment ($Slave^{\text{pc}}$) is systematically associated with the spatial noise (as a placebo outcome) with the statistical significance at p level denoted in the table header. The last column reports the proportion of

[12]To speed up the simulations, I fit the IV-2SLS estimations without the Moran eigenvector spatial filtering. The t-values and statistical significance reported below are based on the Huber-White robust standard errors. For a comparative purpose, I also reestimate the baseline ("true") IV-2SLS regression without the Moran eigenvectors and obtain the t-value based on robust standard errors.

the simulation runs where the noise variable outperforms the original treatment in explanatory power measured by the second-stage t-values. The noise outperformance proportion serves as a rough proxy of the empirical significance level of the treatment (Kelly, 2019, 16).

The spatial noise simulation yields four patterns and provides additional confidence in the main findings. First, the spatial noise variable rarely explains the original outcomes. For example, with $\rho = 0.8$ and at the conventional 5% significance level, the noise variable is systematically associated with *Battle* in only 251 (2.51%) out of 10,000 runs. Regardless of the autoregressive parameter settings, the explanatory power of the noise variable remains similar to or lower than random chance. Second, $Slave^{\text{pc}}$ rarely explains the noise variable. As a reference, although the proportion is higher compared to the first exercise, $Slave^{\text{pc}}$ is systematically associated with spatial noise at the 5% level in only 942 (9.42%) of simulation runs with $\rho = 0.8$. Also note that the Moran's I statistic of one of the original outcome variables, *Battle*, is 0.195 and located close to the mean of the Moran's I statistics, 0.185, obtained from the noise simulations with $\rho = 0.8$. In other words, despite the similar degrees of spatial autocorrelation, the explanatory power of $Slave^{\text{pc}}$ turns out to be negligible once we replace the outcome with the noise variable. Third, the simulations yield a positive association between the degree of spatial correlation of the artificial noise (parametrized by ρ) and the proportions of simulation runs where $Slave^{\text{pc}}$ explains spatial noise or the noise explains the original outcome. This positive association is consistent with the original simulation experiments by Kelly (2019) across major previous works in the persistent-effect literature. Finally, as reported in the seventh column, the noise variable rarely outperforms the original treatment. The empirical significance measure ranges from 0.24% (with $\rho = 0.8$) to 1.13% (with $\rho = 0.925$), and is generally similar to or lower than the nominal significance based on Kelly's (2020) spatial HAC standard errors reported in the main regression tables. Combined, these simulation results fail to falsify the main findings. If anything, the noise simulation reveals only a weak caution against spatial curve-fitting.

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