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 and communities shape subsequent political outcomes? I argue that the demographic shock to indigenous societies induced by Africa's slave trades influences postcolonial politics by improving ethnic institutions and leadership, thereby affecting the coup-civil war trade-off and the underlying commitment problems. The empirical analysis leverages land soil suitability for cassava cultivation to exploit plausibly exogenous variation in the ethnic group-level exposure to slave raids. The findings are four-fold: Ethnic groups with severer slave raid 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. (148 words)

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Keywords

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

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How do historical events and institutions persistently affect subsequent political outcomes? Previous literature highlights three distinct pathways. First, institutions persistently impose constraints on individual behavior and interactions. Historical institutions and community ties shape contemporary economic performance and propensity to engage in 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 destruction 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 breakdown of traditional polities (e.g., Lowes et al., 2017) as well as the damages to communities inflicted by forms of political violence (e.g., Bellows & Miguel, 2006; Gilligan et al., 2014; 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 long-run economic growth of West European states after the sixteenth century through both direct channels of increased economic profits and indirect pathways of facilitated institutional change to assure secure property rights (Acemoglu et al., 2005).

This article expands the third pathway by exploring the long-run impacts of the transatlantic and Indian Ocean slave trades on power sharing and armed conflicts in postcolonial Africa. I argue that descendant ethnic groups with greater historical exposure to slave raids 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 institutional constraints. The insights of existing studies suggest that the slavery-induced shocks improved local political authority and empowered local chiefs (Whatley, 2014). Despite the absence of electoral incentives for inherent chiefs, related studies also uncover the improved collective action capacity and effective leadership of chiefdoms in contemporary Africa (Acemoglu et al., 2014; Baldwin, 2016). The improved ethnic institutions and group collective action capacity enable the leadership to make credible threats and promises, which in turn alleviates the war-causing commitment problems (Fearon, 1995; Powell, 2006).

Empirically, it leverages the soil agricultural suitability for cassava (manioc) as an instrument to exploit plausibly exogenous variation in the ethnic group-level exposure to slave raids. 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 periods spanning from the fifteenth to the nineteenth centuries (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 — just as maize (corn, Cherniwchan & Moreno-Cruz, 2019) — while magnifying slave raiders' incentives and capabilities for slave capture in the suitable regions, thereby exacerbating exposure to slave exports among the local inhabitants.

The first-stage results confirm the ability of soil suitability for cassava to predict group-level exposure to the transatlantic and Indian Ocean slave trades. The second-stage regressions then reveal four empirical patterns consistent with the argument: Ethnic groups with severer slave raid exposure 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. These results remain robust to an alternative slavery exposure measure as well as the adjustments for a vast set of covariates.

A methodological concern for the soil-based IV design is potential violations of the exclusion restriction. For example, one might expect agricultural suitability to affect current economic performance, thereby shaping the local-level susceptibility to political violence. To address this identification concern, the falsification test exploits the arbitrary timing of cassava's arrival in Africa. Unlike maize, which traveled to Africa from the New World in the early sixteenth century and diffused rapidly, cassava was introduced to the continent later and spread across the continent after the seventeenth century (Alpern, 1992, 24–26; Crosby, 1972, 185–188). Its delayed arrival left the earlier parts of the slave trades unaffected while opening up a pathway from soil suitability for cassava to the slavery exposure in the later periods. We thus have little reason to see a systematic association between cassava suitability and slave trade exposure *before* cassava's arrival. The falsification exercise confirms the expectations and lends further credibility to the findings. Additional falsification tests further demonstrate that the first-stage association between soil suitability and slave exports becomes less visible once the instrument is replaced with suitability for non-cassava crops.

This article contributes to broader literature in three ways. First, it speaks to the growing literature on the persistent effects of historical events and institutions on modern outcomes, particularly the studies on the historical roots of contemporary civil conflicts. Some historical events such as precolonial conflicts and ethnic partitioning by colonial border design escalate postcolonial conflicts (e.g., Besley & Reynal-Querol, 2014; Michalopoulos & Papaioannou, 2016), while other institutions including precolonial political centralization have a pacifying effect (e.g., Wig, 2016). By revealing the conflict-reducing legacies of the slave trade, this article sheds light on another historical determinant of contemporary politics. Second, this article contributes to broader political science literature. Revealing the 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; Wucherpfennig et al., 2016). The long-run political consequences of slavery shock also provide another piece of insight into how violence and institutions interact to generate political outcomes. Finally, the findings deepen our understanding of the persistent legacies of the slave trade. Previous literature demonstrates the persistent effects of slavery on, for example, economic growth (Nunn, 2008), traditional political institutions (Whatley, 2014), and trust attitudes (Nunn & Qian, 2011). What remains less clear is the possible impact of the slave trade on postcolonial politics, and this article provides empirical insights on the political legacies of the slave trade in Africa.

1 Slavery, Legacies, and the Coup-Civil War Trade-Off

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

1.1 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 slavery-induced shocks generated "tragically interconnected transformations" into African society (Manning, 1990, 147). 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 slavery-induced transformations. Nunn & Wantchekon (2011) demonstrate how the history of slave trades undermined the interpersonal trust such that individuals with ancestor ethnic groups heavily exposed to the slave raids are less trusting others and political authority today. Dalton & Leung (2014) and Teso (2019) leverage the abnormal distortions in sex ratios induced by the slave traders' preference for males to females in the transatlantic slave trade (Manning, 1990). Their empirical results suggest that the shortage of males facilitated the spread of polygyny and gender equality, or social norms and informal institutions that have persisted to the present day.

This article is most closely related to Whatley (2014) on the impact of the slave trade on precolonial political authority in West Africa. Utilizing the port-level records of slave exports, Whatley (2014) demonstrates that in West Africa, 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, suggesting a slavery-induced transformation of political authority (see also, Obikili, 2016). As Whatley (2014) argues, a possible explanation rests on the increased demands of group members for protection from slave raids: 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" (471). Whatley (2014) further argues and uncovers that the slavery-induced authority survived the colonial periods, as the colonial authorities, both French and British, relied on the existing local authority structure to govern and extract resources in West Africa.

As these studies highlight, Africa's slave trade and inflicted demographic shocks left lasting legacies by displacing inhabitants, damaging the existing formal and informal institutions, and facilitating social and political transformations. What remains less clear is the impact of slavery on postcolonial politics, which this article investigates.

1.2 Bargaining in the Shadow of History and Violence

Another body of literature closely related to this article focuses on the link between traditional ethnic institutions and postcolonial politics. While the direction of the causality remains disputed, civil war literature underscores the role of traditional institutions of ethnic groups in shaping contemporary politics. For example, Wig (2016) demonstrates that ethnic groups with centralized traditional institutions are more able to make credible commitments and experience decreased risks of ethnic conflicts in postcolonial Africa (see also, Depetris-Chauvin, 2016). In contrast, Paine (2019) highlights the conflict-escalating effect of traditional institutions, such that ethnic 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 strategic interactions between distinct ethnic groups.[1] Rather than a single "big man" dominating the political realm, a distinctive feature of contemporary African politics is the strategic interactions in the shadow of violence (Francois et al., 2015; Roessler, 2011, 2016; Roessler & Ohls, 2018). Distinct ethnic groups with differing interests and access to state power often compete for the state power with the threat of armed uprising. Rulers and ruling groups strategically allocate rents and access to state power to buy off potential rivals and thereby prevent an outside rebellion. Both sustained power sharing and failure of such peace arrangements emerge as equilibrium outcomes from these strategic interactions.

[1]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; Posner, 2004; Roessler, 2011) and limit access to the spoils (Fearon, 1999).

Related literature conceives these strategic situations as the coup-civil war trade-off or power-sharing dilemma (Paine, 2020; Roessler, 2011, 2016; see also, Francois et al., 2015; Svolik, 2012). 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 power sharing schemes 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 the substitute 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 in the literature how group-level institutions affect the general coup-civil war trade-off. If traditional institutions shape groups' abilities to make credible commitments, such institutions and the transformations thereof should also influence the severity of the general trade-off and the prospects for domestic peace.

2 How Slave Raid Legacies Shape Postcolonial Politics

This article extends the insights of previous studies to distinguish two distinct channels that link the slave trades and contemporary politics in Africa. The first channel directly follows from the existing insights on the individual-level legacies of the slave trades. As Nunn & Wantchekon (2011) demonstrate, individuals whose ancestor ethnic groups severely suffered from the slave trades remain less trusting others today. If deteriorated interpersonal trust impedes group collective action and mobilization, then the ethnic groups heavily exposed to slave raids are expected to be less likely to engage in active fighting, with decreased chances to be granted access to state power. Remaining less able to organize member behavior and make credible threats of force, groups with severer exposure to slave raids would also remain "too weak" to fight and to be granted power-sharing spoils in postcolonial states.

2.1 Institutional Change and the Coup-Civil War Trade-Off

Rather than the weakness, this article advances a second channel highlighting the slavery-induced institutional change, which would improve group collective action capacity and bargaining leverages. As Whatley (2014) finds, slavery exposure facilitated institutional transformations in indigenous communities toward an absolutist political structure. Once established, chiefs in the communities with a *less* competitive, absolutist political structure would have increased incentives to invest in group-level institutions to organize collective actions (Baldwin, 2016). "[E]conomically and socially embedded in their societies" and expecting to "rule for life," absolutist 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 organize group behavior and earn trust, thereby consolidating their authority and securing their long-run payoffs. Consistent with the reasoning, a Ghanaian local expresses, traditional chiefs lack formal political power and thus "have to earn trust" of the local population to sustain their authority.[2] Similarly, Acemoglu et al. (2014) and Bellows & Miguel (2006), respectively, uncover positive associations between less-competitive chiefdoms and enhanced local collective action capacities, and between exposure to wartime violence and political mobilization in Sierra Leone. Limited abilities of central governments to project power and the reliance on the local authority for governance of peripheral areas also contribute to the survival of traditional chiefdoms during the postcolonial period (Herbst, 2000; Mamdani, 1996).

The deteriorated interpersonal trust emphasized in Nunn & Wantchekon (2011) does not necessarily contradict with the improved ethnic institutions. Indeed, the undermined trust and informal institutional constraints can facilitate endogenous investments and individual incentives to build stronger, costly, and otherwise unnecessary, institutional constraints to monitor and control group members. In parallel with state-like, centralized traditional institutions (Wig, 2016) and historical statehood experiences (Depetris-Chauvin, 2016), the ironically improved institutional constraints enhance group collective action capacity and the leadership's ability to monitor and control group members.

The improved group-level institutions and local political authority affect postcolonial politics by altering the general coup-civil war trade-off in two ways. 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) emphasize, a key determinant of the ruler's decision to offer a power-sharing deal is the potential rival's capabilities to credibly threaten and violently overthrow the regime. Political exclusion and inclusion constitute strategies of consolidating state power, and rulers have little

[2] 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.

incentive to grant power-sharing spoils to outsider groups without the coercive capacity to challenge and recapture sovereign authority. Mobilizational potential combined with collective action capacity permits a group to credibly threaten to rebel from outside and sustain costly fighting, which in turn gives rise to rulers' incentives to grant power-sharing spoils.

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 (Roessler, 2016; Roessler & Ohls, 2018), collective action problems also play a key role in altering the credibility of the threat of an outsider rebellion. Specifically, without institutional devices to organize group behavior or endowments to offer selective (dis)incentives, demographically larger groups can suffer from severer collective action problems (Olson, 1965). Higher mobilizational potential can boost a group's ability to challenge the central government with force. Yet the acute collective action problems for a demographically larger group can simultaneously impede actual mobilizational capacity and threat credibility. Therefore, all else being equal, group-level institutional devices to organize member behavior also play a crucial role in altering the group's ability to make credible threats of outside attacks against political exclusion.

Second, institutions also allow an ethnic group to make credible promises to follow through power-sharing deals and not to stage an inside coup once granted power-sharing spoils. The inclusion of potential rivals in the state power-sharing schemes entails an increased risk of coups from inside, and the inside coup risk is partly a function of the rival's coercive capacity and leadership effectiveness. Indeed, Paine (2020) highlights that the coercive capacity to stage an outside armed uprising "also enhances the elite's ability to challenge via a coup" (3). To alleviate the side effect of improved coercive capacities, therefore, an ethnic group needs to credibly promise to follow through the power-sharing deals and reassure the ruling groups. Meanwhile, as Cunningham (2013) argues, the absence of uncontested leadership undercuts the group's ability to credibly commit to following through prior deals. Even if the current leadership promises not to stage an inside coup, such commitments remain incredible in the absence of effective group authority or the presence of internal fractionalization. Enhanced institutional constraints and uncontested group authority permit the leadership to monitor and control group members and improve the credibility of its commitment, which in turn mitigates the fears of the rulers for insider coups.

For a power-sharing deal to be honored and self-enforcing, a ruler, as well as an outsider group, must credibly commit to the political bargain. Here, as Paine (2020) argues, rulers not only decide whether or not to include an outsider group but also strategically choose the amount of the spoils distributed to the group (2–3). A possible strategy for a ruler to make credible commitments to share power is to grant pieces of state power that allow the outsider groups for effective resistance against the rulers, such as state military organizations. Yet such spoils also contribute to the coup technologies of outsider groups, which in turn makes coup attempts more feasible and can result in higher risks of coup attempts (Paine, 2020; Roessler, 2016; Roessler & Ohls, 2018).

Combined, the improved institutions and leadership shape postcolonial politics by influencing the coup-civil war trade-off, by contributing to group capabilities to make credible threats of force and credible promises to follow through power-sharing deals. Group-level institutional constraints contribute to the group's ability to make credible threats for improved collective action capacity, and effective leadership to monitor and control group members allows for making credible promises. If ethnic groups with severer exposure to slave raids are better-equipped with such institutional devices, we would expect that severer slavery exposure 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.

2.2 Non-Institutional Pathway: Geographic Sorting

An implicit assumption behind both of the two mechanisms above is that slavery legacies primarily operate through group-level pathways. The first mechanism expects slave trade exposure to affect postcolonial politics by undermining bargaining leverage and group capabilities to fight, while the second stresses the role of the slavery-induced institutional change and incentives to build institutional constraints.

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, slave raids create land lots by forcibly removing local inhabitants and could also facilitate subsequent population influx into the cleared lots. The reshuffling of the local population can remove the preexisting inter-group rivalries and hatreds, and the new inhabitants might be less conflict-prone or less equipped with collective action capacity, which can alter the location-level, though not group-level, risks of postcolonial conflicts. This geographic sorting pathway can also generate a negative association such that conflict events are less likely to occur within the traditional homelands of ethnic groups with ancestors heavily exposed to slave raids.

The following analysis separately examines the long-run effect of slavery on location-level and group-level outcome variables to distinguish the underlying mechanisms. While the geographic sorting pathway, as well as the two institutional perspectives, predicts a negative association between slave raid exposure and postcolonial conflicts at the location level, the

Table 1: Testable Predictions

	Location level	Group level		
Mechanism	Battle	Outsider Rebellion	Power Sharing	Insider Coups
Institutional change (main proposition)	−	−	+	+
Distrust/weakened group ties	−	−	−	−
Geographic sorting	−	NA	NA	NA

influx proposition does not expect the negative association at the group level. The two group-level accounts generate contrasting predictions for power sharing chances and coup risks. Table 1 summarizes the testable predictions.

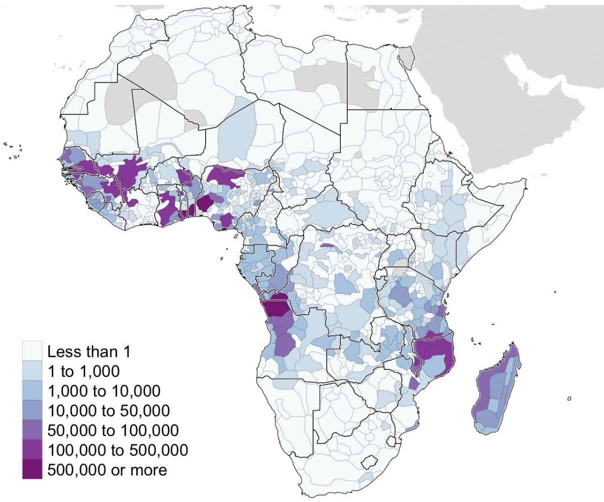
3 Research Design

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

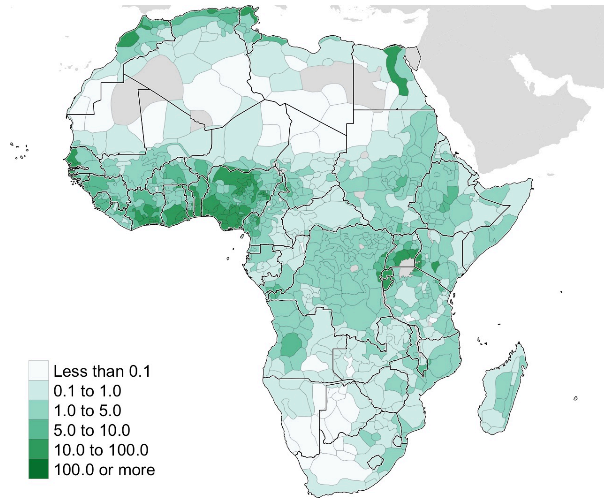
3.1 Data and Measurement

The unit of analysis is ethnic groups nested by host countries. 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.

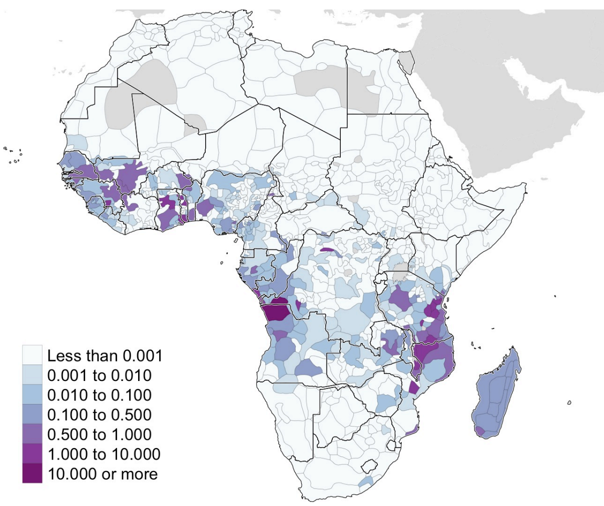
The group-level records of slavery exposure come from Nunn & Wantchekon (2011). The dataset covers the group-level number of slave trade exports in the transatlantic and Indian Ocean slave trades, but not the trans-Saharan and Red Sea slave trades. The scope of the following analysis is thus limited to the former two slave trades. The key treatment variable, $Slave_i^{\text{pc}} = \ln\left(0.01 + \frac{Slave\ Export_i}{Population_{i,1500}}\right)$, measures population-normalized slave raid 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, Figures 1(a)–(c)). As an initial robustness check, I also employ 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.



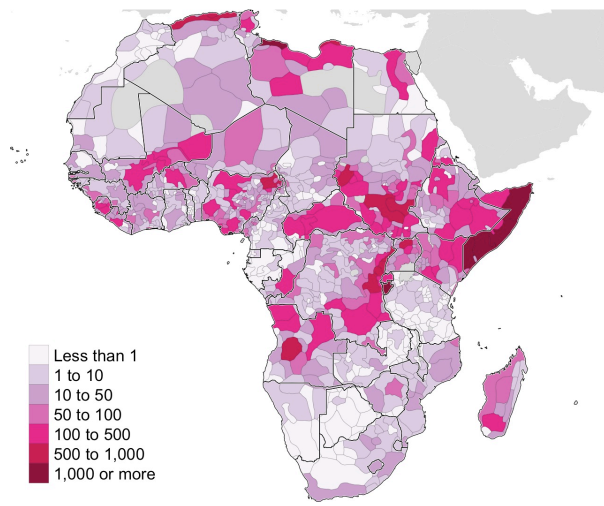
(a) Slave Trade Exports, 1700–1900



(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: (a) Group-level slave exports, 1700–1900 (Nunn & Wantchekon's (2011) data). (b) Population density per km^2 in 1500 (HYDE data). (c) Per capita slave exports, 1700–1900 (relative to group population in 1500). (d) Group-level exposure to battle incidents, 1997–2020 (ACLED data). 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 (gray).

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 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 sub-category of "violent events" category). Out of 59,121 battle events, 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)).

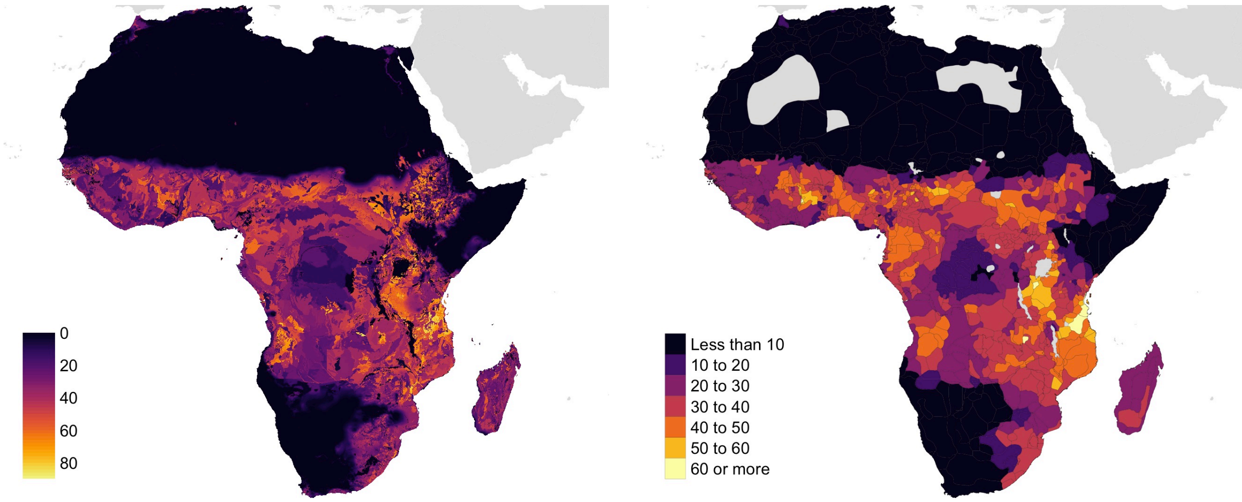
Second, to construct the group-level outcome variables, I first match Murdock ethnic groups to the ethnic groups in the Ethnic Power Relations (EPR) dataset (Cederman et al., 2010; Vogt et al., 2015) using the Linking Ethnic Data from Africa (LEDA) database and algorithm (Müller-Crepon et al., 2019). 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. The resultant correspondence table effectively connects 939 out of 1,282 country-group observations to the EPR groups.[3]

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. *Power Sharing* thus ranges from 0 (totally excluded) to 1 (totally included). *Rebel* and *Coup* are constructed from the EPR-compatible dataset of Roessler & Ohls (2018). *Rebel* and *Coup* take a value of 1 if members of an ethnic group launched a rebellion or a 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 slave raids at the ethnic group level. To construct the instrument, soil suitability for cassava cultivation, 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). 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). As in Figure 2, I measure group-level average suitability measures for cassava and overall agriculture based on the suitability estimates for the baseline 1961–1990 period. The regression models include logged suitability measures.

Broadly following previous studies (e.g., Nunn & Wantchekon, 2011), I measure three sets of group-level covariates to facilitate the analysis. To proxy precolonial political and

[3] I employ the linguistic distance-based matching algorithm implemented in R-package LEDA, with a linguistic distance (ranging from 0 to 1) threshold of 0.2 to generate the Murdock-EPR links. As reported in Appendix B, alternative threshold values produce qualitatively similar results.



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

Figure 2: Soil Suitability for Cassava Cultivation

(a) The GLUES soil suitability index for cassava and (b) group-level mean suitability. Lighter shades represent higher suitability.

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).[4] 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).[5]

The second set measures geographic attributes, including logged mean elevation and ruggedness (Shaver et al., 2019; USGS 1996), indicators for the presence of capital cities and water body, average temperature during the 1901–1910 period (Harris et al., 2020), logged mean malaria ecology index (Kiszewski et al., 2004), geographical areas in logged km^2 of total homelands, share of the areas of country-group settlement relative to total (non-partitioned) traditional homelands, and a dummy variable for partitioning by international borders.[6] I also include the distances in logged kilometers from homeland centroids to capital, closest borders, equator, and coastlines, and the GLUES index for overall agricultural suitability.

The third set includes three dummy variables capturing posttreatment factors that might have been affected by slavery while influencing postcolonial politics. The dummies are equal

[4]I follow Fenske (2014) to measure the Herfindahl index for ecological diversity using the shapefile-version of White's (1983) vegetation map provided by Fenske (2014).

[5]The dummies for the presence precolonial kingdoms and conflicts are taken from the dataset of Michalopoulos & Papaioannou (2016).

[6]Following Michalopoulos & Papaioannou (2016), I code the border partition dummy as 0 if more than 90% of a historical homeland falls into a single country.

to 1 if a settlement area contains colonial railways, European explorer routes, or missions to proxy European influence during the colonial period (Nunn & Wantchekon, 2011). To mitigate posttreatment bias, these additional covariates are included for a robustness check purpose. Table A.1 in the Appendix reports descriptive statistics of the variables.

3.2 Soil Suitability for Cassava Cultivation as an Instrument

Any investigation into long-run causal effects faces the challenges of omitted variable bias and measurement error. For example, ethnic groups less able to resist outside violence might have suffered more from slave raids 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 discourages naïve comparisons.

To address these concerns, I rely on an instrumental variable (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 at least two conditions: A valid instrument (1) must be strongly correlated with the group-level slavery exposure (instrument relevance) while (2) not affecting contemporary political outcomes through any path other than the slave trades (exclusion restriction).

Besides the rich group-level variation shown in Figure 2, there are three reasons to leverage cassava suitability as an instrument. First, unlike actual crop *yields*, soil *suitability* is mainly a function of the time-constant or slow-moving climatic, soil, and topographic conditions exogenous to human activities. This feature of soil suitability helps us alleviate potential 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 stressful environments, high energy yields, and less-input demanding nature (El-Sharkawy, 2004), the introduction of cassava improved land potential to sustain a larger population and slave supply as maize, another New World crop introduced to Africa by the Colombian Exchange (Cherniwchan & Moreno-Cruz, 2019; Crosby, 1972; Curtin, 1969).[7] The improved land potential could have raised the abilities of groups in cassava-suitable areas to engage in warfare and capture slaves while amplifying the incentives of slave raiders for slave capture due to increased local slave supply and food-

[7] As reported in Appendix C, the group-level maize-slavery association remains weaker compared to cassava and the country-level association reported in Cherniwchan & Moreno-Cruz (2019).

stuff 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).

Third, cassava suitability mitigates the concern for the exclusion restriction violations, and its timing of arrival permits a unique falsification test. A common strategy in the slavery legacy literature is to instrument slave raid exposure by the geodesic distance between group locations and coastlines (e.g., Nunn & Wantchekon, 2011). While the distance-based design is valid in other circumstances, the exclusion restriction is less likely to hold in the current context. For example, the established correlation between economic performance and conflict risks, combined with the low 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, or regional development and population size (e.g., Cederman et al., 2010; Fearon & Laitin, 2003; Francois et al., 2015). Turning to falsification, the current IV strategy implies that *before* cassava's arrival, which altered the role of soil suitability in shaping slave raids, we should not see a systematic association between cassava suitability and slavery exposure. A null first-stage regression with the pre-arrival slave exports as the outcome thus serves as a plausible falsification test for the current IV strategy.

3.3 Model Specification

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

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

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

where i indexes an ethnic group and c a host country. Y_{ic} is one of the outcome variables, $Slave_{ic}$ is logged population- or area-normalized slave trade exposure, $\widehat{Slave_{ic}}$ is the corresponding fitted values from the first stage, and $Cassava_{ic}$ is logged cassava suitability index. $\boldsymbol{X}_{ic}$ represents a vector of covariates, α_c and μ_c are country fixed effects, and $f_1(\boldsymbol{s}_{ic})$ and $f_2(\boldsymbol{s}_{ic})$, with $\boldsymbol{s}_{ic} = (\text{Longitude}_{ic}, \text{Latitude}_{ic})$, are cubic polynomials of settlement geo-coordinates. $\boldsymbol{M}_{ic}$ denotes a vector of synthetic covariates representing the Moran eigenvectors to absorb residual spatial autocorrelations in the first and second stages (spatial filtering, Griffith & Peres-Neto, 2006).[8] The country-group setup follows previous studies,

[8]This spatial filtering approach follows Rozenas & Zhukov (2019). Kelly (2019) questions several persistent effect findings for spatial autocorrelations. The location polynomials and the spatial filtering address this methodological concern. I rely on a distance-based spatial weight matrix with a $5°(\approx 550$ km) threshold.

and the inclusion of country fixed effects allows for exploiting within-country variation while accounting for the cross-country differences such as colonizer policies and national political institutions (e.g., Michalopoulos & Papaioannou, 2013, 124). Although unlikely to be driven by the group-level slavery exposure, these cross-country differences influence the baseline risks of domestic fighting and related outcomes.

The parameter of interest is τ_{IV}, which captures the local average treatment effect (LATE) of the group-level slavery exposure on the outcome variables. The expected sign of τ_{IV} for each outcome is summarized in Table 1 above. Following Angrist & Piscke (2008, 197–205), I rely on two-stage least square (2SLS) models throughout the analysis, rather than nonlinear models that require additional estimation assumptions. For a comparative purpose, I also report the corresponding uninstrumented ordinary least square (OLS) models.

4 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 raid-battle incident association with the location-level outcome. I then turn to the group-level outcomes to further investigate the underlying causal pathways, followed by several robustness checks.

4.1 Instrument Relevance

Table 2 reports the first-stage estimates for cassava suitability-slavery association, along with the coefficients on coast distance, an established predictor of slave raids highlighted in previous literature (e.g., Nunn & Wantchekon, 2011). Consistent with the IV strategy, cassava suitability is positively associated with group-level slave raid exposure, 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, respectively, 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. Columns 1 and 4 adjust for a restricted set of covariates, or the two covariates with relative imbalance, along with precolonial population density and settlement area.[9] Columns 2–3 and 5–6 further adjust for the remaining baseline covariates

[9] Figure A.1 in the Appendix displays partial correlations between the instrument and baseline covariates. The partial correlations remain negligible for most of the covariates. Exceptions out of the 22 covariates include overall agricultural suitability and grassland proportion, and to a lesser extent, precolonial conflict presence, ecological diversity index, and water body. Appendix A.2 examines potential model dependence.

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

	Panel A: All Observations					
	Slave^{pc} (population-normalized slave exports, 1700–1900)			Slave^{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***
	(0.032)	(0.035)	(0.036)	(0.044)	(0.046)	(0.047)
Coast Distance		−0.308***	−0.292***		−0.520***	−0.502***
		(0.077)	(0.078)		(0.107)	(0.107)
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.402	0.418	0.424	0.424	0.450	0.453
F-statistic (weak instrument)	25.811	30.509	30.312	20.228	28.178	27.7
Stock and Yogo's critical value	16.380	16.380	16.380	16.380	16.380	16.380
Residual Moran's I (z-score)	−1.210	−0.914	−1.199	−0.758	−0.710	−0.703
	Panel B: LEDA-Connected Observations					
	(1)	(2)	(3)	(4)	(5)	(6)
Cassava Suitability	0.199***	0.196***	0.199***	0.236***	0.236***	0.242***
	(0.041)	(0.044)	(0.043)	(0.055)	(0.058)	(0.057)
Coast Distance		−0.289***	−0.274***		−0.491***	−0.478***
		(0.086)	(0.087)		(0.115)	(0.119)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted covariates	✓	✓	✓	✓	✓	✓
Baseline covariates		✓	✓		✓	✓
Additional covariates			✓			✓
Observations	939	939	939	939	939	939
Adjusted R^2	0.415	0.442	0.447	0.429	0.469	0.470
F-statistic (weak instrument)	23.475	20.026	21.279	18.379	16.745	17.876
Stock and Yogo's critical value	16.380	16.380	16.380	16.380	16.380	16.380
Residual Moran's I (z-score)	−1.069	−1.020	−1.030	−0.963	−0.829	−0.975

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis. Moran eigenvectors in Panel B are obtained from the second-stage estimates with *Power Sharing* as the outcome.

and the additional controls 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 effectively address spatial autocorrelations. The following analysis always adjusts for all baseline covariates to keep the estimates conservative.

4.2 Results I: Battle Exposure

Table 3 reports the IV-2SLS estimates for the battle incident outcome (columns 2–3 and 5–6), along with the uninstrumented OLS results (columns 1 and 4). Despite the insignificant coefficient estimates in the OLS models, once instrumented, slave raid intensity shows

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

	Dependent variable: ln(1 + Battle)					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	0.0003	-0.965^{**}	-1.009^{**}			
	(0.032)	(0.393)	(0.396)			
Slave$^{\mathbf{Area}}$				0.018	-0.761^{**}	-0.803^{**}
				(0.027)	(0.312)	(0.321)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.546			0.545		
F-statistic (weak instrument)		30.509	30.312		28.178	27.7
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	-0.534	-0.469	-0.102	-0.063	-0.787	-0.212

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

statistically significant negative signs, and the coefficient estimates remain stable regardless of model specifications and slave raid measures, thereby providing initial support for the advanced argument. In addition to its persistence, the negative association is also sizable. As both of the treatment and outcome variables are log-transformed, the coefficients can readily be interpreted as elasticity. Substantively, even after centuries, the coefficient of -0.965 in column (2) translates into that a 1 percent increase in the slavery exposure is followed by a roughly 0.97 percent decrease in the 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, IV-2SLS, or both estimates. First, the uninstrumented OLS estimates might be suffering from omitted variable bias, in addition to possible attenuation bias induced by a classical form of measurement error, such that unobserved group-level propensity to engage in fighting is positively associated with the exposure to slave raids and modern battle events. For example, war-prone groups might have been heavily exposed to slave exports due to the increased prisoners of war as a result of fighting. Indeed, historical accounts highlight the role of local warfare and political disturbance in shaping slave exports (Curtin, 1969, 226). The probable result is a bias toward zero in the slavery-battle association, which would mask the underlying negative association between the historical treatment and the modern battles. Second, despite the instrument relevance, the IV-2SLS estimates might still be suffering from the exclusion restriction violations. The falsification tests reported below, however, fail to invalidate the proposed IV strategy and thereby support the interpretation of the IV-2SLS estimates as causal effects.

4.3 Results II: Coup-Civil War Trade-Off

Recall that group-level outcome variables allow for distinguishing the two group-based explanations as well as the geographic sorting pathway. If slavery exposure influences postcolonial politics by the reshuffling of the local inhabitants, the slave trade should be associated with the battle locations but not with the group-level outcomes. By contrast, if the slave trade affects contemporary politics by facilitating institutional change and empowering local chiefs, we should also observe a systematic association between slavery and group-level measures.

Table 4 reports the IV-2SLS and uninstrumented OLS estimates for the group-level outcomes, *Power Sharing*, *Rebel*, and *Coup*. The results lend further support to the advanced argument by revealing the associations consistent with the theoretical predictions of the proposed institutional change pathway. The IV-2SLS results in columns 2–3 and 5–6 indicate that groups with higher exposure to slave raids 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 slavery exposure is followed by a $\tau \times \ln(1.10)$, or a 0.02 increase in power sharing prevalence, a 1.9 percent point decrease in the rebel risk, and a 3.9 percent point increase in the coup risk, respectively, highlighting the persistent political legacy of the slave trade.

4.4 Robustness Checks and Additional Results

Appendix B addresses the remaining robustness concerns. Specifically, it replicates the analysis with (1) alternative slavery exposure measures, or the post-cassava arrival slave raid exposure relative to the pre-arrival intensity, (2) an alternative battle event measure, and (3) alternative linguistic distance thresholds for the LEDA algorithm, with the results remaining qualitatively unchanged. The analysis also reveals (4) a positive association between slavery exposure and the precolonial group-level absolutist political authority, or an intermediate force in the presented argument, lending further confidence in the causal pathway.

5 Falsification Tests and Instrument Validity

Thus far, I interpret the IV estimates as the causal effects of the slavery exposure on postcolonial politics. A remaining concern for the causality claims is the potential exclusion restriction violations. This section summarizes the results of multifaceted falsification tests to address this identification concern while relegating the details to Appendix C.

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

	Panel A. Dependent variable: Power Sharing					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
Slave$^{\mathbf{pc}}$	−0.004	0.196**	0.190**			
	(0.004)	(0.078)	(0.077)			
Slave$^{\mathbf{Area}}$				−0.006	0.163**	0.156**
				(0.004)	(0.069)	(0.066)
Observations	939	939	939	939	939	939
Adjusted R^2	0.731			0.731		
F-statistic (weak instrument)		20.026	21.279		16.745	17.876
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.362	−1.502	−1.251	−0.398	−1.594	−1.107
	Panel B. Dependent variable: Rebel					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
Slave$^{\mathbf{pc}}$	0.007	−0.199***	−0.198***			
	(0.005)	(0.076)	(0.075)			
Slave$^{\mathbf{Area}}$				0.009*	−0.165**	−0.163**
				(0.005)	(0.065)	(0.064)
Observations	939	939	939	939	939	939
Adjusted R^2	0.825			0.826		
F-statistic (weak instrument)		20.558	21.553		16.948	17.498
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.977	−0.285	−1.244	−1.018	−0.461	−1.112
	Panel C. Dependent variable: Coup					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
Slave$^{\mathbf{pc}}$	0.008	0.408***	0.401***			
	(0.007)	(0.136)	(0.136)			
Slave$^{\mathbf{Area}}$				0.005	0.339***	0.329***
				(0.007)	(0.121)	(0.117)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	939	939	939	939	939	939
Adjusted R^2	0.720			0.720		
F-statistic (weak instrument)		20.649	21.805		16.727	18.170
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	0.210	−0.750	0.446	0.112	0.172	0.647

Notes: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$. Robust standard errors in parenthesis.

5.1 Placebo Treatment: Slave Raids Before Cassava's Arrival

First, the current IV strategy implies that we should not observe a systematic first-stage cassava-slavery association *before* cassava's arrival, or a characteristic determined before the instrument (cassava suitability) retained significance. Any systematic cassava-slavery association in the *pre*-arrival period invalidates the IV strategy through unadjusted instrument-outcome confounders and mediators (Garabedian et al., 2014). For this exercise, I first construct per capita and per area slavery measures using the slave export records for the 1400–1599 period (Nunn & Wantchekon, 2011), and then re-estimate the first-stage regressions with the pre-arrival slave raid measures.[10]

Table 5 reports the false first-stage estimates. Consistent with the current IV strategy, the cassava-slavery association remains statistically indistinguishable from zero once the slavery variables are replaced by the pre-arrival measures. By contrast, the coefficients on coast distance remain negative and statistically significant in the pre-arrival period estimates. The consistently negative association further suggests that the exercise is not just picking up the demographically less severe nature of the slave raids during the pre-arrival period.

5.2 Placebo Instrument: Soil Suitability for Non-Cassava Crops

The second exercise is motivated by Lowes & Montero (2020) and Miguel et al. (2004) and leverages soil suitability for non-cassava crops as placebo instruments. If the proposed instrument is valid, we have little reason to see a systematic first-stage association once the cassava suitability measure is replaced with placebo instruments (non-cassava crop suitability).

Figures C.1 and C.2 in the Appendix subsequently re-estimate the first-stage regressions with the instrument replaced with each of the 15 non-cassava crops available in the GLUES data The false first-stage estimates reveal weak predictive power of non-cassava crops with F-statistics never passing the critical value of 16.38, suggesting that the cassava-slavery association is unlikely to be a statistical artifact nor a product of omitted heterogeneity.

5.3 Further Falsification Tests

Appendix C reports three additional falsification tests. The first exercise leverages riots and protests as a placebo outcome. Such demonstration events are largely self-organized from the bottom and unrelated with group-level institutions linking the slavery legacies and postcolonial politics. The second test examines reduced-form associations. Not to invite

[10] I drop the seventeenth-century records of slave exports as the slavery exposure during this period is likely to be partly (un)affected by soil suitability for cassava. Table C.1 in the Appendix re-estimates the analysis without the dummies for historical kingdom and conflict prevalence (1400–1700).

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

	$\text{Slave}^{\text{pc}}_{1400-1599}$ (population-normalized slave exports, 1400–1599)		$\text{Slave}^{\text{Area}}_{1400-1599}$ (area-normalized slave exports, 1400–1599)	
	(1)	(2)	(3)	(4)
Cassava Suitability	0.017	0.017	0.026	0.027
	(0.014)	(0.017)	(0.017)	(0.021)
Coast Distance	-0.137^{***}	-0.104^{**}	-0.171^{***}	-0.129^{***}
	(0.039)	(0.042)	(0.042)	(0.038)
Country FE	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓
LEDA-connected sample		✓		✓
Observations	1,282	939	1,282	939
Adjusted R^2	0.404	0.416	0.435	0.456
F-statistic (weak instrument)	1.544	0.962	2.381	1.678
Stock and Yogo's critical value	16.380	16.380	16.380	16.380
Residual Moran's I (z-score)	−0.222	−0.952	0.028	−0.618

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

concerns for the exclusion restriction violations, the instrument should remain unassociated with the correlates of domestic fighting and power sharing, or contemporary nightlight intensity (as a proxy of regional development) and population density. The last exercise utilizes the groups in today's North and South African states with little exposure to slave raids as a negative control (placebo) sample. Because the slavery pathway is absent in the subsample, we should not see any systematic reduced-form association between cassava suitability and the outcomes. These additional tests lend further support for the current IV strategy.

6 Conclusion

Historical institutions matter in shaping modern political outcomes, including domestic power sharing and armed conflicts. This article has exploited African slave trades to unpack the persistent effects of the external shock to local societies on postcolonial politics in Africa. It argues that the historically rare shock to local communities by slavery had a counterintuitive pacifying effect by strengthening ethnic institutions and thereby easing the general coup-civil war trade-off and the underlying commitment problems. The empirical analysis reveals that ethnic groups with greater exposure to slave raids 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 against the central government, while (4) more likely to stage coups in postcolonial states.

These findings carry implications for a broader body of literature and offer avenues for future research. First, the persistent effects of the slave trade on postcolonial politics in Africa highlight the importance of historical confounders for investigations into the power sharing-conflict link. Although limited to the context of postcolonial Africa, the empirical results suggest that the historical exposure to slave raids systematically alters the chances of power sharing, civil wars, and coups d'état. The reduced-form results in the Appendix also highlight the significant associations between soil agricultural suitability and the aspects of the coup-civil war trade-off. A related implication follows that when omitted, group-level slavery exposure and agricultural suitability would invite an omitted variable bias in investigations into the power sharing-conflict link. The literature pays careful attention to the selection process and strategic logics governing the coup-civil war trade-off, both theoretically and empirically (e.g., Paine, 2020; Roessler, 2011, 2016; Wucherpfennig et al., 2016). Yet the historically deeper roots and confounders also warrant further attention.

Second, the findings underline how the interactions between historical events and institutional change shape subsequent political outcomes. As in the earlier work of Whatley (2014), the slavery-induced shock facilitated institutional change and empowered local chiefs. The externally-facilitated 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, the findings illuminate how external shocks to local societies foster institutional transformations, and the induced institutional change influences subsequent outcomes, which in turn generates persistent legacies of past 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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Overview

This Online Appendix presents a series of robustness checks and additional results for the empirical analysis reported in "On the Persistent Effects of the Slave Trade on Postcolonial Politics in Africa." Section A presents summary statistics of the outcome, treatment, instrument variables along with covariates. It also displays partial correlation plots to examine covariate balance and examines possible model dependence. Section B presents the details of the robustness checks briefly reported in the main text and additional regression results examining the cassava suitability-absolutist chiefdom and slavery-absolutist chiefdom associations. Section C reports the results of the falsification tests.

The empirical analysis was conducted in R 4.0.2. I primarily rely on cshapes, sf, and sp packages for geoprocessing procedures (Bivand et al., 2013; Pebesma & Bivand, 2005; Weidmann et al., 2010). The Murdock-EPR group matching procedure and the regression analysis utilize LEDA, lfe, and spatialreg packages for R (Bivand & Piras, 2015; Gaure, 2013; Müller-Crepon et al., 2019).

A Descriptive Statistics, 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.

A.1 Partial Correlation

Figure A.1 displays partial correlations between the cassava suitability index and the baseline covariates to examine covariate balance. The instrument on the horizontal axis is residualized by the baseline covariates excepting for the covariate on the vertical axis, location polynomial, and country fixed effects, while the covariate on the vertical axis is residualized by the other baseline covariates, location polynomial, and country fixed effects. Solid segments represent linear regression fits, and the text reports the estimated slope.

Specifically, for each of the baseline covariates, I estimate the following linear regressions with the instrument (cassava suitability) and covariate k as the dependent variables:

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

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

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: Baseline Covariates				
Overall Suitability	1,282	3.497	1.033	3.820	0.451
Grassland in 1500	1,282	1.314	0.647	1.366	0.859
Population Density in 1500	1,282	0.578	1.539	0.659	1.789
Area	1,282	9.801	1.321	9.716	1.904
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
Area Share	1,282	0.642	0.390	0.837	0.766
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
Avg. Temperature, 1901–1910	1,282	24.220	3.257	24.918	4.865
Malaria Ecology Index	1,282	2.320	1.053	2.731	1.247
Water Body	1,282	0.596	0.491	1.000	1.000
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
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
	Panel D: Additional Covariates				
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. The two covariates included in the restricted covariate set, along with area and population density in 1500 are noted in **bold texts**.

where all right-hand-side variables and coefficients are defined analogously to the two-stage specification in the main text. All variables are standardized for a comparative purpose. Since the cassava-covariate association induced by other covariates has been partialled out, the remaining covariance in each panel is attributable to the conditional correlation between the cassava suitability index on the horizontal axis and the covariate on the vertical axis.

Reassuringly, the broadly flat regression fits indicate fair balance for most of the covari-

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 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)
Baseline Covariates	
Overall Agricultural Suitability	GLUES data (Zabel et al., 2014)
Area	Murdock (1959), 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 (1983), shapefile from Fenske (2014)
Malaria Ecology Index	Kiszewski et al. (2004)
Water Body	Natural Earth (https://www.naturalearthdata.com/10m-physical-vectors/)
Precolonial Kingdom	Besley & Persson (2011), taken from the dataset of Michalopoulos & Papaioannou (2016)
Precolonial Conflict	Besley & Persson (2011), taken from the dataset of Michalopoulos & Papaioannou (2016)
Cities in 1500	Reba et al. (2016)
Area Share	Murdock (1959), digitized in shapefile-format by Nunn (2008), partitioned by the international borders as of 2000, using 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	cshapes data (Weidmann et al., 2010)
Additional Covariates	
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 slave exports in 1700–1900 relative to the 1400–1599 exports (Nunn & Wantchekon, 2011), group population are computed using the HYDE data (Kees et al., 2011)
$\Delta\text{Slave}^{\text{Area}}$	Per area slave exports in 1700–1900 relative to the 1400–1599 exports (Nunn & Wantchekon, 2011), settlement areas are computed based on Murdock (1959), digitized by Nunn (2008)
xSub Battle	xSub data (Zhukov et al., 2019)
Nightlight in 2010	DMSP-OLS (NSDC 2014)
Population Density in 2010	WorldPop (2016)
Riots and Protests	ACLED data (Raleigh et al., 2010)
Non-Cassava Crop Suitability	GLUES data (Zabel et al., 2014)

Notes: All variables are constructed by geoprocessing procedures based on the listed sources.

ates. Exceptions include overall agricultural suitability and grassland proportion (panels (a) and (b)) out of the 22 baseline covariates, and to a lesser extent, ecological diversity index and precolonial conflict presence ((c) and (e)). The absence of systematic associations in the remaining panels suggests that, as far as other covariates are adjusted for, these baseline covariates remain incapable of inducing confounding bias, while including these covariates in the regression model increases the accuracy of the model if they are associated with the outcomes. To prevent the covariate imbalance from plaguing the analysis and keep the estimates, the empirical analysis in the main text always adjust for the baseline covariates.

Figure A.2 re-estimates and displays the 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, and the estimation models in Table 4 in the main text always control for the covariate.

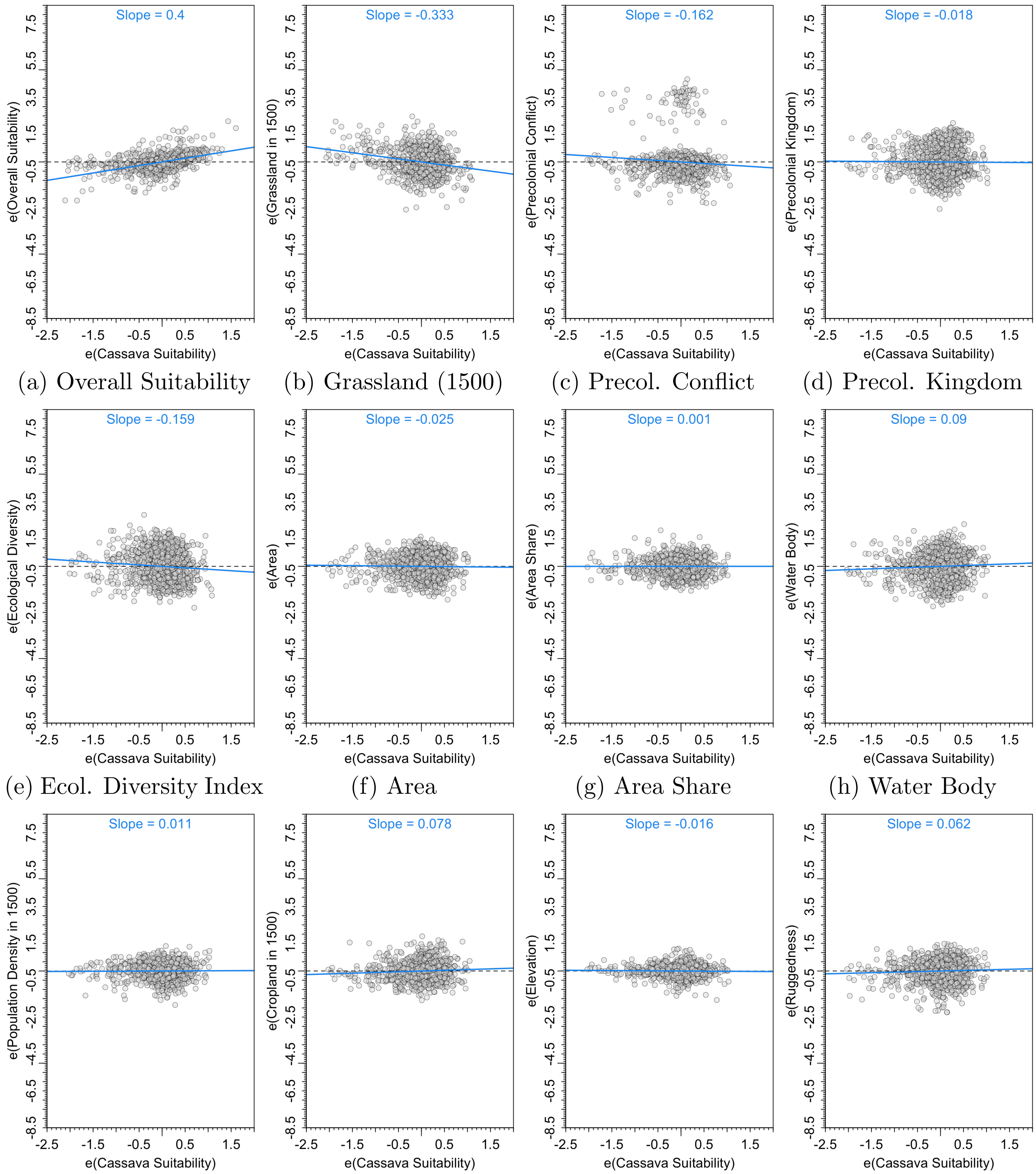
A.2 Model Dependence

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 $2^{18} = 262,144$ model specifications with different combinations of 18 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. Recall that the models in the main text always adjust for these right-hand-side variables.

Figure A.3 shows the empirical distribution of the coefficient estimate obtained from the $262,144$ 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 and Additional Results

The first three subsections of this section present the details of the robustness checks reported in the main text. The last subsection examines the association between slave raid exposure and precolonial absolutist political structure. The additional analysis further validates the institutional change pathway linking slavery exposure and postcolonial politics.



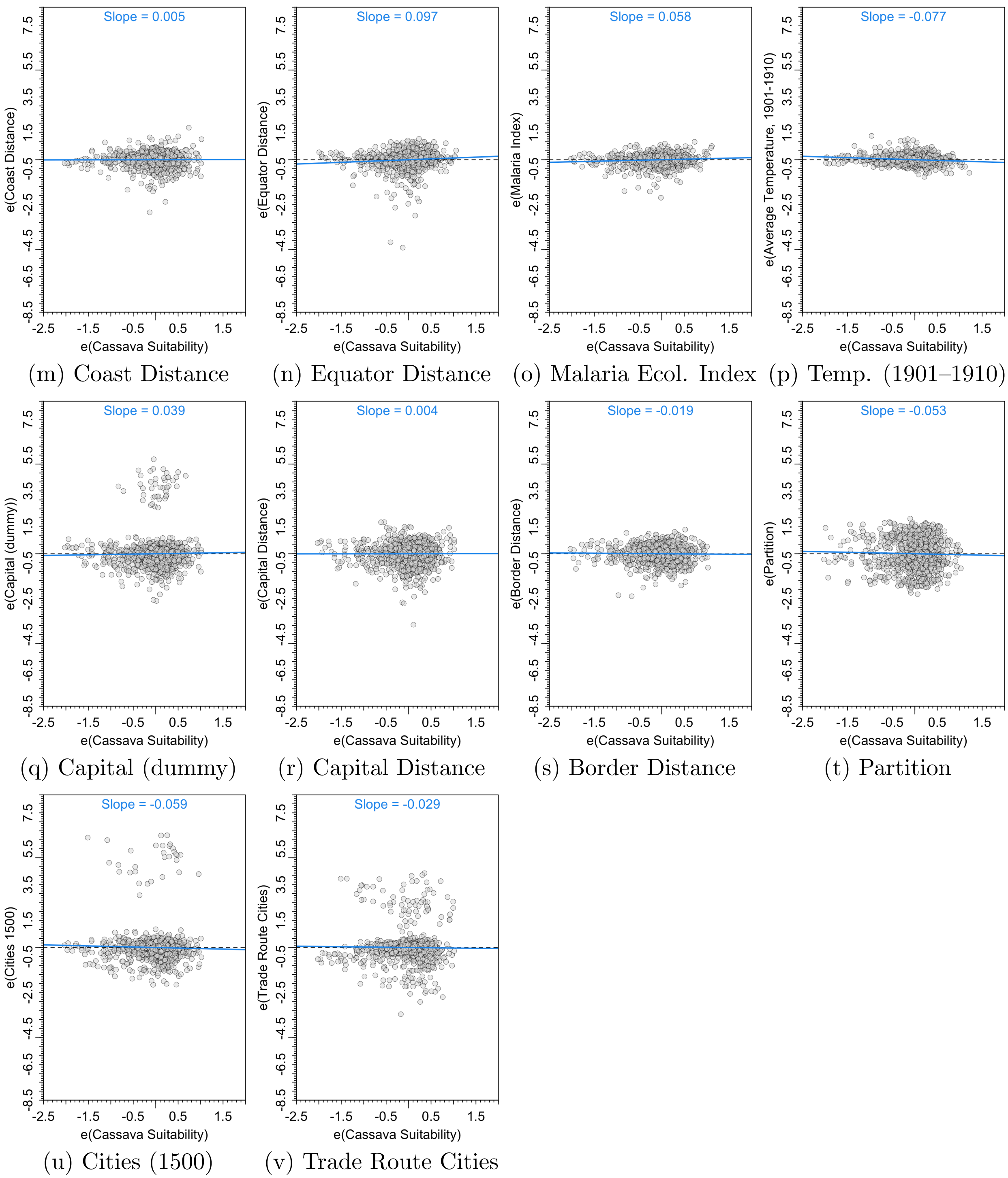
(a) Overall Suitability (b) Grassland (1500) (c) Precol. Conflict (d) Precol. Kingdom

(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 ($N_{\text{obs}} = 1,282$)

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 residualized by the baseline covariates excepting for the vertical axis variable, location polynomial, and country fixed effects, while the covariate on the vertical axis is residualized 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 reports the corresponding regression slope estimates.

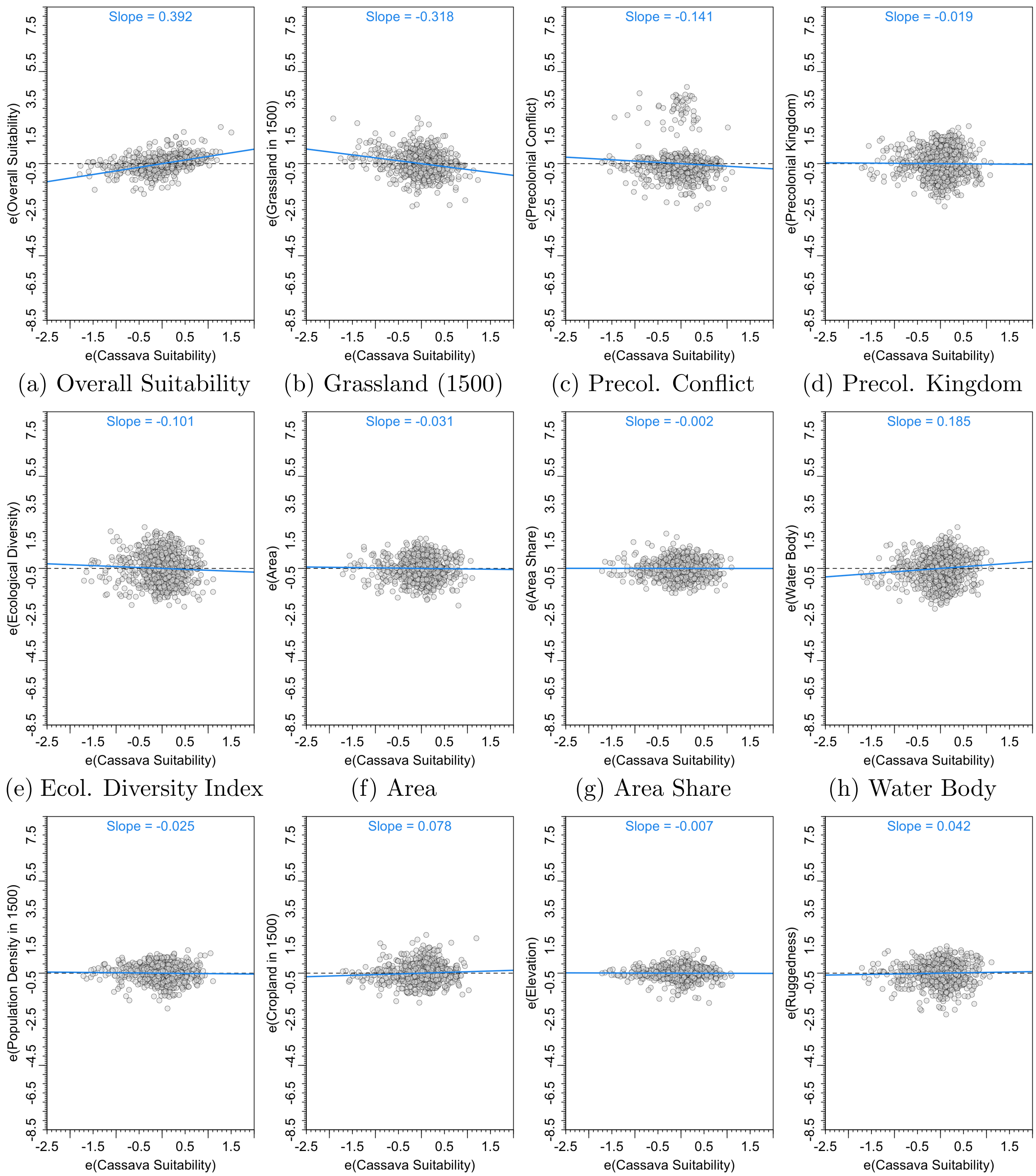


(m) Coast Distance (n) Equator Distance (o) Malaria Ecol. Index (p) Temp. (1901–1910)

(q) Capital (dummy) (r) Capital Distance (s) Border Distance (t) Partition

(u) Cities (1500) (v) Trade Route Cities

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



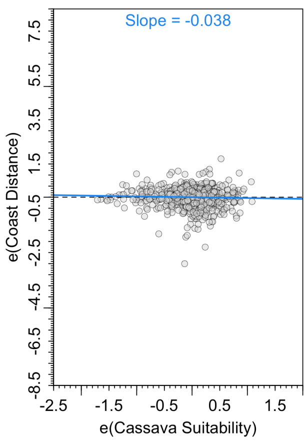
(a) Overall Suitability (b) Grassland (1500) (c) Precol. Conflict (d) Precol. Kingdom

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

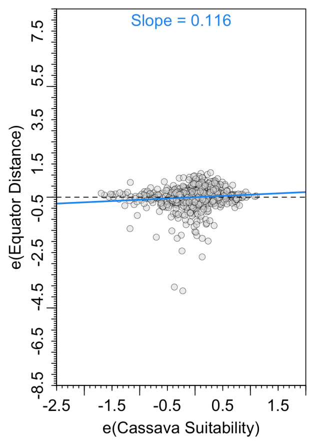
(i) Pop. Density (1500) (j) Cropland (1500) (k) Elevation (l) Ruggedness

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

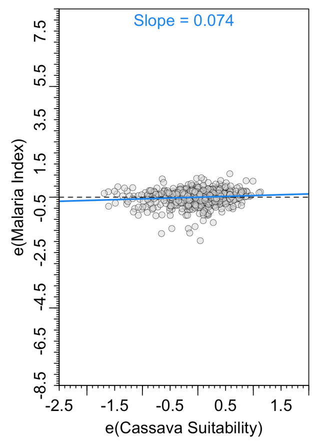
Notes: See notes in Figure A.1.



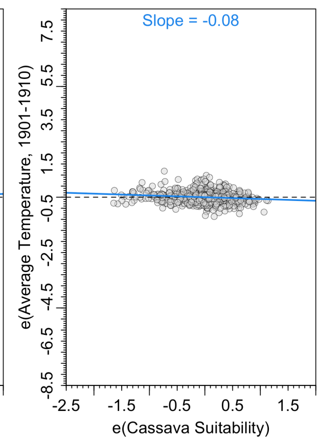
(m) Coast Distance



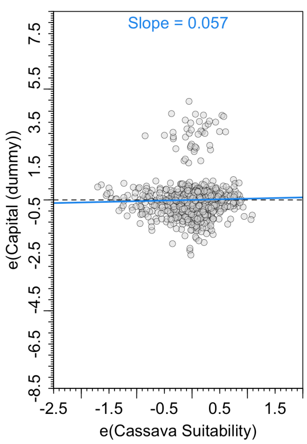
(n) Equator Distance



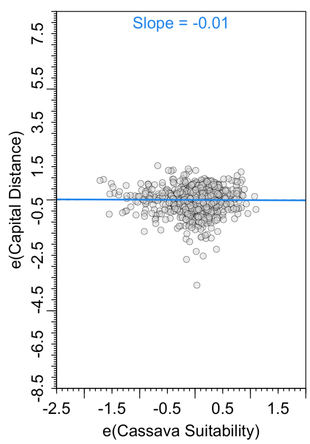
(o) Malaria Ecol. Index



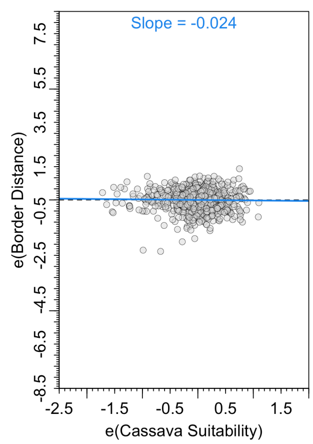
(p) Temp. (1901–1910)



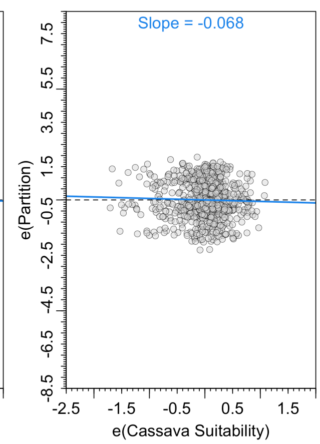
(q) Capital (dummy)



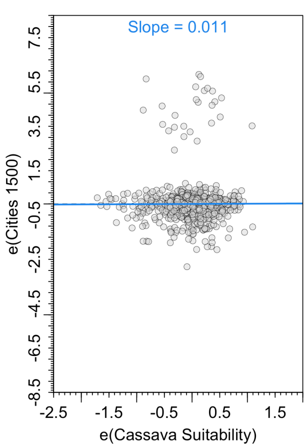
(r) Capital Distance



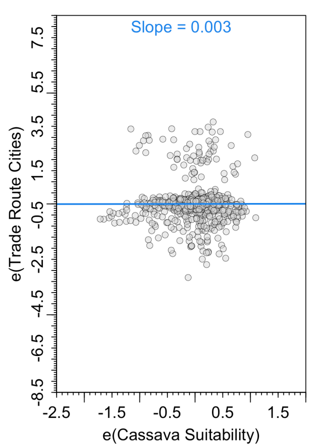
(s) Border Distance



(t) Partition

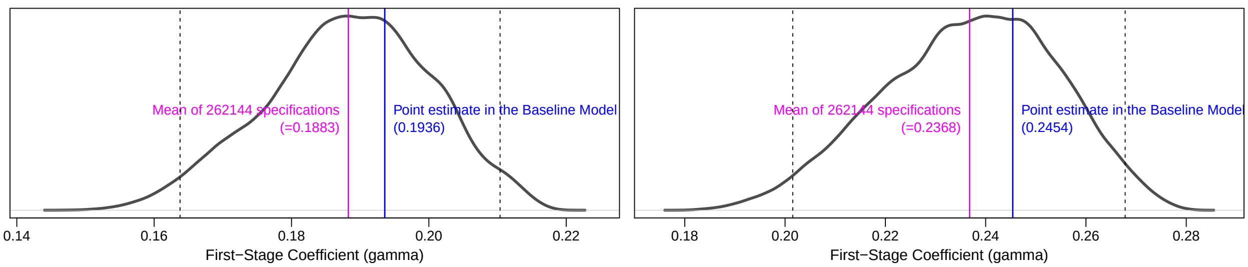


(u) Cities (1500)



(v) Trade Route Cities

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



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

Figure A.3: Empirical Distributions of the First-Stage Cassava-Slavery Association

Notes: Empirical distributions of the first-stage coefficients on cassava suitability across $2^{18} = 262,144$ 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.

B.1 Alternative Slave Raid Measure

A possible alternative treatment index measures the *difference* between the slave raid intensity in the pre-cassava arrival period and the post-arrival period rather than the post-arrival exposure in the baseline specification. The current IV strategy implies that ethnic groups with higher cassava soil suitability should have experienced increased slave raid exposure after cassava's arrival in Africa (1700–1900) relative to the pre-arrival slavery exposure (1400–1599). To empirically investigate this observable implication, I employ alternative 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 re-estimate 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 slave raid measures ($\Delta Slave_i^{\text{Area}}$). While the relatively weaker first-stage cassava-slavery associations caution against the interpretation of the second-stage estimates as consistent estimates, the second-stage coefficient signs remain unchanged for all outcome variables and provide additional support for the main findings.

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 xSub database to construct an alterna-

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

	Dependent variable: ln(1 + Battle)					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
Δ**Slave**$^{\mathbf{pc}}$	0.091	-2.958^{**}	-3.231^{**}			
	(0.079)	(1.287)	(1.336)			
Δ**Slave**$^{\mathbf{Area}}$				0.088	-3.469^{**}	-3.702^{**}
				(0.107)	(1.600)	(1.654)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.546			0.544		
F-statistic (weak instrument)		23.068	20.596		14.901	13.665
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	-0.678	-0.108	-0.102	-0.115	-0.007	-0.376

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

tive battle incident measure (Zhukov et al., 2019). The xSub database hosts different event datasets with different spatial and temporal coverages, including the ACLED data and the UCDP Georeferenced Event Data (GED, Sundberg & Melander, 2013). It 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). I restrict the temporal scope of the analysis 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 us 79,985 records of battle incidents in the study region during the 1997–2019 period. I then aggregate the xSub battle incidents at the country-group level following the same geoprocessing procedure in the main text to construct the alternative battle exposure measure, *xSub Battle*.[1] Figure B.1 depicts the distribution of the xSub battle incidents.

Table B.3 replicates the OLS and IV-2SLS regressions of Table 3 in the main text with ln(1 + *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.

[1] I extract "DYAD_A_B" (Government-Opposition interaction) entries to construct *xSub Battle* variable. See Zhukov et al. (2019) for the event coding in the xSub database.

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

	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
	Panel A. Dependent variable: Power Sharing					
ΔSlave^{pc}	−0.017	0.628**	0.596**			
	(0.012)	(0.265)	(0.255)			
ΔSlave^{Area}				−0.038***	0.685**	0.659**
				(0.015)	(0.305)	(0.296)
Observations	939	939	939	939	939	939
Adjusted R^2	0.731			0.732		
F-statistic (weak instrument)		14.788	14.713		12.501	13.016
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.405	−1.668*	−1.379	−0.425	−0.441	−0.522
	Panel B. Dependent variable: Rebel					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
ΔSlave^{pc}	0.014	−0.635**	−0.620**			
	(0.012)	(0.251)	(0.252)			
ΔSlave^{Area}				0.009	−0.693**	−0.686**
				(0.025)	(0.286)	(0.284)
Observations	939	939	939	939	939	939
Adjusted R^2	0.824			0.822		
F-statistic (weak instrument)		15.479	14.505		12.342	12.926
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.638	−0.110	−0.514	−0.229	0.169	0.274
	Panel C. Dependent variable: Coup					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
ΔSlave^{pc}	0.025	1.303***	1.256***			
	(0.016)	(0.476)	(0.468)			
ΔSlave^{Area}				0.025	1.422**	1.390***
				(0.030)	(0.559)	(0.533)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	939	939	939	939	939	939
Adjusted R^2	0.718			0.719		
F-statistic (weak instrument)		15.419	14.879		12.869	13.116
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	0.866	0.407	0.406	0.861	0.758	0.785

Notes: $^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

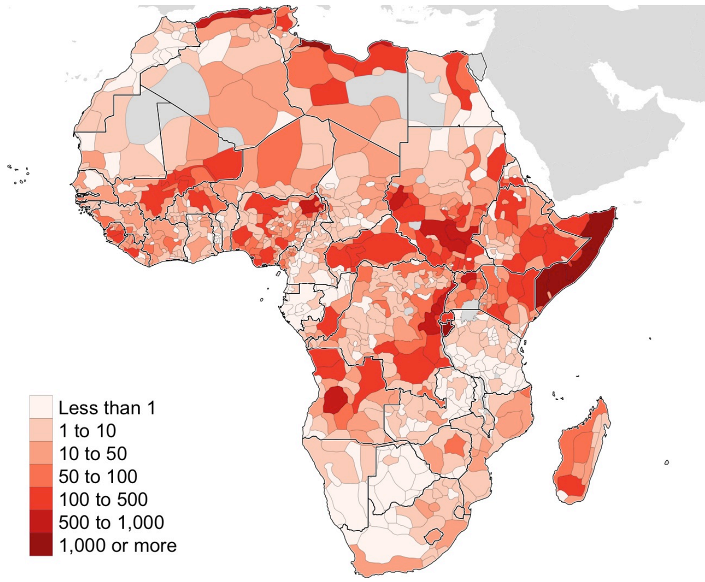


Figure B.1: xSub Battle Events, 1997–2019

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 (gray).

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

	Dependent variable: $\ln(1+\text{xSub Battle})$					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	0.028	-0.929^{**}	-1.038^{***}			
	(0.036)	(0.390)	(0.393)			
Slave$^{\mathbf{Area}}$				0.048	-0.733^{**}	-0.826^{**}
				(0.030)	(0.317)	(0.325)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.552			0.552		
F-statistic (weak instrument)		31.274	30.537		27.420	27.538
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−1.331	−1.589	−1.581	−1.263	−1.481	-1.820^{*}

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

B.3 Alternative Murdock-EPR Group-Matching Threshold

The main analysis connects 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) database (Müller-Crepon et al., 2019). The analysis employs LEDA's linguistic distance algorithm, with the distance (ranging from 0 to 1) threshold of 0.2. Yet 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 re-estimates 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 matching. The results remain qualitatively unchanged regardless of the threshold values for the group-matching procedure and the resultant (increased) sample size.

B.4 Slave Raid Exposure and Absolutist Political Authority

An implicit and untested assumption in the advanced mechanism in the main text is the positive association between slavery exposure and the institutional change in the affected communities toward absolutist political authority. Although this association is reported in Whatley (2014) for the ethnic groups in West Africa, it warrants a focused examination given the expanded sample in the current analysis. Recall that the advanced institutional change pathway assumes the positive intermediate association between slavery exposure and absolutist political structure as a mediating force, such that "slave raids $\rightarrow$ absolutist political structure $\rightarrow$ postcolonial politics." Although the slavery-absolutism association is reported in Whatley (2014) for the ethnic groups in West Africa, it warrants a focused examination given the current IV design and the expanded sample in the current analysis.

The outcome variable for the additional analysis is the absolutism index of Whatley (2014) based on the dataset of Murdock (1967) *Ethnographic Atlas*. Specifically, the absolutism indicator variable, *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) in Murdock (1967), and 0 otherwise. I rely on the digitized and expanded version of the *Ethnographic Atlas* of Giuliano & Nunn (2018) to construct this variable. The specification broadly follows the baseline two-stage specification, with the spatial polynomial term is specified as a linear polynomial for the reduced sample due to additional missing values in the absolutist index. As the analysis focuses on the colonial periods, the unit of analysis is Murdock ethnic group (not split or nested by host countries), and country fixed effect is similarly replaced by region fixed effect. The covariates reflecting modern international borders and capitals are also excluded from the specifications.[2]

Table B.5 reports the cassava-slavery and the slavery-absolutism associations. The first-stage estimate in columns (1) and (4) underline the positive cassava-slavery association for the restricted sample, regardless of the slavery exposure measures. The first-stage F-statistics

[2]Excluded covariates are *Area Share*, *Border Distance*, *Capital* (dummy), *Capital Distance*, and *Partition*. The remaining baseline covariates in Table A.1 are included in the models as group-level covariates.

Table B.4: Slave Raid 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
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−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
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−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)
LEDA language distance threshold	Median	Median	Median	Median	Median	Median
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
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
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	0.232	−0.211	−0.194	0.156	−0.229	−0.276

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

Table B.5: Slave Raid Intensity and Absolutist Political Authority

	Dependent variable:					
	$Slave^{pc}$	Absolutism		$Slave^{Area}$	Absolutism	
	First stage	OLS	IV-2SLS	First stage	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
$Slave^{pc}$		0.003	0.170**			
		(0.018)	(0.086)			
$Slave^{Area}$					0.004	0.141*
					(0.014)	(0.072)
Cassava Suitability	0.598***			0.721***		
	(0.101)			(0.123)		
Observations	429	429	429	429	429	429
Adjusted R^2	0.353	0.105		0.405	0.106	
F-statistic (weak instrument)	35.185		35.185	34.114		34.114
Stock and Yogo's critical value	16.380		16.380	16.380		16.380
Residual Moran's I (z-score)	0.158	0.436	0.810	0.272	0.421	0.521
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Group-level covariates	✓	✓	✓	✓	✓	✓

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

pass the critical value of Stock & Yogo (2005) and reject the null hypothesis of weak instruments for both population-normalized and area-normalized slave export measures. The second-stage results in columns (3) and (6) also reveal a positive association between slavery exposure and absolutist political authority measured as the index of Whatley (2014). Consistent with the earlier findings of Whatley (2014), the positive second-stage association suggests greater exposure to slave raids was followed by absolutist political authority and empowered local chiefdoms. These additional results are consistent with the theoretical expectation and provide further support for the institutional change mechanism by highlighting the shorter-run political legacies of slave raids among the affected communities.

C Falsification Tests

This article employs a series of falsification tests, leveraging (1) the arbitrary timing of cassava's arrival in Africa and the slave exports during the corresponding periods (placebo treatment), (2) soil suitability measures for non-cassava crops (placebo instrument), (3) riot and protest events (placebo outcome), (4) reduced-form regressions, and (5) North and South African observations barely exposed to slave raids (placebo subsample), with the results of the first two tests briefly reported in the main text. Table C.1 reestimates the null first-stage regressions in Table 5 in the main text without the dummy variables for conflict prevalence

Table C.1: False First-Stage Estimates for Group-Level Slave Exports, 1400–1599, without Historical Conflict and Kingdom Dummies

	$\text{Slave}^{\text{pc}}_{1400\text{–}1599}$ (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	0.015	0.016	0.025	0.026
	(0.013)	(0.017)	(0.016)	(0.020)
Coast Distance	-0.137^{***}	-0.103^{**}	-0.171^{***}	-0.128^{***}
	(0.039)	(0.042)	(0.042)	(0.039)
Country FE	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓
Baseline covariates (w/o Historical Conflict and Historical Kingdom dummies)	✓	✓	✓	✓
LEDA-connected sample		✓		✓
Observations	1,282	939	1,282	939
Adjusted R^2	0.405	0.413	0.441	0.456
F-statistic (weak instrument)	1.309	0.808	2.341	1.627
Stock and Yogo's critical value	16.380	16.380	16.380	16.380
Residual Moran's I (z-score)	−0.129	−0.739	−0.100	−0.742

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

and kingdom presence (1400–1700) and yields qualitatively similar estimates. The remainder of this section reports the details of the second to fifth falsification tests.

C.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 in the main text exploits the land soil suitability for non-cassava crops as placebo instruments.[3] If cassava suitability, rather than omitted heterogeneity, influenced the group-level exposure to slave raids, we have little reason to see a systematic first-stage association for the placebo suitability measures for non-cassava crops. Discernible first-stage associations for the placebo instruments invalidate the proposed IV strategy by suggesting that the cassava-slavery association reflects unadjusted heterogeneity correlated with the soil suitability for cassava as well as non-cassava crops.

To empirically 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

[3] Lowes & Montero (2020) present a series of reduced-form regressions with placebo instrument, non-cassava crop suitability relative to millet suitability, instead of their main instrument, cassava suitability relative to millet suitability, as the key predictors. Miguel et al. (2004, 736) present a similar 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).

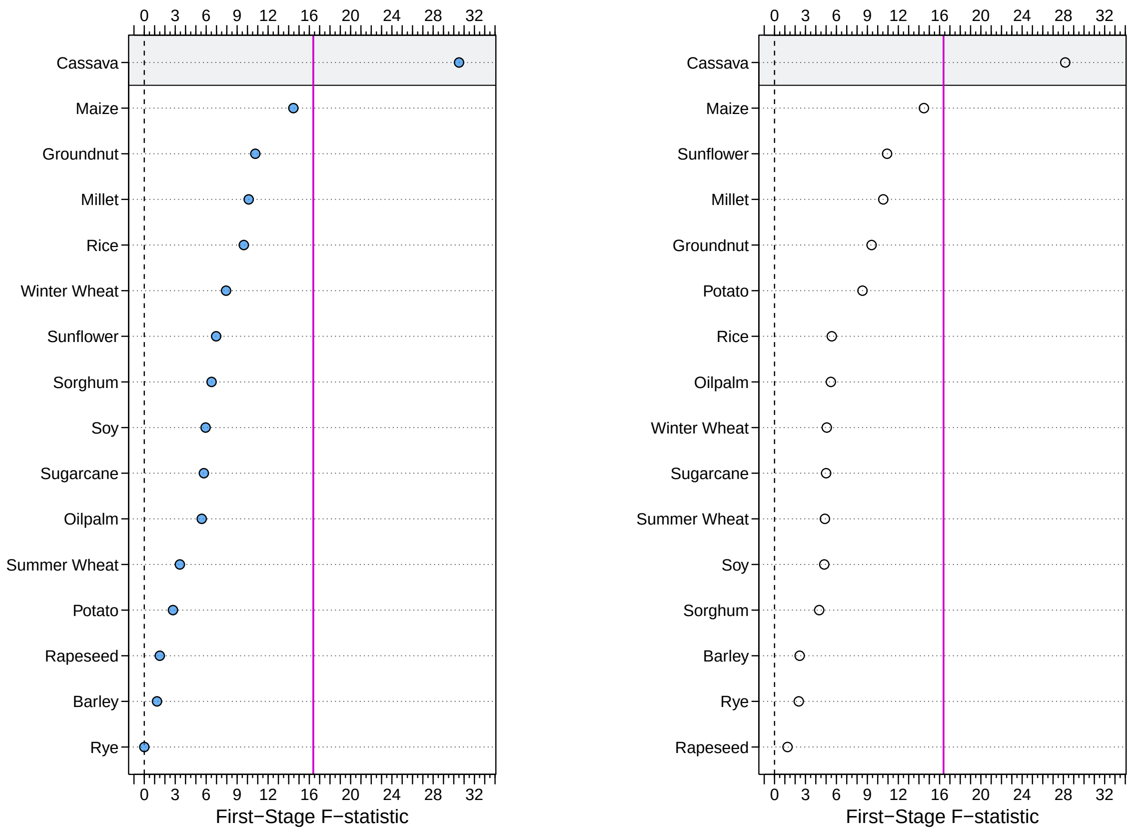
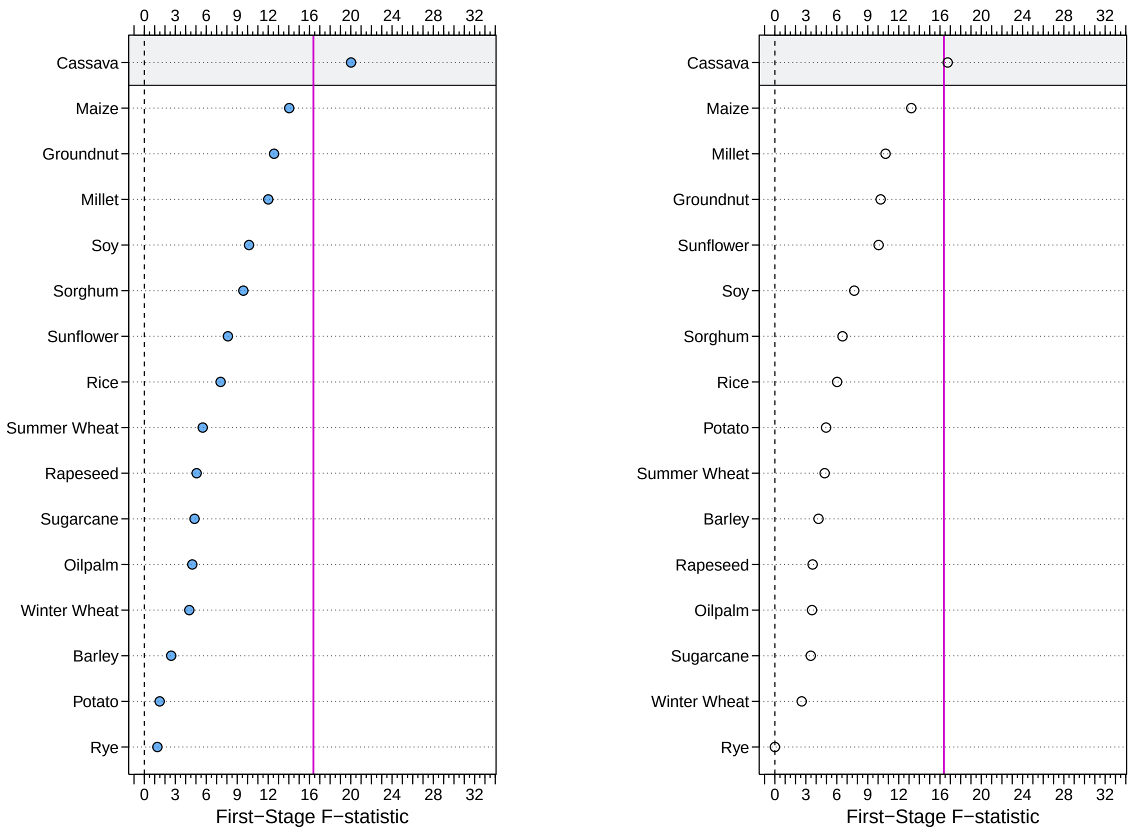


Figure C.1: First-Stage F-statistics for Cassava Suitability and Placebo Instruments

Notes: Soil suitability measures for cassava and non-cassava (placebo) crops and the corresponding first-stage F-statistics with (a) population-normalized and (b) area-normalized slave trade exposure measures as the first-stage outcomes. Each dot indicates the first-stage F-statistics for the suitability measure based on Huber-White robust standard errors for the crop on the vertical axis. Solid vertical segments indicate the critical value against weak instruments in Stock & Yogo (2005).

data and re-estimate the first-stage regression. Figures C.1 and C.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 C.1) and the LEDA-connected observations (Figure C.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 measure, and the solid vertical segments indicate Stock & Yogo's (2005) critical value of 16.38 for weak instruments.

The first-stage F-statistics for all non-cassava crops remain smaller than the critical value of 16.38 and fail to reject the null hypothesis that the instrument is weak. Somewhat consistent with country-level findings of Cherniwchan & Moreno-Cruz (2019), among the 15 non-cassava crops, the suitability for maize, another New World crop, is most strongly associated with slavery exposure, yet the association remains weaker at the ethnic group level



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

Figure C.2: First-Stage F-statistics for Cassava Suitability and Placebo Instruments, with the LEDA-Connected Observations

Notes: See notes in Figure C.1. The model specification follows Table 2 of the main text. The LEDA-connected sample includes 939 country-group observations with the LEDA linguistic distance threshold of 0.2, as in the baseline setting.

than in country level and fails to pass the critical value. Regardless of the slavery measures and (sub)samples, none of the suitability measures for non-cassava crops passes Stock & Yogo's critical value. These results also underline the distinctive role of soil suitability for cassava, rather than other Old World and New World crops, in shaping slave raids.

C.2 Placebo Outcome: Riot and Protest Events

The third exercise leverages placebo or negative control outcomes that can plausibly be assumed to share similar confounders while unaffected by the treatment, or "U-comparable" to battles (Lipsitch et al., 2010). While riots and protests are likely to share a similar set of confounders with battle events such as local economic performance and population size, the institutional account in this article leads to no clear effects of slavery on demonstration events. Riots and protests are largely self-organized from the bottom and therefore would

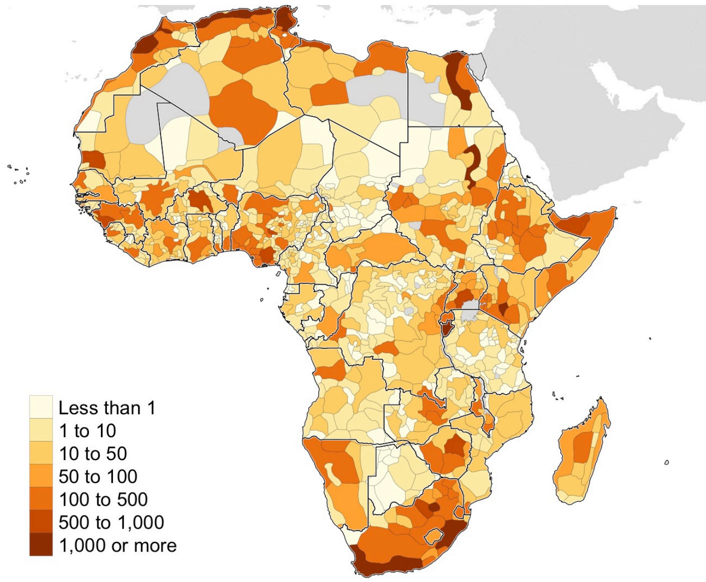


Figure C.3: ACLED Riot and Protest Events, 1997–2020

Notes: Darker shades represent greater prevalence of riot and protest 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 (gray).

likely remain unrelated with the group-level institutional constraints linking the slavery legacies and postcolonial politics.[4] The expected null-finding and outcome specificity provide further confidence in the main results (Lipsitch et al., 2010; Rosenbaum, 1989).

Specifically, relying on the same geoprocessing procedure in the main text, I aggregate the 69,448 riot and protest events during the 1997–2020 period at the country-group level, as shown in Figure C.3. Table C.2 replicates the analysis of Table 3 in the main text, with the logged count of riot and protest events as the outcome variable. The results reveal no discernible association between slavery exposure and current riots and protests regardless of the model specifications and lend additional credence for the main findings.

C.3 Direct Pathways: Correlates of the Coup-Civil War Trade-Off

Fourth, additional reduced-form regressions also inform us about violations of the exclusion restriction invited by potential 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 territories and (2) between current population density and battle activities can open up a direct instrument-outcome pathway. Not to invite the concerns for the exclusion restriction violations, the instrument should not be associated

[4]Although not in an IV setup, Depetris-Chauvin (2016) presents a similar approach exploiting the records of violent and non-violent events.

Table C.2: Slave Raid Intensity and Contemporary Riot and Protest Events, 1997–2020

	Dependent variable: ln(1 + Riots and Protests)					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	−0.017	0.067	−0.081			
	(0.030)	(0.316)	(0.315)			
Slave$^{\mathbf{Area}}$				−0.003	0.053	−0.065
				(0.026)	(0.249)	(0.252)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.636			0.636		
F-statistic (weak instrument)		29.404	29.792		27.729	27.331
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.390	−2.090**	−1.093	−0.356	−2.163**	−1.165

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

with the (unadjusted) correlates of civil war or power sharing. To empirically investigate the concern, I estimate the following reduced-form models:

$$Y_{ic}^{RF} = \kappa_c + \zeta Cassava_{ic} + \boldsymbol{X}_{ic}'\boldsymbol{\phi} + \boldsymbol{EV}_{ic}'\boldsymbol{\rho} + h(\boldsymbol{s}_{ic}) + v_{ic}, \tag{C.3}$$

where the right-hand-side variables and parameters are defined analogously to the two-stage specification in the main text, with $\boldsymbol{EV}$ denoting the Moran eigenvectors. Y_{ic}^{RF} is one of a broader set of outcomes, including *Nightlight*, or the logged nightlight intensity (as a proxy for local-level economic performance, NSDC 2014) and *Population Density*, or logged population density in 2010 (WorldPop, 2016) as well as the main outcomes.

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 trade-off, *Nightlight* and *Population Density* ($\zeta = 0$), which would otherwise open up the instrument-outcome links not through slave raids.

Table C.3 reports auxiliary reduced-form regression results. Reassuringly, the additional estimates reveal no systematic associations between land soil suitability for cassava cultivation and current nightlight intensity or population density, despite the strong reduced-form associations with the main outcome variables. Yielding no clear signs of the exclusion restriction violations, these results provide additional credibility to the main results and IV strategy while discouraging the use of alternative instrument candidates in the current analysis.

Table C.3: Reduced-Form Regressions for the Main and Additional Outcomes

	Main Outcomes				Potential Correlates	
	ln(1 + Battle)	Power Sharing	Rebel	Coup	Nightlight	Population Density
	(1)	(2)	(3)	(4)	(5)	(6)
Cassava Suitability (ζ)	−0.187***	0.038***	−0.039***	0.080***	−0.007	−0.066
	(0.069)	(0.012)	(0.011)	(0.021)	(0.061)	(0.049)
Overall Suitability	0.159*	−0.013	0.036**	−0.011	−0.069	0.441***
	(0.094)	(0.022)	(0.016)	(0.021)	(0.087)	(0.078)
Coast Distance	−0.034	0.041***	0.013	0.088***	−0.480***	−0.070
	(0.084)	(0.015)	(0.014)	(0.029)	(0.087)	(0.071)
Baseline covariates	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
LEDA-connected sample		✓	✓	✓		
Observations	1,282	939	939	939	1,282	1,282
Adjusted R^2	0.547	0.734	0.820	0.725	0.585	0.722
Residual Moran's I (z-score)	−0.038	−0.439	0.167	0.747	−0.180	−1.027

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

C.4 Placebo Sample: Groups in North and South African States

The last 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, groups in today's North and South African states have barely been exposed to slave raids and constitute a plausible negative control subsample. Because the slavery pathway ("cassava suitability → slave raid → 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).[5] 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 instrument 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 fall into today's North and South African states. Figure C.4 depicts the distribution of the negative control observations.

Table C.4 displays the subsample reduced-form estimates with the model specification of equation (C.3) above. Consistent with the current IV strategy, the reduced-form associations remain statistically indistinguishable from zero for most of the outcomes. The reduced-form results suggest the second-stage slavery-outcome association or the LATE estimate should also be indistinguishable from zero in the placebo sample.[6] An exception is the marginally

[5]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. (2016) also employ a similar approach in the context of the slavery legacies in the United States. See Altonji et al. (2005) and Angrist et al. (2010) for a related approach.

[6]Note that the IV estimator can be written as the ratio of the reduced-form instrument-outcome associ-

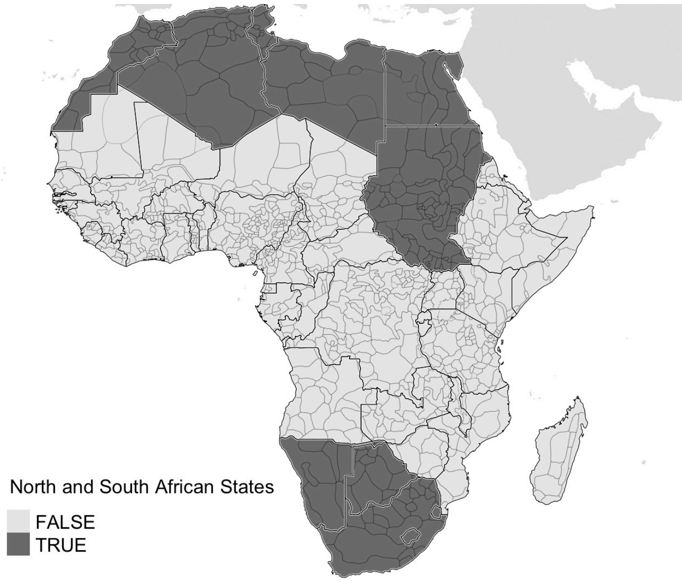


Figure C.4: Country-Group Observations (Not) Falling into North and South African States
Notes: Thin (solid) segments represent group boundaries (international borders as of 2000). Darker shades represent the settlement areas of the country-group observations in North and South African states.

Table C.4: Reduced-Form Regressions for the Negative Control Observations

	$\ln(1 + \text{Battle})$	Power Sharing	Rebel	Coup
	(1)	(2)	(3)	(4)
Cassava Suitability (ζ)	0.251*	0.024	0.009	−0.007
	(0.140)	(0.028)	(0.015)	(0.037)
Baseline covariates	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓
LEDA-connected sample		✓	✓	✓
Observations	248	197	197	197
Adjusted R^2	0.595	0.389	0.950	0.746
Residual Moran's I (z-score)	1.037	−2.383**	0.153	1.030

Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

significant ($t = \frac{0.251}{0.140} \approx 1.793$) association between cassava suitability and battle exposure in column (1). The reduced-form coefficient is signed positive and inconsistent with the main finding of the negative slavery-battle association.

ation relative to the first-stage instrument-treatment association (Angrist & Piscke, 2008, 120–121). A zero reduced-form association thus immediately indicates a zero second-stage association.

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