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

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October 20, 2020

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 strengthening ethnic institutions and leadership, thereby affecting the coup-civil war trade-off and the underlying commitment problems. The empirical analysis leverages the land-level agricultural suitability for cassava cultivation to exploit plausibly exogenous variation in the ethnic group-level exposure to slave raids. The main findings are four-fold: Ethnic groups with greater slave raid exposure 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. Falsification tests exploiting the timing of cassava's arrival in Africa and the regional variation in slavery exposure lend further credibility to the findings. (150 words)

Word Count

$\sim 10,000$ words (excluding the Online Appendix)

Keywords

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

*I am grateful to Kentaro Fukumoto, Atsushi Tago, Yuichiro Yoshida, and session participants of the 2020 Annual Meeting of the Japanese Political Science Association for valuable comments and suggestions. Financial support from the Japan Society for the Promotion of Science is gratefully acknowledged (Grant Numbers: 18KT0054 and 20K01464). All remaining errors are mine.

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How do historical events and institutions persistently affect subsequent political outcomes? Previous literature highlights three distinct pathways. First, historical institutions impose persistent constraints on individual behavior and interactions. Historical institutions and community ties shape today's economic performance and propensity to engage in armed conflicts by influencing institutional constraints, group 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 emerge and are 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 the direct channel of increased economic profits and the indirect pathway 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.[1] 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 empowered local political authority and chiefs (Whatley, 2014). Despite the absence of electoral incentives for chiefs with the inherent local authority, related studies also uncover the improved collective action capacity and effective leadership of chiefdoms in contemporary Africa (Acemoglu et al., 2014; Baldwin, 2016). The ironically strengthened ethnic institutions and improved collective action capacity enable the leadership to make credible threats and promises and to monitor and control group members, which in turn alleviates the war-causing commitment problems (Fearon, 1995; Powell, 2006).

Empirically, it leverages the soil agricultural suitability for cassava — a New World crop introduced to Africa by the Portuguese — 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 traveled to

[1] As explained below, this article relies on the group-level records of slave exports in the transatlantic and Indian Ocean slave trades developed by Nunn & Qian (2011) to construct the key treatment indicator.

the continent in the middle of the slave trade periods spanning from the fifteenth to the nineteenth centuries (Alpern, 1992; Crosby, 1972). The ecological features of cassava include its tolerance to stressful environments and high energy yields (El-Sharkawy, 2004), as well as the suitability for lengthy travels. The introduction of cassava increased land potential to sustain a larger population while magnifying slave traders' incentives and capabilities for slave raids in the affected regions, thereby exacerbating the exposure to slavery among the local inhabitants (cf. Cherniwchan & Moreno-Cruz, 2019).

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 reveal four empirical patterns consistent with the argument: Ethnic groups with *greater* slave raid exposure experience (1) *fewer* incidents of battles between armed forces within their historical homelands, while (2) *more* likely to be included in power-sharing schemes, (3) *less* likely to rebel against the central government from outside, and (4) *more* likely to stage coup attempts from inside in the postcolonial period. These results remain robust to an alternative slavery exposure measure as well as the adjustments for a vast set of covariates.

An important concern for the soil suitability-based IV design is potential violations of the exclusion restriction. In the current context, for example, one might expect agricultural suitability to affect current economic performance and population growth, thereby systematically shaping the local-level susceptibility to political violence. To address this identification concern, the main falsification test exploits the arbitrary timing of cassava's arrival in Africa. Unlike maize (corn), 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 during the seventeenth to nineteenth centuries (Alpern, 1992, 24–26; Crosby, 1972, 185–188). The delayed arrival left the earlier parts of the slave trades unaffected while opening up a pathway from soil suitability for cassava cultivation 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 *prior* to cassava's arrival. The falsification exercise confirms the expectations and lends credibility to the main findings.

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. This article is closely related to the literature 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 trades, this article sheds light on another deep historical determinant of contemporary politics. Second, this article contributes to the civil war and broader political science literature. Revealing the historical determinants, 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). Previous studies also examine the effectiveness of forms of political violence on subsequent political attitudes and behavior, both in short and long terms (e.g., Gilligan et al., 2014; Rozenas & Zhukov, 2019). The long-run political consequences of slavery shock provide another piece of insight into how large-scale violence and institutions interact with each other 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. This article fills the relatively scarce empirical insights by examining how slavery legacies influence power sharing and armed conflicts in postcolonial Africa.

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

Scholars have increasingly investigated the persistent effects of Africa's slave trades as well as the institutional determinants of postcolonial politics. This section reviews the insights from related literature, followed by the mechanisms and testable predictions that might link slave raid exposure and postcolonial politics in the next section.

1.1 Slave Trade Legacies

One of the most prominent aspects of Africa's slave trades is the sizable 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, The empirical analysis in 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 possible impact of slavery on postcolonial politics, which this article investigates.

1.2 Bargaining in the Shadow of History and Violence

Another body of literature that is closely related to this article focuses on the link between traditional ethnic institutions and postcolonial politics. While the direction of the causality remains disputed, recent 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.[2] Rather than a single "big man" dominating

[2] 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

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.

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 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

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

to be granted access to state power. Remaining less able to organize member behavior and make credible threats of force, groups with greater exposure to slave raids would 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.[3] Indeed, Acemoglu et al. (2014) and Bellows & Miguel (2006), respectively, uncover positive associations between less-competitive chiefdoms and greater local collective action capacities, and between exposure to wartime violence and local 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 the local 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. Rather, 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 centralized or state-like traditional institutions (Wig, 2016) and historical statehood experiences (Depetris-Chauvin, 2016), the ironically strengthened institutional constraints improve group collective action capacity and the leadership's ability to monitor and control group members.

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

These ironically improved group-level institutions and sustained 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 make credible threats and violently overthrow the regime. Political exclusion and inclusion constitute strategies of consolidating state power, and rulers would have little incentive to grant power-sharing spoils to outsider groups without the coercive capacity to challenge the recapture sovereign authority. Mobilizational potential combined with collective action capacity permits an ethnic 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 (Ito, 2020). 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 access. 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 mobilizational capacities. 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, which reduces the dictator's rents and enhances prospects for elite conflict" (3). To alleviate the side effect of improved coercive capacities and obtain power access, 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 effective and 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.

A related, inherent side-effect of the inclusion of outsider groups with higher coercive capacities is an increased risk of insider coups. For a power-sharing deal to be honored under the strategic situation, a ruler, as well as an outsider group, need to credibly commit to the bargain. Here, as Paine (2020) points out, 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 would also provide the outsider groups enhanced coup technologies that make coup attempts more feasible, which in turn result in higher risks of coup attempts (Paine, 2020; Roessler, 2011, 2016; Roessler & Ohls, 2018). Put another way, the increased chance and spoils of power sharing for groups with higher coercive capacities lead to increased risks of insider coups.

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 more heavily exposed to slave raids are associated with such improved institutional devices, we would expect that greater 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 *higher* risks of coup attempts, as higher power-sharing spoils improve coup technology.

2.2 Testable Predictions

The discussion leads to several testable predictions. First, both of the two mechanisms predict a negative association between slavery exposure and postcolonial conflicts.

Hypothesis 1 *Ethnic groups with greater exposure to the slave trade are less likely to experience armed conflicts in the postcolonial period.*

On the chances of power sharing, the two mechanisms generate contrasting predictions. The first mechanism postulates a *negative* association between slave raids and the likelihood of political inclusion, while the second perspective predicts a *positive* association.

Table 1: Testable Predictions

	Location level	Group level		
Mechanism	Battle	Outsider Rebellion	Power Sharing	Insider Coups
Institutional change	− (H1)	− (H1)	+ (H2a)	+ (H3a)
Distrust/weakened group ties	− (H1)	− (H1)	− (H2b)	− (H3b)
Geographic sorting	− (H1)	NA	NA	NA

Hypothesis 2a *Ethnic groups with greater exposure to the slave trade are more likely to be included in the power sharing schemes in the postcolonial period.*

Hypothesis 2b *Ethnic groups with greater exposure to the slave trade are less likely to be included in the power sharing schemes in the postcolonial period.*

The discussion above also suggests that a power-sharing deal entails increased coup risks. Here, again, the two accounts lead to mutually exclusive predictions.

Hypothesis 3a *Ethnic groups with greater exposure to the slave trade are more likely to engage in coup attempts in the postcolonial period.*

Hypothesis 3b *Ethnic groups with greater exposure to the slave trade are less likely to engage in coup attempts in the postcolonial period.*

2.3 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 emphasizes the role of migrations and the influx of new inhabitants. Group members may relocate across generations, and their historical homelands do not necessarily correspond to the current settlement areas. In particular, slave raids create land lots by forcibly removing local inhabitants and could also facilitate subsequent population influx into the cleared lots. The new inhabitants might have been less conflict-prone or less equipped with collective action capacity, which can alter the location-level, though not group-level, risks of postcolonial armed conflicts. This geographic sorting pathway also leads us to expect a negative association between slavery exposure and modern conflicts such that conflict events are less likely to take place 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 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 illustrated testable predictions.

3 Research Design

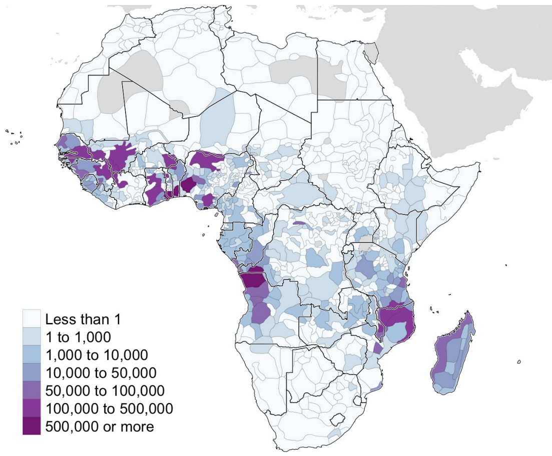
The key features of the empirical strategy are twofold. First, to unpack the causal effects, it adopts an instrumental variable approach leveraging soil suitability for cassava cultivation along with the timing of its arrival in Africa. Second, to investigate the mechanisms, it employs location- and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

3.1 Data and Measurements

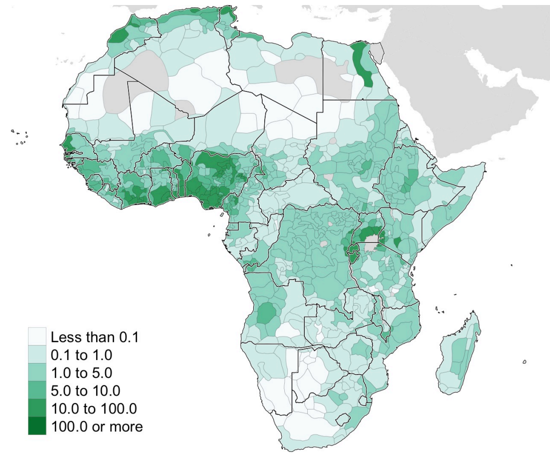
The unit of analysis is ethnic groups nested by host countries. The sample ethnolinguistic groups and settlement areas build upon the map of Murdock (1959) digitized by Nunn (2008). As described in detail below, the primary treatment indicator of slavery exposure is measured at Murdock ethnic group-level (Nunn, 2008; Nunn & Wantchekon, 2011). 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, $Slave^{\text{pc}}$, is normalized by the estimated population of each ethnic group in 1500 (HYDE data, Kees et al., 2011) as $Slave_i^{\text{pc}} = \ln\left(0.01 + \frac{\textit{Slave Export}_i}{\textit{Population}_{i,1500}}\right)$. $\textit{Slave Export}_i$ reflects the number of slave exports of group i during the 1700–1900 (post-cassava arrival) period, and $\textit{Population}_{i,1500}$ is group population in 1500 (Figures 1(a)–(c)). As an initial robustness check, I also employ an area-normalized measure, $Slave_i^{\text{Area}} = \ln\left(0.01 + \frac{\textit{Slave Export}_i}{\textit{Area}_i}\right)$, with Area_i indicating the area in km^2 of a group's historical homeland.

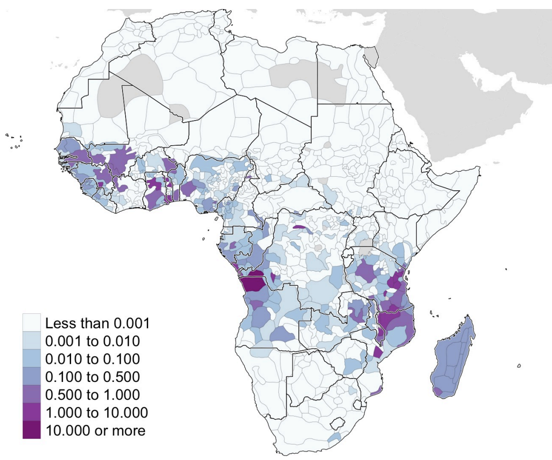
The analysis employs two series of outcome variables, each capturing distinct aspects of slave trade legacies. First, to capture the local dynamics of armed conflicts, I utilize the



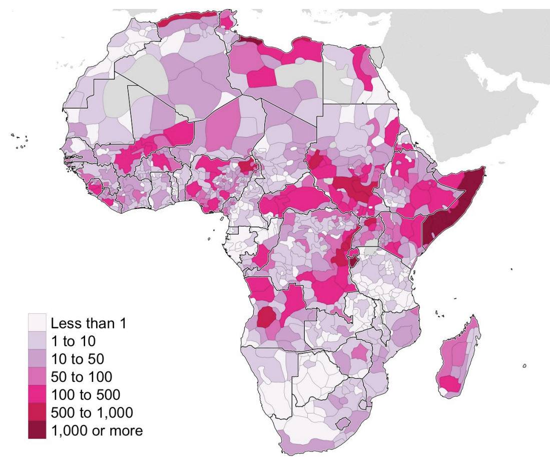
(a) Slave Trade Exports, 1700–1900



(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).

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.[4]

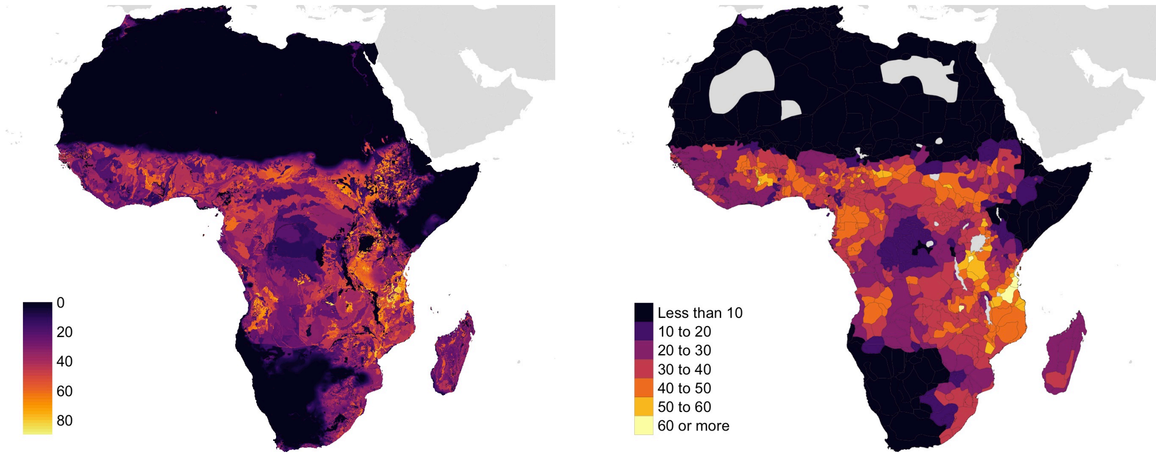
The coding of the group-level outcomes follows previous studies on the coup-civil war trade-off (e.g., Roessler, 2011, 2016; Roessler & Ohls, 2018). *Power Sharing* measures the fraction of years in which an ethnic group is included in the national power sharing schemes or coded as "monopoly," "dominant," "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.

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 overall 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 ($\sim$ 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 economic geographies, the first set includes logged population density in 1500, logged percentages of cropland and grassland in 1500 (Kees et al., 2011), and ecological diversity index (Fenske, 2014).[5] It 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

[4] 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 Murdock-EPR links. As reported in Appendix B, alternative threshold values produce qualitatively similar results.

[5] 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).



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

Figure 2: Soil Suitability for Cassava Cultivation

settlement area (1400–1700, Besley & Reynal-Querol, 2014).[6]

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, logged mean malaria ecology index (Kiszewski et al., 2004), geographical areas in logged km^2 of (total) homelands, share of the areas in host countries relative to the whole traditional homelands, and a dummy variable for partitioning by international borders.[7] I also include the distances in logged kilometers from homeland centroids to capital, closest borders, equator, and coastlines, and the GLUES overall agricultural suitability measure.

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 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 the concern for posttreatment bias, these variables are included for a robustness check purpose. Table A.1 in the Appendix reports descriptive statistics of the variables.

3.2 Identification Strategy: Cassava Suitability as an Instrument

Our 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 (classical) measurement error in slave export records

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

[7]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.

invites another identification concern for attenuation bias and discourages naïve comparisons.

To address these concerns, I rely on an instrumental variable approach. Motivated by previous studies (e.g., 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 show 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).[8] 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 foodstuff to sustain their travel. These pathways also mirror the insights of historical studies, for example, that "the exports of an individual region responded far more to local supply conditions than they did to the demand of European traders" (Curtin, 1969, 226).

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

[8]As reported in Appendix C, the maize-slavery association remains weaker compared to cassava at the group level, both substantially and statistically.

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 thus serves as a plausible falsification test.

3.3 Model Specification

To estimate the effect of slavery exposure on postcolonial political outcomes, I rely on the following two-stage specification:

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

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

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 for the first and second stages. $\boldsymbol{M}_{ic}$ 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).[9] The country-group setup follows previous studies, 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 are influence the baseline risks of domestic fighting and related outcomes.

The quantity of interest is τ, which captures the local average treatment effect (LATE) of group-level slavery exposure on the outcome variables. The expected sign of τ for each outcome is summarized in Table 1 in the previous section. 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.

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

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 estimates of the impacts of slavery exposure on battle incidents 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 five covariates with relative imbalance, along with precolonial population density and settlement area.[10] Columns 2–3 and 5–6 further adjust for the remaining baseline covariates 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. To keep the estimates conservative, the following analysis always adjusts for all baseline covariates.

4.2 Results I: Battle Exposure

Table 3 reports the IV-2SLS estimates for the location-based battle incident outcome (columns 2–3 and 5–6), along with the uninstrumented OLS results (columns 1 and 4). The coefficient estimates for slavery exposure remain indistinguishable from zero in the OLS estimates. Once instrumented, however, slave raid intensity shows a statistically significant negative coefficient sign, and the coefficient estimates remain stable regardless of model specifications

[10] Figure A.1 in the Appendix presents partial correlations between the instrument and each of the baseline covariates. The partial correlations remain negligible for most of the covariates. Exceptions out of the 21 baseline covariates include overall agricultural suitability and grassland proportion, and to a lesser extent, precolonial conflict presence, ecological diversity index, and water body presence. 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)			$\text{Slave}^{\text{Area}}$ (area-normalized slave exports, 1700–1900)		
	(1)	(2)	(3)	(4)	(5)	(6)
Cassava Suitability	0.192***	0.193***	0.195***	0.250***	0.244***	0.245***
	(0.034)	(0.034)	(0.035)	(0.047)	(0.047)	(0.047)
Coast Distance	−0.296***	−0.306***	−0.288***	−0.472***	−0.516***	−0.496***
	(0.061)	(0.073)	(0.073)	(0.086)	(0.103)	(0.102)
Observations	1,282	1,282	1,282	1,282	1,282	1,282
Adjusted R^2	0.405	0.420	0.424	0.433	0.448	0.452
F-statistic (weak instrument)	32.290	31.158	30.850	28.335	27.470	27.034
Stock and Yogo's critical value	16.380	16.380	16.380	16.380	16.380	16.380
Residual Moran's I (z-score)	−0.574	−0.923	−0.864	−0.068	−0.706	−0.680
	Panel B: LEDA-Connected Observations					
	(1)	(2)	(3)	(4)	(5)	(6)
Cassava Suitability	0.208***	0.194***	0.197***	0.257***	0.231***	0.237***
	(0.044)	(0.041)	(0.042)	(0.057)	(0.055)	(0.055)
Coast Distance	−0.301***	−0.286***	−0.271***	−0.471***	−0.483***	−0.470***
	(0.073)	(0.086)	(0.084)	(0.101)	(0.114)	(0.115)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Restricted covariates	✓	✓	✓	✓	✓	✓
Baseline covariates		✓	✓		✓	✓
Additional covariates			✓			✓
Observations	939	939	939	939	939	939
Adjusted R^2	0.426	0.440	0.447	0.450	0.469	0.470
F-statistic (weak instrument)	22.499	22.123	22.249	20.485	17.574	18.398
Stock and Yogo's critical value	16.380	16.380	16.380	16.380	16.380	16.380
Residual Moran's I (z-score)	−0.677	−0.990	−1.142	−0.737	−0.987	−0.903

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 second-stage outcome.

and slave raid measures, thereby providing initial support for Hypothesis 1.

In addition to its persistence, the conflict-reducing effect is 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 estimate of −0.896 in column (2) translates into that, a 1 percentage point increase in the slavery exposure is followed by a roughly 0.9 percentage point decrease in the battles in a group's traditional homeland.

Besides the difference between the average treatment effect (ATE) and the LATE, the discrepancy between the OLS and IV-2SLS estimates 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 inherent or unobserved group-level propensity to engage in fighting is positively associated with slavery exposure and current exposure to battle events. For example, inherently war-prone groups might have been heavily exposed by slave exports due to the increased prisoners of war as a result of fighting. Indeed, historical

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

	Dependent variable: $\ln(1 + \text{Battle})$					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	0.001	-0.896^{**}	-0.957^{**}			
	(0.032)	(0.387)	(0.394)			
Slave$^{\mathbf{Area}}$				0.019	-0.708^{**}	-0.762^{**}
				(0.027)	(0.305)	(0.316)
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	0.544			0.551		
F-statistic (weak instrument)		31.158	30.850		27.470	27.034
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	-0.037	-0.409	-0.551	-0.414	-0.617	-0.262

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

accounts highlight the role of local warfare and political disturbance in shaping slave supply and exports (Curtin, 1969, 226). The likely 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 outcome. Second, the IV-2SLS, instead of the OLS, estimates might remain biased due to the violations of the identification assumptions Despite the instrument relevance, the IV-2SLS estimates can still be suffering the exclusion restriction violations. The falsification tests in Section 5, however, fail to falsify the proposed design and provide credibility to the interpretation of the IV results as causal effects.

4.3 Results II: Coup-Civil War Trade-Off

Recall that group-based outcome variables allow for distinguishing the two group-level pathways as well as ruling out the alternative explanation stressing population influx rather than group-level legacies. If slavery exposure influences postcolonial politics by removing the indigenous population and facilitating the influx of new 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 proposed

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

	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Dependent variable: Power Sharing					
Slave$^{\mathbf{pc}}$	−0.004	0.212***	0.205***			
	(0.004)	(0.079)	(0.078)			
Slave$^{\mathbf{Area}}$				−0.006	0.178**	0.171**
				(0.004)	(0.072)	(0.069)
Observations	939	939	939	939	939	939
Adjusted R^2	0.729			0.730		
F-statistic (weak instrument)		22.123	22.249		17.574	18.398
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.196	−0.942	−0.652	−0.244	−0.618	−0.701
	Panel B. Dependent variable: Rebel					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	0.007	−0.215***	−0.214***			
	(0.005)	(0.078)	(0.077)			
Slave$^{\mathbf{Area}}$				0.009*	−0.181***	−0.178***
				(0.005)	(0.069)	(0.067)
Observations	939	939	939	939	939	939
Adjusted R^2	0.822			0.822		
F-statistic (weak instrument)		20.888	21.192		16.744	17.506
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	0.145	−0.597	−0.828	0.278	−0.490	−0.879
	Panel C. Dependent variable: Coup					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	0.007	0.373***	0.368***			
	(0.007)	(0.129)	(0.125)			
Slave$^{\mathbf{Area}}$				0.005	0.314***	0.305***
				(0.007)	(0.115)	(0.112)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	939	939	939	939	939	939
Adjusted R^2	0.727			0.726		
F-statistic (weak instrument)		22.680	22.640		18.071	18.569
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.491	0.032	−1.356	−0.380	0.743	−0.040

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

institutional account by revealing coefficient signs consistent with the theoretical predictions. The IV-2SLS results in columns 2–3 and 5–6 indicate that groups with greater exposure to slave raids are more likely to be included in national power-sharing schemes (Panel A) while less likely to fight civil wars against the central government (Panel B) and more likely to stage coup attempts in postcolonial states (Panel C). Recall that *Rebel* and *Coup* are binary, while *Slave* is measured in the logarithm scale. Substantively, the estimates in column (2) indicate that a 10% increase in slavery exposure is followed by a 2 percent increase in power sharing prevalence ($1.10^{0.212} \sim 1.02$), a 2 percent decrease in the rebel risk ($1.10^{-0.215} \sim 0.98$), and a 3.6 percent increase in coup risk in the postcolonial period ($1.10^{0.373} \sim 1.036$).

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 exposure relative to the pre-arrival exposure, (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 a shorter-run outcome in the presented argument. Collectively, these results lend further confidence in the robustness of the main empirical results and the advanced causal pathway.

5 Falsification Tests and Instrument Validity

Thus far, I interpret the IV estimates as the causal effects of group-level slave trade exposure on postcolonial politics. A remaining but important concern for the causality claims is the potential exclusion restriction violations. To address this concern, this section presents multifaceted falsification tests motivated by theoretical, empirical, and methodological reasons.

5.1 Placebo Treatment: Slave Raids Before Cassava's Arrival

The first falsification test exploits the timing of cassava's arrival in Africa in the sixteenth to seventeenth centuries. If the proposed instrument is valid, we should not observe a systematic first-stage association between cassava suitability and slavery exposure *before* cassava's arrival, or a characteristic determined before the instrument (cassava suitability) retained significance and the assignment of the treatment (post-arrival exposure). Any systematic cassava suitability-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.[11]

Table 5 reports the false first-stage estimates. Consistent with the proposed IV strategy, the cassava-slavery association remains statistically indistinguishable from zero once the slavery variables are replaced by the pre-arrival measures.[12] By contrast, the coefficients on coast distance remain negative and statistically significant in the pre-arrival period estimates.

[11] I drop the records of slave exports in the 1600s as the slavery exposure during this period is likely to be partly (un)affected by the cassava suitability instrument.

[12] Table C.1 in the Appendix replicates the analysis without the dummies for historical kingdom and conflict prevalence (1400–1700) as these variables can be a direct product of the earlier slave trades.

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	0.016	0.017	0.025	0.028
	(0.014)	(0.017)	(0.017)	(0.020)
Coast Distance	-0.135^{***}	-0.104^{**}	-0.170^{***}	-0.130^{***}
	(0.038)	(0.042)	(0.041)	(0.039)
Country FE	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓
LEDA-connected sample		✓		✓
Observations	1,282	939	1,282	939
Adjusted R^2	0.397	0.419	0.435	0.454
F-statistic (weak instrument)	1.450	1.028	2.352	1.820
Stock and Yogo's critical value	16.380	16.380	16.380	16.380
Residual Moran's I (z-score)	0.625	−0.815	0.307	−0.417

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

The consistently negative association further suggests that the exercise is not just picking up the demographically less severe nature of the slave raids in 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 other 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 (non-cassava crop) instruments

Figures C.1 and C.2 in the Appendix plot the F-statistics for the placebo instruments. The figures subsequently replace the cassava suitability by one of the 15 crops available in the GLUES data and re-estimate the first-stage regressions, for each of the population-normalized and area-normalized slavery measures as the first-stage outcome. The false first-stage estimates reveal weak predictive power of non-cassava crops, with the corresponding 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 unadjusted heterogeneity.

5.3 Further Falsification Tests

Appendix C reports three additional falsification tests. The first exercise leverages riots and protests as a placebo outcome. Demonstration events are largely self-organized from the bottom and unrelated with group-level institutions linking the slavery legacies and postcolonial politics. The second examines reduced-form associations. Not to invite concerns for the exclusion restriction violations, the instrument should not be associated 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 credible 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 important implications for a broader body of literature and offer avenues for future research. First, the persistent impacts of slavery on postcolonial politics in Africa highlight the importance of historical confounders to investigate the power sharing-conflict link. Although limited to the context of postcolonial Africa, the empirical results suggest that the history of slavery exposure 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
(Not for Publication)

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October 19, 2020

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 additional falsification tests.

The empirical analysis reported in this article were conducted in R 4.0.2. I primarily rely on sf, sp, and cshapes packages for geoprocessing procedures (Bivand, Pebesma, and Gomez-Rubio, 2013; Pebesma and Bivand, 2005; Weidmann, Kuse, and Gleditsch, 2010). The Murdock-EPR group matching procedure and the regression analysis utilize LEDA and lfe packages for R (Gaure, 2013; Müller-Crepon, Pengl, and Bormann, 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. Figure A.1 displays partial correlations between the instrument (cassava suitability) and the baseline covariates.

A.1 Partial Correlation

Each panel of Figure A.1 plots partial correlation between the cassava suitability index and one of the baseline covariates. 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 baseline covariates excepting itself, location polynomial, and country fixed effects. Solid segments in each panel represents a linear regression fit, and the text reports the estimated slope.

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

$$\begin{aligned} Cassava_{ic} &= \alpha_c + \boldsymbol{X}'_{ic,-k}\boldsymbol{\beta} + f(\boldsymbol{s}_{ic}) + e_{ic}, \\ X_{ic,k} &= \mu_c + \boldsymbol{X}'_{ic,-k}\boldsymbol{\eta} + g(\boldsymbol{s}_{ic}) + u_{ic}, \end{aligned}$$

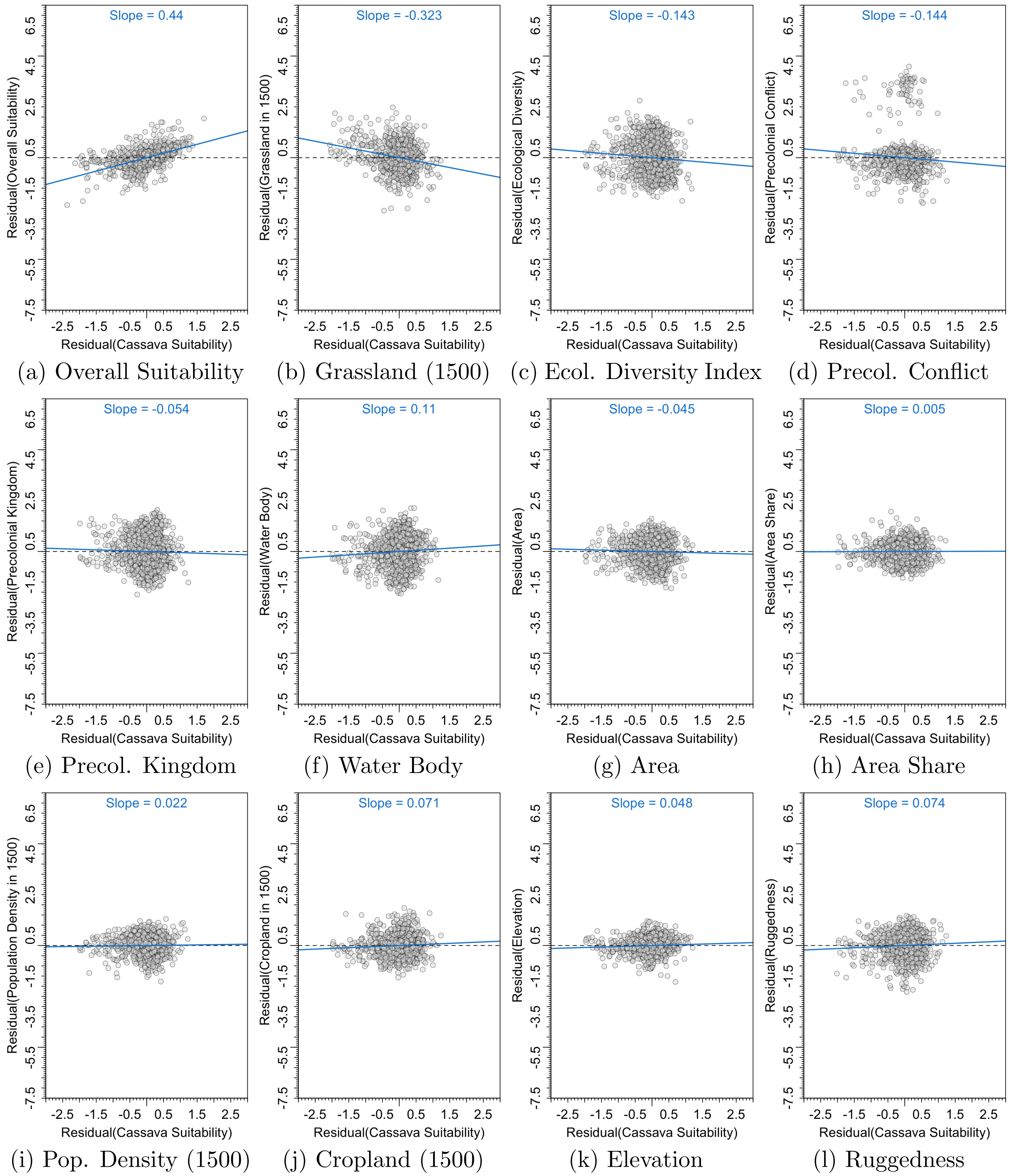
Table A.1: Descriptive Statistics

	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
Coast Distance	1,282	5.978	1.159	6.300	1.456
Population Density in 1500	1,282	0.578	1.539	0.659	1.789
Cropland in 1500	1,282	0.708	0.724	0.473	0.804
Grassland in 1500	1,282	1.314	0.647	1.366	0.859
Area	1,282	9.801	1.321	9.716	1.904
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
Malaria Index	1,282	2.320	1.053	2.731	1.247
Water Body	1,282	0.596	0.491	1.000	1.000
Ecological Diversity Index	1,282	0.279	0.229	0.292	0.474
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.

where all right-hand-side variables and coefficients are defined analogously to the main specification in Section 3 of the main text. The left-hand-side (dependent) variables are standardized for the purpose of comparison. I then compute and plot the regression residuals in Figure A.1. Since the cassava-covariate association attributable to the other covariates has been partialled out, the depicted covariance can be attributed to the conditional covariance between cassava suitability index and the kth covariate.

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



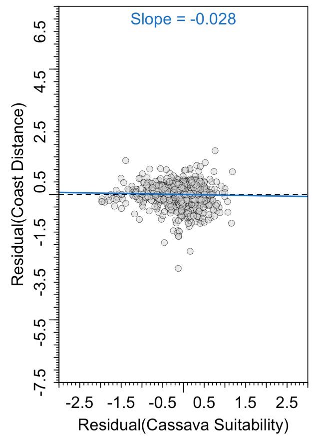
(a) Overall Suitability (b) Grassland (1500) (c) Ecol. Diversity Index (d) Precol. Conflict

(e) Precol. Kingdom (f) Water Body (g) Area (h) Area Share

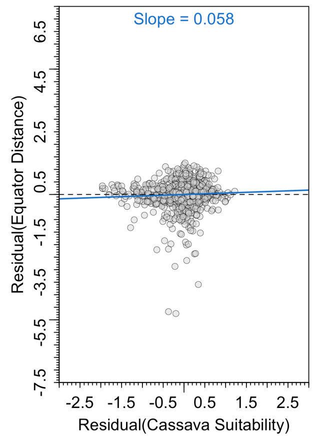
(i) Pop. Density (1500) (j) Cropland (1500) (k) Elevation (l) Ruggedness

Figure A.1: Partial Correlations between the Cassava Suitability Index and the Baseline Covariates

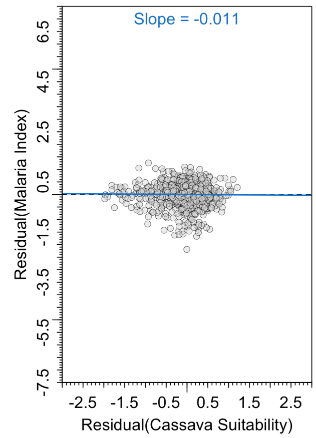
Notes: Each panel displays (standardized) partial correlation between the cassava suitability index (instrument) and one of the baseline covariates. The cassava suitability index on the horizontal axis is residualized by the baseline covariates excepting for the vertical axis variable, location polynomial, 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. Solid segments represent linear regression fits, and the text reports the estimated slopes.



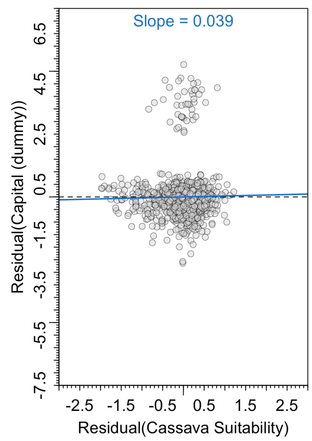
(m) Coast Distance



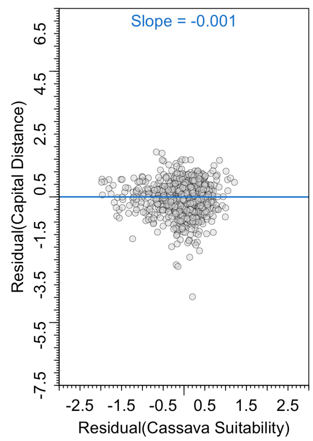
(n) Equator Distance



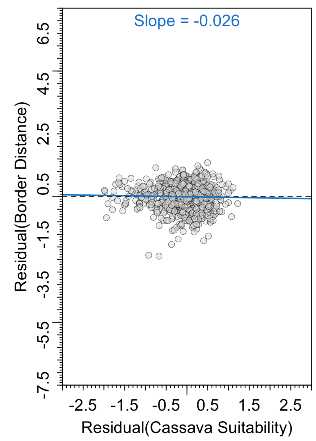
(o) Malaria Index



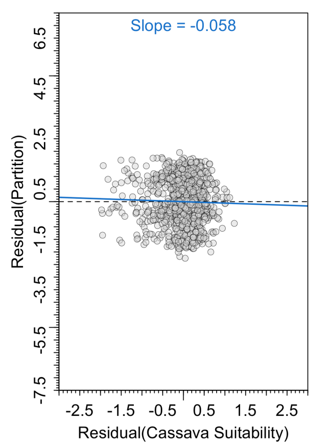
(p) Capital (dummy)



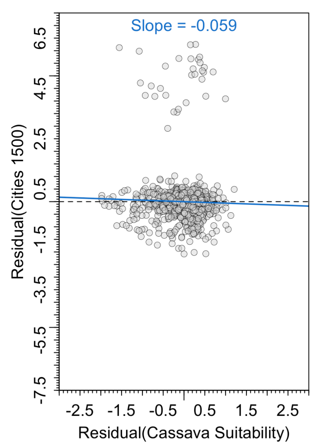
(q) Capital Distance



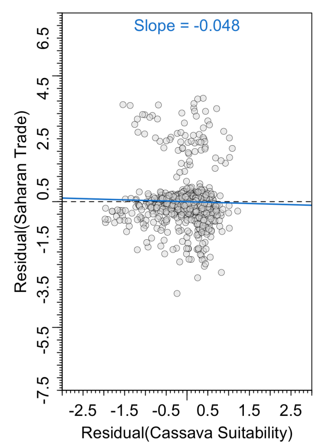
(r) Border Distance



(s) Partition



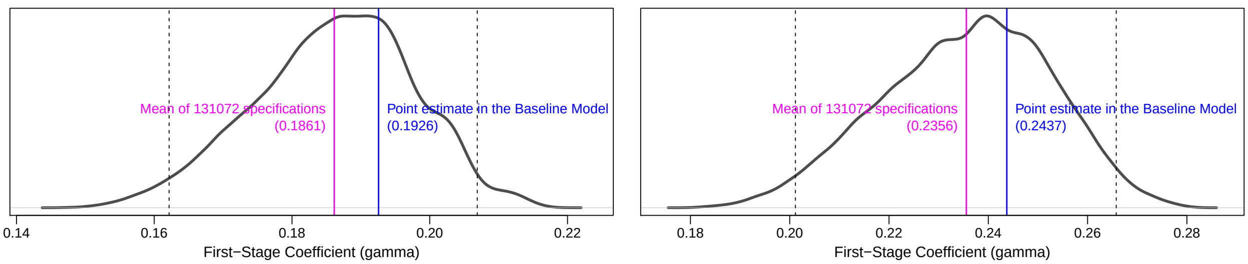
(t) Cities (1500)



(u) Trade Route Cities

Figure A.1 (contd.): Partial Correlations between the Cassava Suitability Index and the Baseline Covariates

ates. Exceptions include overall agricultural suitability and grassland proportion (panels (a) and (b)) out of the 21 baseline covariates, and to a lesser extent, Fenske's (2014) ecological diversity index, precolonial conflict presence, and water body presence (panels (c), (d), and



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

Figure A.2: Empirical Distributions of the First-Stage Cassava-Slavery Association

Notes: Empirical distributions of the first-stage coefficients on cassava suitability (γ) across $2^{17} = 131,072$ model specifications, with (a) population-normalized and (b) area-normalized slave trade 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 of the empirical distributions, and the range between dashed segments cover the corresponding 95% confidence intervals.

(f)). The absence of systematic associations between the instrument (cassava suitability) and most of the covariates suggest that, as far as other covariates are adjusted for, most of the baseline covariates remain incapable of inducing confounding bias due to the lack of systematic association with the instrument while including these covariates in the regression model increases accuracy of the model if they are associated with the outcomes. To prevent the covariate imbalance from plaguing the analysis, the empirical analysis in the main text always adjust for the baseline covariates and report conservative estimates.

A.2 Model Dependence

One might wonder if covariate imbalance and possible model dependence invite any bias and robustness concerns into the analysis. To guard against arbitrary picking of model specifications, I follow the approach of Ho, Imai, King et al. (2007) to replicate the first-stage estimate for each of possible $2^{17} = 131,072$ model specifications with a different combination of 17 covariates, along with the two covariates with relative imbalance (overall suitability and grassland proportion), precolonial population density, settlement area, location polynomial, and country fixed effects. Recall that the models in Table 2 in the main text always adjust for these right-hand-side variables.

Figure A.2 shows the empirical distribution of the coefficient estimate obtained from the $131,072$ model specifications, for each of 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 (exclusion) of the remaining covariates are unlikely to invite systematic bias into the first-stage estimates beyond random noise.

B Robustness Checks and Further Results

The first three subsections in the following present the details of the robustness checks, and the last examines the slavery-absolutism association to further validate the institutional change pathway linking slavery exposure and postcolonial politics.

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 exposure (1400–1599). Specifically, I employ alternative slave export measures, $\Delta Slave_i^{\text{pc}} = \ln\left(0.01 + \frac{\Delta Slave\ Export_i}{Population_{i,1500}}\right)$ and $\Delta Slave_i^{\text{Area}} = \ln\left(0.01 + \frac{\Delta Slave\ Export_i}{Area_i}\right)$, with $\Delta Slave\ Export_i = Slave\ Export_{i,1700\text{–}1900} - Slave\ Export_{i,1400\text{–}1599} + s$ and $s = |\min(Slave\ Export_{i,1700\text{–}1900} - Slave\ Export_{i,1400\text{–}1599})|$.[1]

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 important concern is the reliance on the ACLED data to construct the location-based battle exposure in the empirical analysis in the main text. Reasonably, the exclusive reliance on a single database invites a robustness concern such that the empirical findings might depend on the use of the ACLED data.

To address this robustness concern, I employ the xSub database to construct an alternative battle incident measure (Zhukov, Davenport, and Kostyuk, 2019). The xSub dataset hosts a number of different event datasets including the ACLED data and the UCDP Georeferenced Event Data (GED, Sundberg and Melander, 2013), and allows for combining the entries in different sources into a single dataset by the Matching Event Data by Location, Time, and Type (MELTT) algorithm (Donnay, Dunford, McGrath et al., 2019). I restrict the temporal scope of the analysis to the 1997–2019 period as the ACLED data, one of the major

[1] I thank Yuichiro Yoshida for suggesting this specification.

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

	Dependent variable: ln(1 + Battle)					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Δ**Slave**$^{\mathbf{pc}}$	0.092	-2.755^{**}	-3.072^{**}			
	(0.082)	(1.229)	(1.326)			
Δ**Slave**$^{\mathbf{Area}}$				0.090	-3.162^{**}	-3.442^{**}
				(0.109)	(1.522)	(1.607)
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	0.547			0.546		
F-statistic (weak instrument)		24.318	20.658		15.702	12.999
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	-0.472	-0.473	-0.197	-0.278	-0.326	-0.126

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

sources of MELTT-integrated xSub event dataset for the study region, covers the period after 1997.[2] I rely on the xSub-MELTT algorithm with a 1-km by 1-day spatio-temporal window setting 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 Section 3 of the main text to construct the alternative battle exposure measure, *xSub Battle*.[3] Figure B.1 depicts the spatial distribution of the battle incidents in the xSub database.

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.

B.3 Alternative Murdock-EPR Group-Matching Thresholds

The main analysis connects the ethnic groups in Murdock (1959) to the groups in the Ethnic Power Relations (EPR) dataset (Cederman, Wimmer, and Min, 2010; Vogt, Bormann,

[2] The temporal coverage of the xSub database is limited to the pre-2019 period.

[3] I extract "DYAD_A_B" (Government-Opposition interaction) entries to construct *xSub Battle* variable. See Zhukov, Davenport, and Kostyuk (2019) for the event coding in the xSub database. The xSub records were obtained on October 14, 2020, using R-package xSub.

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

	Panel A. Dependent variable: Power Sharing					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
ΔSlave$^{\mathbf{pc}}$	-0.017	0.675**	0.640**			
	(0.011)	(0.268)	(0.260)			
ΔSlave$^{\mathbf{Area}}$				-0.038**	0.733**	0.705**
				(0.015)	(0.308)	(0.296)
Observations	939	939	939	939	939	939
Adjusted R^2	0.730			0.731		
F-statistic (weak instrument)		15.835	16.337		13.580	14.084
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	-0.253	-0.675	-0.611	-0.282	-0.265	-0.526
	Panel B. Dependent variable: Rebel					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
ΔSlave$^{\mathbf{pc}}$	0.013	-0.685***	-0.667***			
	(0.011)	(0.261)	(0.255)			
ΔSlave$^{\mathbf{Area}}$				0.008	-0.743**	-0.735**
				(0.024)	(0.292)	(0.290)
Observations	939	939	939	939	939	939
Adjusted R^2	0.821			0.822		
F-statistic (weak instrument)		15.223	16.364		13.182	13.934
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	0.426	-0.460	-0.052	0.026	0.090	0.309
	Panel C. Dependent variable: Coup					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
ΔSlave$^{\mathbf{pc}}$	0.025	1.187***	1.145***			
	(0.016)	(0.445)	(0.433)			
ΔSlave$^{\mathbf{Area}}$				0.024	1.289**	1.262**
				(0.031)	(0.505)	(0.491)
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	939	939	939	939	939	939
Adjusted R^2	0.724			0.721		
F-statistic (weak instrument)		16.440	16.537		13.941	13.922
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	-0.025	0.743	0.060	0.408	0.853	0.019

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

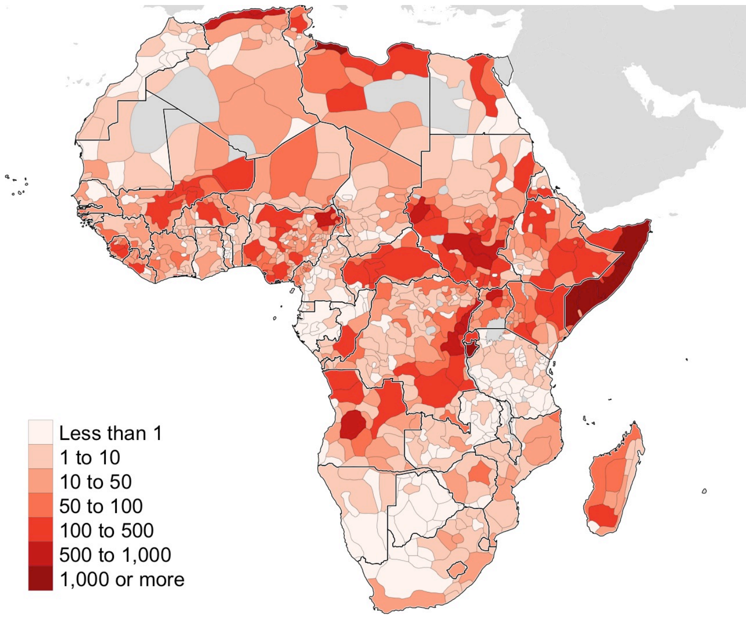


Figure B.1: xSub Battle Events, 1997–2019

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	-0.952^{**}	-1.069^{***}			
	(0.036)	(0.391)	(0.403)			
Slave$^{\mathbf{Area}}$				0.048	-0.752^{**}	-0.852^{***}
				(0.030)	(0.318)	(0.329)
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	0.549			0.550		
F-statistic (weak instrument)		31.075	31.835		27.309	27.023
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	-1.305	-1.260	-1.458	-1.241	-1.269	-1.622

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

Ruegger et al., 2015) using the Linking Ethnic Data from Africa (LEDA) database (Müller-Crepon, Pengl, and Bormann, 2019). The analysis employs LEDA's linguistic distance algorithm, with the linguistic distance (ranging from 0 to 1) threshold of 0.2. The baseline setting restricts the Murdock-EPR links to the group pairs with the linguistic distance smaller than 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 sensitivity concern, Tables B.4 and B.5 replicate the analysis of Table 4 in the main text with alternative threshold values of 0.3 and 0.4, respectively. A larger threshold value permits matching with greater linguistic distances, and a smaller value restricts the matching to the pairs with smaller distances. As reported in the tables, the results remain qualitatively unchanged regardless of the choice of threshold values for the group-matching procedure and the resultant sample sizes.

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 → absolutist political structure → 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 identification strategy 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*, take 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 and Nunn (2018) to construct this variable. The model specification broadly follows the baseline two-stage specification in Section 3 of the main text, with the spatial polynomial term is specified as a linear polynomial for the reduced sample due to additional missing values in the absolutist index. A the analysis concerns colonial periods before independence of African countries, 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.[4]

[4] Excluded covariates are *Area Share*, *Border Distance*, *Capital* (dummy), *Capital Distance*, and *Partition*. Remaining group-level 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 LEDA Linguistic Distance Threshold = 0.3

	Panel A. Dependent variable: Power Sharing					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
Slave^{pc}	−0.001	0.235***	0.220***			
	(0.004)	(0.082)	(0.082)			
Slave^{Area}				−0.004	0.194***	0.182***
				(0.004)	(0.071)	(0.070)
Observations	1,080	1,080	1,080	1,080	1,080	1,080
Adjusted R^2	0.727			0.726		
F-statistic (weak instrument)		22.171	22.131		18.441	18.323
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	0.138	−1.151	−0.884	0.259	−1.438	−1.464
	Panel B. Dependent variable: Rebel					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
Slave^{pc}	−0.003	−0.151*	−0.161**			
	(0.006)	(0.077)	(0.079)			
Slave^{Area}				0.0005	−0.125*	−0.134**
				(0.006)	(0.064)	(0.067)
Observations	1,080	1,080	1,080	1,080	1,080	1,080
Adjusted R^2	0.756			0.757		
F-statistic (weak instrument)		23.411	22.865		19.041	18.098
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	0.189	−0.337	−0.266	0.074	−0.480	−0.239
	Panel C. Dependent variable: Coup					
	OLS (1)	IV-2SLS (2)	IV-2SLS (3)	OLS (4)	IV-2SLS (5)	IV-2SLS (6)
Slave^{pc}	0.001	0.302**	0.291**			
	(0.006)	(0.119)	(0.121)			
Slave^{Area}				0.001	0.250**	0.241**
				(0.006)	(0.102)	(0.103)
LEDA language distance threshold	0.3	0.3	0.3	0.3	0.3	0.3
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	1,080	1,080	1,080	1,080	1,080	1,080
Adjusted R^2	0.762			0.762		
F-statistic (weak instrument)		24.098	23.996		19.071	18.702
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.761	−0.515	−0.196	−0.620	−0.824	−0.357

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

Table B.5: Slave Raid Intensity and Power Sharing, Rebel, and Coup, 1946–2013, with LEDA Linguistic Distance Threshold = 0.4

	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	0.176**	0.165**			
	(0.004)	(0.075)	(0.075)			
Slave$^{\mathbf{Area}}$				−0.001	0.145**	0.136**
				(0.003)	(0.063)	(0.062)
Observations	1,136	1,136	1,136	1,136	1,136	1,136
Adjusted R^2	0.753			0.753		
F-statistic (weak instrument)		25.694	23.268		20.650	19.605
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.480	−1.274	−1.656*	−0.164	−0.741	−1.587
	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	−0.160**	−0.171**			
	(0.004)	(0.076)	(0.077)			
Slave$^{\mathbf{Area}}$				−0.002	−0.131**	−0.141**
				(0.004)	(0.063)	(0.065)
Observations	1,136	1,136	1,136	1,136	1,136	1,136
Adjusted R^2	0.812			0.812		
F-statistic (weak instrument)		26.470	25.084		21.325	19.916
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	0.444	0.213	0.194	0.420	0.235	−0.009
	Panel C. Dependent variable: Coup					
	OLS	IV-2SLS	IV-2SLS	OLS	IV-2SLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave$^{\mathbf{pc}}$	−0.004	0.278**	0.282**			
	(0.005)	(0.119)	(0.121)			
Slave$^{\mathbf{Area}}$				−0.006	0.228**	0.233**
				(0.004)	(0.099)	(0.103)
LEDA language distance threshold	0.4	0.4	0.4	0.4	0.4	0.4
Country FE	✓	✓	✓	✓	✓	✓
Lon-Lat polynomial	✓	✓	✓	✓	✓	✓
Moran eigenvectors	✓	✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Additional covariates			✓			✓
Observations	1,136	1,136	1,136	1,136	1,136	1,136
Adjusted R^2	0.803			0.803		
F-statistic (weak instrument)		24.140	23.725		20.646	19.571
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.598	−0.883	−1.151	−0.295	−0.805	−1.321

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

Table B.6: Slave Raid Intensity and Absolutist Political Authority

	Dependent variable:					
	Slave^{pc}	Absolutism		$\text{Slave}^{\text{Area}}$	Absolutism	
	First stage	OLS	IV-2SLS	First stage	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Slave^{pc}		0.002	0.174^{**}			
		(0.018)	(0.088)			
$\text{Slave}^{\text{Area}}$					0.003	0.144^{*}
					(0.014)	(0.073)
Cassava Suitability	0.591^{***}			0.715^{***}		
	(0.100)			(0.126)		
Observations	429	429	429	429	429	429
Adjusted R^2	0.359	0.107		0.406	0.108	
F-statistic (weak instrument)	35.024		35.024	32.345		32.345
Stock and Yogo's critical value	16.380		16.380	16.380		16.380
Residual Moran's I (z-score)	−0.114	0.518	0.604	0.334	0.511	0.604
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.

Table B.6 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 pass the critical value of Stock and 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 systematic second-stage association suggests greater exposure to slave raids is 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 Additional Falsification Tests

Section 5 of the main text reports a series of falsification tests, leveraging (1) the arbitrary timing of cassava's arrival in Africa (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

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

	$\text{Slave}^{\text{pc}}_{1400-1599}$ (population-normalized slave exports, 1400–1599)		$\text{Slave}^{\text{Area}}_{1400-1599}$ (area-normalized slave exports, 1400–1599)	
	(1)	(2)	(3)	(4)
Cassava Suitability	0.014	0.016	0.024	0.026
	(0.014)	(0.016)	(0.016)	(0.019)
Coast Distance	-0.136^{***}	-0.103^{**}	-0.170^{***}	-0.128^{***}
	(0.039)	(0.042)	(0.041)	(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	0.401	0.418	0.437	0.454
F-statistic (weak instrument)	1.120	0.973	2.181	1.775
Stock and Yogo's critical value	16.380	16.380	16.380	16.380
Residual Moran's I (z-score)	0.717	−1.316	0.761	−0.353

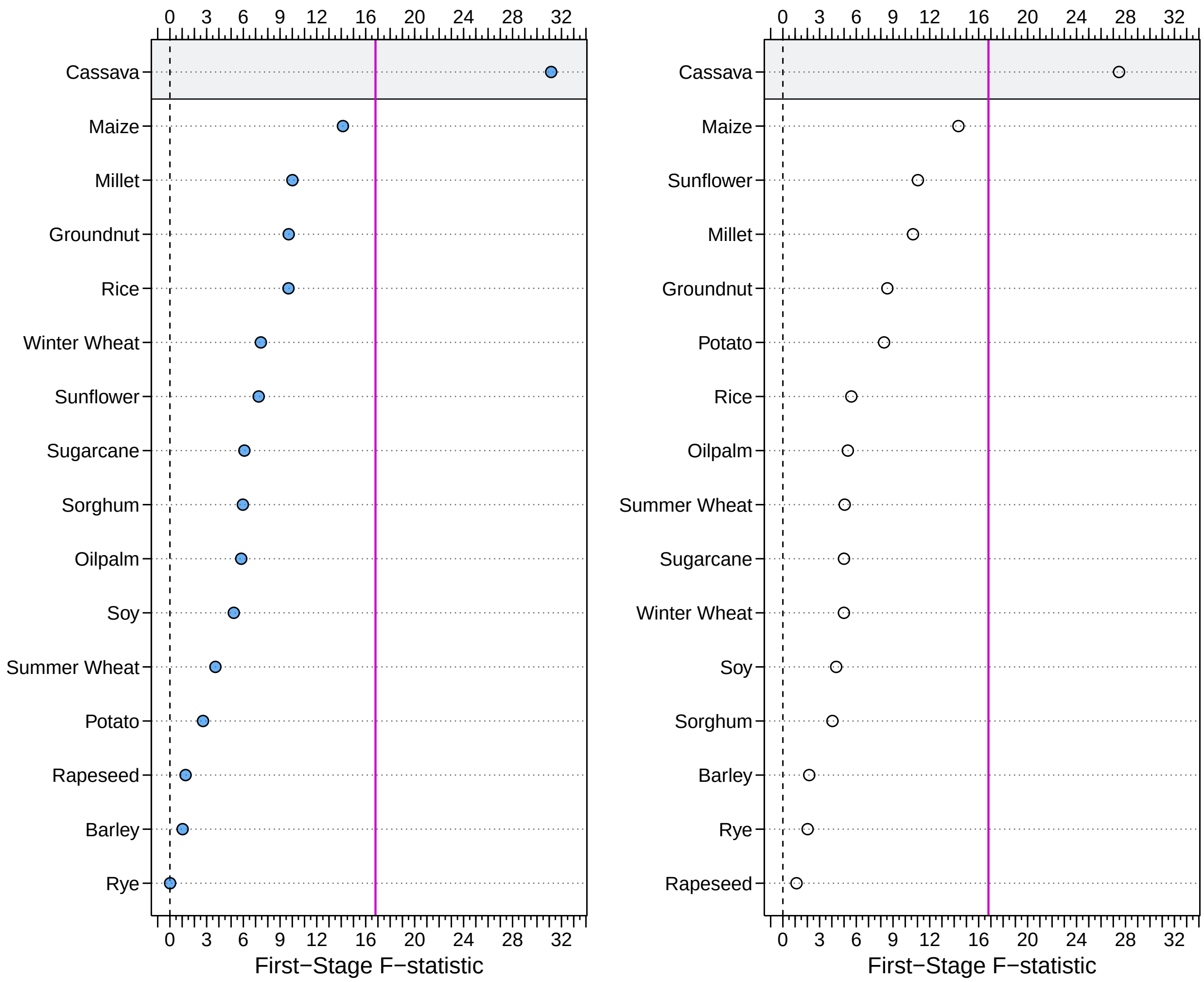
Notes: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Robust standard errors in parenthesis.

slave raids (placebo subsample). Table C.1 replicates the falsification regressions in Table 5 in the main text without the dummy variables for conflict prevalence and kingdom presence (1400–1700) and yields qualitatively similar estimates. The remainder of this section reports the details of the remaining four falsification tests.

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

Motivated by the approaches of Lowes and Montero (2020) and Miguel, Satyanath, and Sergenti (2004), the second falsification test in the main text exploits the land soil suitability for non-cassava crops as placebo instruments.[5] If cassava suitability, rather than omitted heterogeneity, influenced group-level slavery exposure, we have little reason to see a systematic first-stage association for the placebo suitability measures for non-cassava crops. Strong first-stage associations for non-cassava crops suitability would 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.

[5]Lowes and 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, Satyanath, and Sergenti (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).

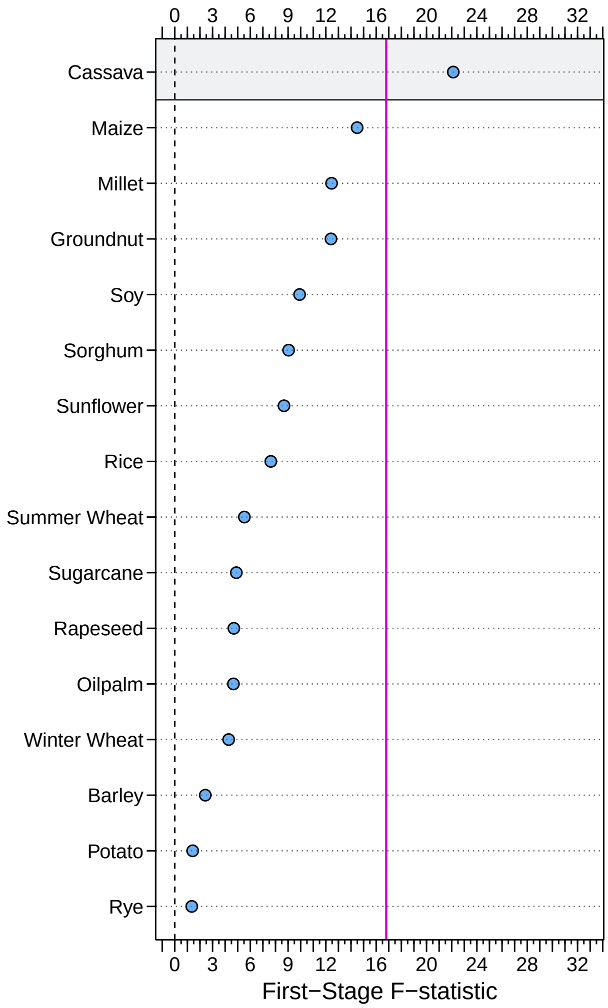


(a) Population-Normalized Slave Trade Export (b) Area-Normalized Slave Trade Export

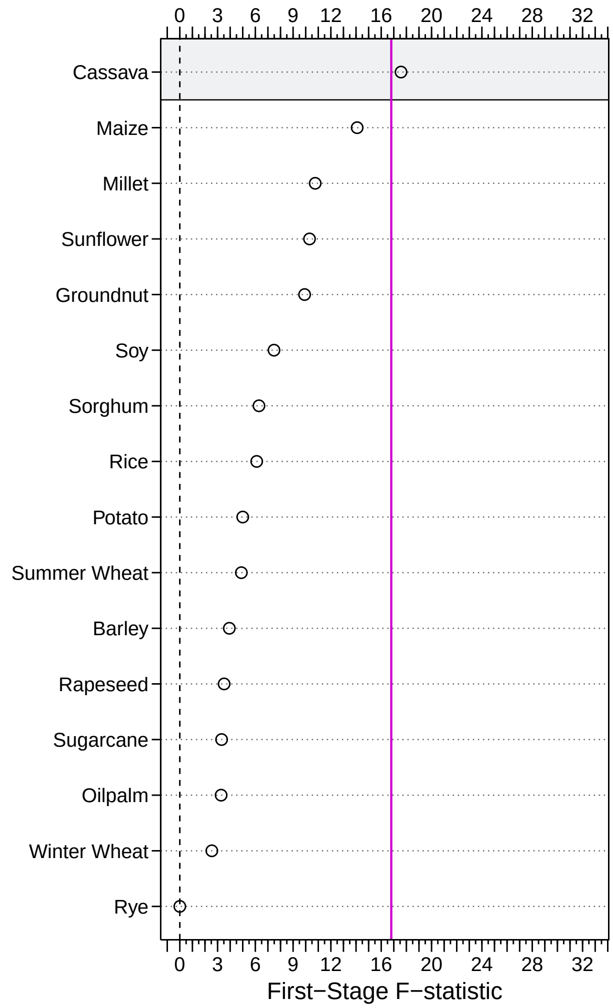
Figure C.1: First-Stage F-statistics for Cassava Suitability and Placebo Instruments

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 for the crop on the vertical axis. Solid vertical segments indicate the critical value against weak instruments in Stock and Yogo (2005).

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 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



(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.

segments indicate Stock and Yogo's (2005) critical value of 16.38 for weak instruments.

As in Figures C.1 and C.2, 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 and Moreno-Cruz (2019), among the 15 non-cassava crops, the suitability for maize, another major New World crop introduced to Africa, is most strongly associated with slavery exposure, yet the association remains weaker at ethnic group level than in country level and fails to pass the critical value. Regardless of the slavery measures and (sub)samples, only cassava suitability index out of the 16 crop suitability measures passes Stock and 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 raid exposure.

C.2 Placebo Outcome: Riot and Protest Events

The third exercise leverages placebo or negative control outcomes, or the outcomes that can plausibly be assumed to share similar confounders while unaffected by the treatment, or "U-comparable" to battles (Lipsitch, Tchetgen, and Cohen, 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 likely remain unrelated with the group-level institutional constraints linking the slavery legacies and postcolonial politics.[6] The expected null-finding and outcome specificity provide further confidence in the main results (Lipsitch, Tchetgen, and Cohen, 2010; Rosenbaum, 1989).

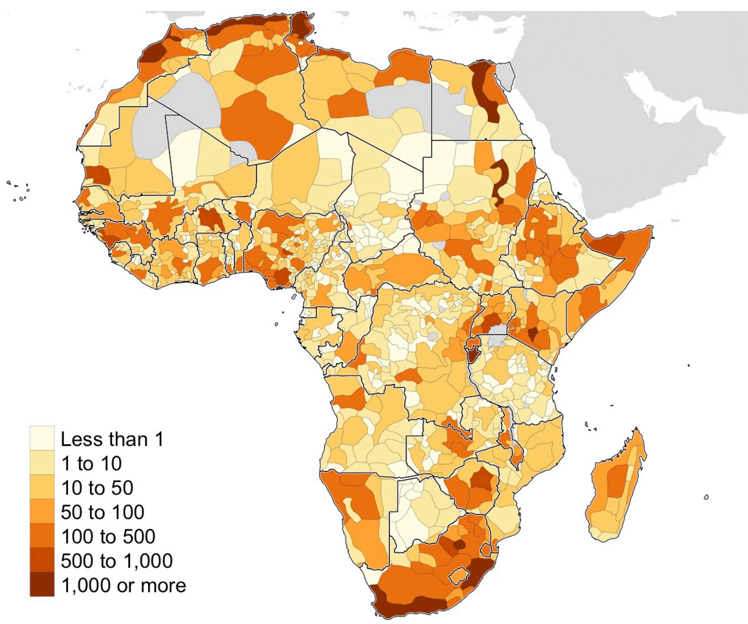


Figure C.3: ACLED Riot and Protest Events, 1997–2020

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).

Specifically, relying on the same geoprocessing procedure in Section 3 of the main text, I aggregate the 69,448 riot and protest events during the 1997–2020 period at the country-

[6] Although not in an IV setup, Depetris-Chauvin (2016) presents a similar approach.

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$^{\mathbf{pc}}$	−0.016	0.188	0.037			
	(0.030)	(0.323)	(0.321)			
Slave$^{\mathbf{Area}}$				−0.003	0.148	0.030
				(0.026)	(0.253)	(0.257)
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	0.634			0.634		
F-statistic (weak instrument)		30.652	30.493		26.678	26.941
Stock and Yogo's critical value		16.380	16.380		16.380	16.380
Residual Moran's I (z-score)	−0.204	−0.339	−0.843	−0.173	−0.471	−0.892

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

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 effects of slavery exposure on 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 help us investigate violations of the exclusion restriction invited by potential cassava-outcome pathways other than slave trade exposure. For example, a combination of systematic associations (1) between cassava suitability and current population density within group territories and (2) between current population density and battle activities can open up an instrument-outcome pathway not through slavery exposure. Not to invite the concerns for the exclusion restriction violations, the instrument should not be associated with the (unadjusted) correlates of civil war or power sharing. To empirically investigate the concern, I estimate the reduced-form associations with the following specification:

$$Y_{ic}^{RF} = \kappa_c + \zeta Cassava_{ic} + \boldsymbol{X}_{ic}'\boldsymbol{\phi} + \boldsymbol{EV}_{ic}'\boldsymbol{\rho} + h(\boldsymbol{s}_{ic}) + v_{ic}, \tag{C.1}$$

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

Table C.3: Reduced-Form Regressions for the Main Outcomes and the Correlates of the Coup-Civil War Trade-Off

	Main Outcomes				Potential Correlates	
	$\ln(1 + \text{Battle})$	Power Sharing	Rebel	Coup	Nightlight	Population Density
	(1)	(2)	(3)	(4)	(5)	(6)
Cassava Suitability (ζ)	-0.173^{**}	0.041^{***}	-0.042^{***}	0.073^{***}	0.023	-0.063
	(0.068)	(0.012)	(0.011)	(0.020)	(0.060)	(0.049)
Coast Distance	-0.064	0.036^{**}	0.018	0.101^{***}	-0.544^{***}	-0.077
	(0.083)	(0.015)	(0.014)	(0.027)	(0.085)	(0.070)
Overall Suitability	0.187^{**}	-0.007	0.030^{*}	-0.025	-0.010	0.447^{***}
	(0.092)	(0.022)	(0.016)	(0.020)	(0.087)	(0.078)
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	0.548	0.732	0.821	0.725	0.582	0.722
Residual Moran's I (z-score)	-0.353	-0.253	0.089	0.853	0.005	-0.995

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

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 revel 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 further credibility to the main results and underline the validity of the cassava suitability instrument over alternative instrument candidates.

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, Thomas, Taylor 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 proposed slavery pathway ("cassava suitability

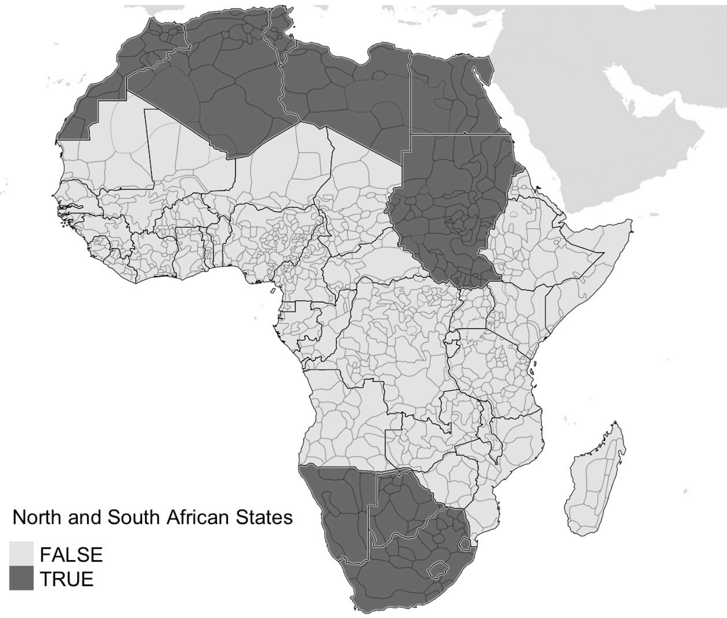


Figure C.4: Country-Group Observations (Not) Falling into North and South African States
Notes: Thin (solid) segments represent group boundaries (international borders as of 2000). Darker shades represent the settlement areas of the placebo country-group observations (groups in North and South African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

→ slave raid → outcome variables") is absent in the subsample, we should not see any systematic reduced-form association between cassava suitability and the outcomes (Davies, Thomas, Taylor et al., 2017, 2070).[7] Systematic instrument-outcome associations in the negative control subsample suggest the presence of unblocked direct pathway linking the instrument and the outcomes, which violates the exclusion restriction and invalidates the proposed IV strategy. 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.1) above. Consistent with the proposed 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.[8] An exception is the

[7]Nunn and 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, Blackwell, and Sen (2016) also employ a similar approach in the context of the slavery legacies in the United States. See Angrist, Lavy, and Schlosser (2010) for a related approach.

[8]Note that the IV estimator can be written as the ratio of the reduced-form instrument-outcome association to the first-stage instrument-treatment association (Angrist and Piscke, 2008, 120–121). A zero

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.

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

reduced-form association thus indicate a zero second-stage association.

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