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STATE BUILDING, DEVELOPMENT AND CONFLICT IN 20TH CENTURY AFRICA

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ABSTRACT

This dissertation examines the geography of state building in 20th century Africa and assesses the effects of local state capacity on development and conflict. I argue that rulers invest in building their states to increase the revenue they extract from society. Trading taxes for public services, rulers' profits are constrained by their bargaining power and transaction costs. They tilt both in their favor by investing in direct or indirect rule over internal competitors and by building transport infrastructure that connects them to their people. To maximize returns, rulers target their investments to local socio-geographic environments. The result is spatial variation in state capacity, which affects local development and conflict.

The first empirical part of the dissertation analyzes African states' investments into local state capacity. To analyze where colonizers increased their bargaining power, I present new data on local direct and indirect rule in French and British colonies. These data show that British governments crushed precolonial polities half as often as their French counterparts, who commanded more administrative resources and mirrored direct governance in metropolitan France. The data also show that the British ruled indirectly over precolonial states, but directly over societies without centralized institutions to co-opt. I then examine where rulers reduced their transaction costs by investing in transport infrastructure. To this end, I gather road network data from British colonial reports (1900–1950) and use machine learning to digitize road maps of post-colonial Africa (1966–2016). Highlighting the extractive roots of local state reach, I find that resource-rich areas have featured more roads and better connections to ports and capitals throughout the century.

The second empirical part examines the effects of indirect rule and local state reach on development and conflict. As indirect colonial rule increased the bargaining power of local populations, indirectly ruled citizens received more public services in return for producing resources taxed by their governments, in particular cash crops. Analyses that exploit variation in indirect rule across neighboring ethnic groups and within groups split by French-British borders bolster this result. Citizens also profit from expanding trade with the government that comes with lower transaction costs. Assessing the impact of local changes in travel times towards administrative capitals, I find generally positive effects on local education, infant mortality, and nightlight emissions. Lastly, transport infrastructure allows governments to keep the peace but can also facilitate rebel mobilization. Drawing on credibly exogenous variation in simulated road networks, I find that roads that connect ethnic groups with national capitals curb conflicts, while roads that connect groups internally increase the risk of violence.

This dissertation adds to the literature by analyzing the extractive roots of local state capacity in Africa, highlighting the interaction between colonial and precolonial institutions, and focusing on the trade between states and their citizens to explain local development and conflict. New data on local state capacity in 20th century Africa enable these contributions.

ZUSAMMENFASSUNG

Diese Dissertation untersucht die Geographie der Staatenbildung im Afrika des 20. Jahrhunderts und ihre Auswirkungen auf lokale Entwicklung und gewaltsame Konflikte. Herrscher investieren in den Aufbau ihrer Staaten, um ihre Einnahmen aus der Besteuerung der Gesellschaft zu erhöhen. Als Gegenleistung für Steuereinnahmen produzieren sie öffentliche Güter und Dienstleistungen für ihre Bürgerinnen und Bürger. Die Tauschgewinne von Herrschern werden dabei von ihrer Verhandlungsmacht und ihren Transaktionskosten bestimmt. Herrscher vermehren ihre Gewinne indem sie alternative Machtzentren in der Gesellschaft unter ihre Kontrolle bringen und ihr Territorium durch Verkehrsinfrastruktur erschliessen. Um die Rendite dieser Investitionen zu maximieren, reagieren Herrscher auf lokale geografische und soziale Bedingungen. Dies führt zu einer ungleichen geographischen Verteilung von Staatskapazität, die wiederum lokale ökonomische Entwicklung und Konflikt beeinflusst.

Um zu untersuchen wo Herrscher ihre Macht über alternative Machtzentren in der Gesellschaft vermehren, präsentiere ich neue Daten zu direkter und indirekter Herrschaft in britischen und französischen Kolonien. Diese zeigen, dass präkoloniale Eliten in britischen Kolonien halb so oft entmachtet wurden wie in französischen Territorien. Hier hatten Kolonialregierungen mehr administrative Ressourcen und folgten zentralistischen Vorbildern in Paris. Britische indirekte Herrschaft wurde jedoch nur dort eingeführt, wo präkoloniale politische Strukturen zentralisiert waren. Gesellschaften ohne kooptierbare Institutionen wurden direkter beherrscht. Eine Analyse von kolonialen Investitionen in Transportnetzwerke zeigt auf, wie Kolonialmächte ihre extraktiven Ziele verfolgten. Strassen und Eisenbahnen wurden überwiegend gebaut, um ressourcenreiche Gebiete mit Häfen und nationalen wie regionalen Hauptstädten zu verbinden.

Der zweite empirische Teil dieser Arbeit untersucht die Effekte von indirekter Herrschaft und dem physischen Wirkungsradius von Staaten auf lokale Entwicklung und Gewalt. Zuerst zeige ich, dass indirekte Kolonialherrschaft lokalen Bevölkerungen mehr Einfluss auf die Verteilung von Ressourcen gab. Dies führte dazu, dass in indirekt beherrschten Gebieten lokaler Ressourcenreichtum einen stärkeren positiven Einfluss auf Bildungsraten hatte, als in direkt beherrschten Gegenden. Zudem profitieren Bürgerinnen und Bürger von niedrigeren Transaktionskosten mit dem Staat. Lokale Bildung, die Gesundheit von Neugeborenen, und nächtliche Lichtemissionen steigen an, wenn regionale und nationale Hauptstädte besser mit der von ihnen beherrschten Bevölkerung verbunden werden. Bessere Strassennetze erlauben es Regierungen auch, Gewalt zu monopolisieren. Sie bieten aber gleichzeitig potenziellen Rebellen die Möglichkeit, lokale Bevölkerungen zu mobilisieren. Die entsprechende Analyse zeigt, dass Strassen, die nationale Hauptstädte mit der Bevölkerung ethnischer Gruppen verbinden, friedensfördernd sind. Im Gegenzug erhöhen Strassen, die periphere ethnische Gruppen intern verbinden, das Gewaltrisiko.

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1

INTRODUCTION

Over the past couple of centuries, the modern state has seen a spectacular rise. About one hundred years ago, state territories covered the entire inhabited landmass of our planet for the first time. In the course of history, some states have amassed more and more power over their citizens, monitoring, policing, and taxing them. In return for taxation, governments promise to keep the peace, enact the law, and provide public goods. Modern states have thereby laid the foundations for the substantive increase in human welfare that came with their rise. But although today's states seem to rule over the entire world population, their capability to govern in many cases lags behind their nominal aspirations. In particular, many African states are too weak to establish a monopoly of violence, secure property rights, and provide basic public services. And even the stronger ones have weak spots, areas in which they are unable to rule. Oftentimes, patterns of development and conflict follow those of uneven statehood on the continent.

This dissertation puts African states front and center and explores the links between their capacity, development, and conflict in the 20^{th} century. Because the historical foundations of state capacity are inextricably related to its effects, I first ask how and where African states were built, and for what reasons. On this basis, I then inquire how African states' capacity affects development and conflict in their societies.

To address this broad research agenda, I develop a theory of extractive state building and test its empirical implications. Rulers profit from extracting revenue from their people and provide peace and public services in return. They build their states as an investment to maximize their profits in the future. In doing so, state builders respond to local factors that determine whether investments into their capacity to extract revenue will pay off or not. The resulting variation in state capacity affects how states and their citizens interact and thereby shape the prospects of local development and peace. I test the theoretical arguments with new, geographically disaggregated data on the modes of governance and territorial reach of African states in the 20^{th} century.

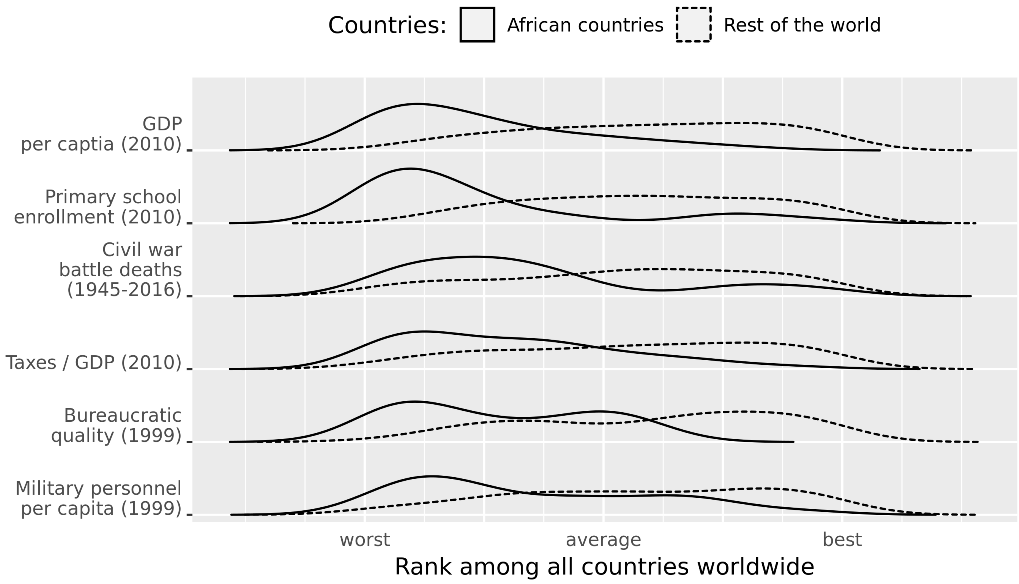
1.1 THE STATE OF STATES IN AFRICA

The United Nation's yearly reports on the progress towards reaching its development goals regularly put Africa, and Sub-Saharan Africa in particular, at the bottom of rankings of global human welfare. While prosperity is improving, many of the continents' inhabitants continue to have inadequate access to education and health services, experience low levels of economic opportunity, and all too often even lack basic nutrition. But 'Africa' is certainly not a country, and even subnational regions exhibit great divergence in development outcomes.

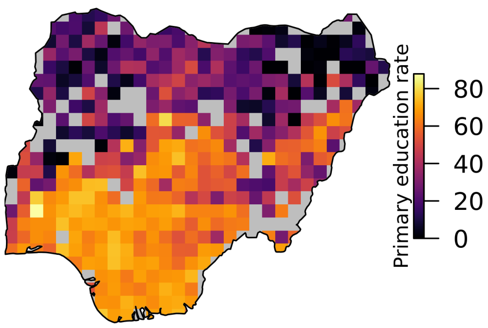
As Figure 1.1 illustrates, the summary statistics presented by the UN and other organizations capture the fact that the median African country is located in the lower half of rankings of GDP per capita and primary education. It also experiences more political violence than most countries in the rest of the world. However, exceptions do exist. Some countries and in particular petroleum producers such as Equatorial Guinea or Angola have a high GDP per capita by global standards. Others, such as Rwanda or Tunisia, have achieved near universal primary school enrollment. Countries such as Benin, Malawi, or Tanzania have remained unaffected by civil wars since their independence, proving oversimplified images of Africa as a violent continent wrong.

Even more variation strikes the eye once we zoom into countries, where development and conflict are typically unevenly distributed. In Nigeria (Figures 1.1b and 1.1c), for example, some regions enjoy comparatively high levels of public service provision and peace, while the country's Northeast is left behind with no access to schools and Boko Haram's insurgents wreaking havoc on people's lives and livelihoods.

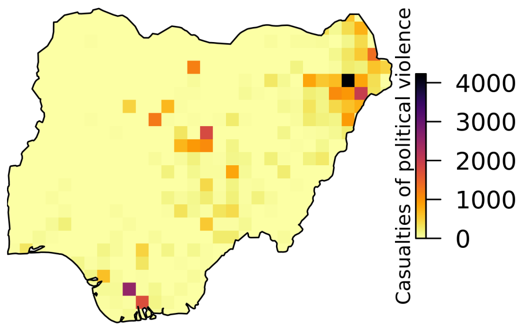
Thomas Hobbes (1651, p. 408) describes the state of nature without a strong Leviathan as a world in which economic development and trade stall and conflicts abound, making "the life of man, solitary, poor, nasty, brutish, and short." In the same vein, policy makers and scholars oftentimes attribute low levels of development and violent conflict to governments' incapability to keep the peace, enforce the law, and provide public services (e.g. Bates, 2001; Evans and Rauch, 1999; Fearon and Laitin, 2003; Herbst, 2000). Indeed, Figure 1.1a shows that the scores of most African states in global rankings of their capacity are in the lower third. Only very few states, one of them being South Africa, collect more taxes, oversee more capable bureaucracies, or command larger armies than the average country outside of Africa. And even strong



(a) Distribution of countries' ranks regarding development, conflict, and state capacity in Africa and the rest of the world



(b) Primary education of Nigerians born after 1980



(c) Conflict fatalities in Nigeria, 1989–2016

Figure 1.1: Development, conflict, and state capacity in Africa.
Note: Data on GDP per capita and education rates come from the World Bank's (2018) World Development Indicators; data on casualties of political violence come from the UCDP Battle-Related Deaths dataset (Pettersson and Eck, 2018); data on tax ratios is provided by ICTD/UNU-WIDER (2018); indicators of bureaucratic quality and military personnel per capita come from Hendrix (2010). Local primary education rates in Nigeria are computed using the Demographic and Health Surveys (2018).

states are not strong throughout their territory. Rather, their capacity varies in space and decreases in their peripheries (e.g. Boone, 2003; Herbst, 2000), thus contributing to uneven patterns of development and conflict.

Why do many African states appear to be less capable of governing, fostering development, and keeping the peace than their peers in other world regions? Why are they more capable in some regions than in others? In search of answers, researchers have followed three main lines of inquiry. First, a burgeoning historical literature finds that many African states are weak because of the precolonial and colonial legacies they inherited (e.g. Acemoglu, Johnson and Robinson, 2001; Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013*b*). This literature blames the continent's mix of weak precolonial political centralization and extractive colonial rule. Both have left rulers of independent Africa with states that lack strong institutional foundations and focus their capacities on a few resource-rich areas. A second strand of research shows that many African states are weak because of their geography. In particular, many states suffer from sparse access to multi-ethnic populations spread thinly over inhospitable territories defined by suboptimal colonial borders (e.g. Englebert, 2000; Herbst, 2000). A third line of argument highlights that African states are weak because they are not subject to lethal interstate competition over territory. Pampered instead with international aid, they have little incentive to grow stronger (e.g. Jackson, 1990).

While illuminating important determinants of (a lack of) state capacity on the continent, the literature exhibits three main shortcomings. First, while it is certainly true that African states' capacity has important historical foundations, the respective research focuses on them in an isolated rather than integrated manner. Doing so often results in compressing African history to two points in time – a moment in the precolonial or colonial past and a second moment in the present – while neglecting the pathway that connects them. This approach thus rushes over important interactions between precolonial and colonial institutions that affected how states were built. Second, the literature's emphasis on the historical, geographic, and international constraints to strong statehood oftentimes overlooks states' internal incentives to grow stronger. Focusing instead on internal drivers of state building can help us understand why and where African states have overcome their historical and geographic hurdles. Third, the lack of comprehensive data on the capacity of African states and its development over time impedes quantitative analyses of state building and its effects. The substantive variation of state capacity calls for spatial and temporal disaggregation of such data. With a theory of extractive state building and spatio-temporally disaggregated data on state capacity in 20$^{\text{th}}$ century Africa, this dissertation addresses these three issues.

1.2 THE ARGUMENT

In search of the roots of local variation in African states' capacity and its effects on development and conflict, I develop a theory of extractive state building. I argue that state leaders rule to extract revenue from their subjects through direct and indirect taxation. In return, their states supply public order, justice, and services such as health care and education. In this trade of taxes for public services, the profits of rulers depend on their bargaining power and the transaction costs they incur when dealing with their citizens (Levi, 1988; North, 1981). State building is the process through which rulers increase their bargaining power and reduce transaction costs to maximize their profits. Similar to Besley and Persson (2009, 2013), I understand state building as a local investment that rulers carry out where the expected returns outweigh its costs.

States' bargaining power vis-à-vis their citizens primarily depends on the existence of alternative sources of political authority within their territory, such as local leaders or, in the case of colonial states,[1] precolonial elites. To diminish the power of such internal competitors for revenue extraction, states can invest to crush and replace them with schemes of direct rule. Alternatively, they can choose to implement less lucrative but cheaper indirect rule. I argue that the choice between the two modes of governance is determined by states' resources and the level of centralization of the competing political structures. Where these are too fragmented and decentralized to be ruled indirectly, direct rule becomes the strategy of choice.

The costs of state-society transactions affect how expensive it is to extract revenue from citizens and to supply them with public services in return. Among the main determinants of transaction costs is the geography of a state. Its terrain and population distribution affect the costs of physical interactions between governments and their people. Where citizens are physically inaccessible, state agents are unable to monitor,

1 Note that the term 'colony' is meant to include the various forms of colonial dominions, that is settler colonies, non-settler colonies, protectorates, and mandated territories such as Togo and Tanganyika.

tax, and service them. To conquer space and reduce transaction cost, rulers invest in transport infrastructure. Because tax potentials are unevenly distributed, such investments primarily target areas with valuable and taxable resources. In colonial and postcolonial Africa, this entailed extending road and rail networks to areas suitable for the production of export commodities at the expense of economically less profitable regions.

The resulting patterns of local direct or indirect rule and the extent and structure of transport networks have important implications for the trade of state revenue for government services, which in turn affects development and conflict in Africa. Where indirect rule prevails, local elites and the populations they control enjoy more bargaining power over the state and get more services in return for the revenue they provide to the state. Where roads bring citizens closer to states' administrations, state-society transactions become more frequent, increasing public service provision and local development. Roads do not, however, only spur transactions between the state and its population, but may also be used by potential rebels to mobilize against the state. Depending on the structure of road networks, this mobilization effect can outweigh the state's ability to police challengers and thus create opportunities for rebellion.

This theory of extractive state building and its effects on development and conflict extends the current literature in four regards. First, its focus on local investments into state building offers a dynamic version of theories of predatory governance, which explains the outcomes of state-society bargains on the basis of relative bargaining powers and transaction costs (e.g. Levi, 1988). Second, by explicitly accounting for the ways in which colonial governments interacted with competing precolonial centers of power, the argument bridges the current gap between the literatures on the long-term effects of precolonial and colonial states in Africa. Third, the theoretical argument incorporates the incentives of states to make their populations accessible through infrastructure investments. It thus avoids the implicit determinism of many studies on the effects of natural geography on state capacity, development, and conflict. Fourth, I move beyond approaches that see state building as a result of international competition between states. Instead, I focus on rulers' rent-seeking behavior as a more general driver of state building.

The theory of extractive state building and its effects on development and conflict is highly stylized and therefore comes with important limitations. The argument's materialistic underpinnings do not intend to discount the importance of, for example, ideologies or international dynamics and their effects on state building and state-society interactions. However, I argue that the argument captures the extractive goals of colonial state builders that shaped the foundation of modern states in Africa. In addition, I brush over complications that may arise from variation in rulers' intertemporal discount rates and their effects on rulers' investment strategies. Lastly, I focus only on the most important interactions between transaction costs and bargaining powers, leaving a broad array of open questions for future research.

1.3 DATA AND EMPIRICAL EVIDENCE

The empirical chapters of this dissertation provide a series of tests of the implications of my theory of extractive state building and its effects on development and conflict. Following the structure of the argument, the first empirical part explores how and where colonial states increased their bargaining power through direct and indirect rule and lowered their transaction costs by investing in transport infrastructure. The second empirical part studies the effects of these two means of state building on development and conflict. As briefly summarized in the following, each empirical analysis draws on new, spatially and temporally disaggregated data that capture local state capacity and its development over time.

Focusing on the state-building efforts of colonial conquerors that laid the foundations of modern states in Africa, the first empirical part explores patterns of direct and indirect rule as well as transport infrastructure investments. To assess where colonial states invested to increase their bargaining power by establishing direct or indirect rule, I collect new data on the survival of precolonial polities and the power of indigenous authorities in British and French colonies. With more administrative resources at their disposal and bound to a centralized metropolitan administration, French colonial governments crushed precolonial polities more often and ruled in a more direct manner than British rulers did. The British, however, could only follow through on their preference for indirect rule where pre-existing political structures were centralized and could be co-opted. In contrast, British colonial governments ruled in a more direct manner over acephalous societies without strong precolonial political hierarchies.

Faced with vast distances between administrative capitals and the populations they had conquered, colonial governments prioritized the construction of transport networks early on. To test whether these efforts to reduce their transaction costs aimed at increasing revenue extraction, I collect two data sets and construct time-varying African road atlases similar to Google maps. The first one covers the first half of the century in eight British colonies. The second data set stems from the Michelin map corpus. Philipp Hunziker and I use a fully convolutional neural network for image segmentation to digitize the corpus that covers the entire continent after 1966. Both data sets provide evidence that transport networks in Africa were built to extract: Areas suitable for the production of exportable cash crops and minerals feature denser road networks, better connections to ports, and swifter access to the administrative capitals of their states. All these factors reduce the costs of revenue extraction.

The second empirical part explores the effects of local state capacity on development and conflict, again focusing on differences between direct and indirect rule and the impact of transport networks. The respective chapters delve into three mechanisms. The first chapter explores how the bargaining power of populations under direct and indirect colonial rule affected the provision of colonial public services in return for taxing resource production, in particular cash crop agriculture. Drawing on variation in indirect rule between French and British as well as within British colonies, I find that indirect rule increased populations' returns on resource taxation. Empirically, I use soils' suitability for cash crop agriculture as an exogenous proxy for real production and reconstruct local colonial education rates from survey data of individuals raised before independence. The data show that indirect colonial rule increased the marginal effect of soil suitability on education. This result holds when I exploit variation in indirect rule in pairs of neighboring ethnic groups and within groups split by French-British colonial borders. Further analyses show that the resulting spatial inequalities in local development persist until the present day.

Turning to the effects of state-society transaction costs, I then analyze whether states' efforts to shorten the distance between their administrative headquarters and citizens increase local development. I measure transaction costs through local travel times towards regional and national capitals. These are calculated with the data on postcolonial road networks coupled with newly collected information on all regional administrative units and their capitals since independence. The analysis assesses the impact of variation in travel times to capitals within the same location and finds generally positive effects on local development, captured by postcolonial education and infant survival rates as well as nightlight emissions since 1992.

However, roads do not only facilitate interactions between the state and society, but can also give the state's internal challengers the opportunity to mobilize and rebel. Based on joint work with Philipp Hunziker and Lars-Erik Cederman, the last empirical chapter tests this prediction. Focusing our analysis on ethnic groups, which oftentimes provide the identity basis for rebellion, we find that the nexus between roads and conflict is a literal two-way street, with the structure of road networks determining their net effect: the incidence of conflict decreases with ethnic groups' accessibility from national capitals but increases with their internal connectedness. We address the potential endogeneity of road networks by employing instrumental variables from simulated road networks that optimally connect countries' populations and are unaffected by politics by design.

The data I use in the aforementioned empirical analyses cover the entire African continent in an irregular manner. Because of data constraints, not all analyses draw on data from each colony and country at every point in time. Rather, pragmatism dominates the choice and limits of data sources. This leads to smaller samples where I analyze patterns of direct and indirect rule in French and British colonies, and samples dictated by the availability of geocoded survey data where I study individual-level outcomes such as education. Table A.1 in the Appendix provides a comprehensive overview over the coverage of individual countries in each chapter. With some chapters[2] drawing on data from the entire continent, I depart from studies that limit their attention to Sub-Sahara Africa (e.g. Herbst, 2000). While North African states certainly have a different precolonial history and operate in the state system of the Middle East, my main arguments travel across the Sahara. Colonizers in Northern Africa, as indeed all around the world, had to deal with varying precolonial political institutions, strove to extract revenue, and certainly had to grapple with geographies that made it difficult to control parts of their population.

2 In particular the analyses of road networks and their effects on development and conflict in Chapters 5, 7 and 8.

The use of observational data to probe the validity of the theoretical arguments raises the specter of inferential problems ranging from biased historical information, over reverse causality, to omitted variable biases. Noting that the historical nature of my inquiry forbids the use of experiments, the above summary has highlighted that I bolster potentially biased baseline results with analyses that rely on credibly exogenous variation across borders, over time, and in simulated data. With the appropriate assumptions and coupled with micro-level data that span Africa in the 20$^{\text{th}}$ century, these strategies lead to "huge" but hopefully not "stupendous" comparisons (Tilly, 1984) that improve our understanding of state building and its effects on development and conflict on the continent.

The broad empirical scope of the dissertation naturally comes with important limitations. First, my empirical focus on the two state-building dimensions consisting of the local mode of rule and transport infrastructure investments disregards many other areas in which states are built. Second, the study is geographically restricted to Africa, leaving open questions about the results' validity beyond the continent. Third, the empirical exploitation of within-colony and -country variation makes it impossible to draw inferences about general equilibrium effects at the macro-level. Fourth, the analyses focus on average impacts of the mechanisms they study and leave many opportunities for future research on heterogeneous effects over time and space.

1.4 OUTLINE OF THE THESIS

The remainder of the dissertation follows the structure outlined above. After reviewing the literature on the determinants of African states' capacity in Chapter 2, Chapter 3 presents the theory of extractive state building and its effects on development and conflict. The first empirical part then examines the drivers of direct and indirect colonial rule (Chapter 4) and of states' efforts to decrease transaction costs through investments in transport infrastructure (Chapter 5). The second empirical part focuses on the effects of state capacity on development and conflict. Chapter 6 studies how indirect colonial rule affected the provision of public services in return for resource extraction, and Chapters 7 and 8 assess the impact of state reach on development and conflict. These last two chapters' findings on the impact of distances to national capitals on local development and conflict lead me to divert on a brief excursus.

Through an analysis of counterfactual national geographies, I quantify Herbst's (2000) argument that peripheral, densely populated regions could have fared better with more 'efficiently' placed colonial borders and capitals. The conclusion finally highlights the key findings of the dissertation and discusses their implications for policy makers and the study of state building and its effects in Africa and beyond.

The reader will note that I have relegated some technical discussions into boxes. This is not to discount their importance, but to improve the readability of the text without banning interesting insights and techniques into the appendix. Many other technicalities have received less mercy. The appendices present technical details of the various data collection and preparation processes as well as extensive discussions of additional analyses that the main text only briefly summarizes.

2

LITERATURE

States in Africa have a miserable reputation. They are weak and suspended in mid-air (Hydén, 1983), nothing more than "quasi-states" (Jackson, 1990) that have not attained full internal sovereignty. Weak states are unable to enforce law and order and incapable of providing public goods, making them one of the most important causes of low levels of development and conflict (e.g. Bates, 2001; Evans and Rauch, 1999; Fearon and Laitin, 2003; Herbst, 2000).

Why are some states in Africa weak? Over the past decades, a large, multi-disciplinary literature has tackled the issue from three directions. A first thread of research explores the roots of states' capacity in history, highlighting path dependencies that reach back to the precolonial and colonial past of the continent. A second strand of the literature has taken on studying the effects of geography, arguing that the territory and natural environment of many states in Africa make them inherently difficult to govern. A third prominent research agenda has focused on interstate relations in Africa. They are held to be too friendly to give rise to international competition over power and territory that stimulated state building elsewhere on the globe.

Summarizing the wealth of knowledge from this big body of work on states in Africa, the following literature review identifies three main theoretical and empirical gaps this dissertation seeks to address. First, the historical literature tends to compress history to the point of neglecting the pathways on which African states developed, pathways marked by strong interactions between precolonial, colonial, and postcolonial factors. Second, the literature on the various roots of state capacity in Africa disregard the incentives of rent-seeking rulers to overcome the constraints they face through investments into their states' capacity. Third, current quantitative research faces a lack of spatially and temporally disaggregated data on the development of African states' capacity over the long run. With this focus on the origins of state capacity in Africa, the literature review only briefly touches on research on the former's effects on development and conflict. I discuss the relevant works in the respective empirical chapters.

2.1 STATES' HISTORICAL LEGACIES

African states' capacity has been shaped by the historical legacies they inherited from their colonial and precolonial predecessors. Historical inquiries on African politico-economic development have a tradition (e.g. Mamdani, 1996; Young, 1994) that has flourished in the wake of Acemoglu, Johnson and Robinson's (2001; 2002) seminal studies on the colonial origins of contemporary development. The current 'renaissance' of historical studies has particularly focused on the long-term impacts of precolonial and colonial institutions.[1]

This burgeoning literature suggests that precolonial state institutions affect state capacity, public service provision, and development until today. Englebert (2000) argues that precolonial statehood has an enduring impact on states' legitimacy and capacity in particular where postcolonial states largely overlap with their undestroyed precolonial predecessors. Similarly, Gennaioli and Rainer (2007) show that countries with a larger population from ethnic groups that had precolonially centralized political structures profit from better infrastructure, health care, and education. Michalopoulos and Papaioannou (2013*b*) present similar, but not undisputed (Cogneau and Dupraz, 2014), findings at the level of ethnic groups, taking nightlight luminosity as a broad 'catch-all' measure for local development. Green and Bandyopadhyay (2016) focus on private wealth levels in Uganda and report similar findings. All of these strongly resonate with Bockstette, Chanda and Putterman (2002), who argue that state antiquity is associated with higher levels of political stability, institutional quality, and economic development across the globe (see also Putterman and Weil, 2010; Borcan, Olsson and Putterman, 2018).[2]

In a similar vein, a large body of research explores the origins of low levels of state capacity and development in colonial rule, mostly in the period after the 'Scramble for Africa' starting 1885. Colonial rule was extractive and oftentimes indirect, two factors that set the ground for low levels of state capacity today.

1 For a comprehensive review, see Michalopoulos and Papaioannou (2018).
2 For similar evidence, see Dell, Lane and Querubin (2018) on Vietnam and Angeles and Elizalde (2017) on South America.

Most colonial states in Sub-Saharan Africa had, so the first line of argument goes, a mainly extractive purpose. Too disease-ridden to be settled by European emigrants (Acemoglu, Johnson and Robinson, 2001), colonial states on the continent primarily followed the interests of their metropolises, which prioritized primary resource extraction demanded by growing industries. Thus, colonial "gatekeeper" states (Cooper, 2002) controlled the ports, but not their hinterlands, which they had no interest in developing. However, gatekeepers had nothing to extract if they did not foster production in the resource-rich areas of the colonies. Strategies to do so sometimes consisted in selling concessions that led to highly extractive regimes in private hands with negative developmental consequences observed in the Congo Free State (Lowes and Montero, 2018). Colonial governments also devised less brutal policies to induce cash crop production by smallholders through building economic infrastructures, research centers, and distributing seeds. On average, such production as well as the extraction of minerals, is associated with positive long-term effects on local development across Sub-Saharan Africa (Roessler et al., 2018).[3] These diverging developmental effects of 'good', 'bad', and 'ugly' colonial extraction are in line with research on the impact of colonialism in the Americas (Bruhn and Gallego, 2012).

A second strand of inquiries on the effects of colonial legacies focuses on the consequences of direct and indirect rule on state capacity, development, and conflict. Facing a lack of personnel (Kirk-Greene, 1980), colonial governments oftentimes relied on regional and local intermediaries to attain control over their nominal territories. Two debates on indirect rule prevail. One camp argues that few differences existed in its application across empires (Herbst, 2000; Gerring et al., 2011; Mamdani, 1996), while another notes substantial variation, highlighting in particular differences between the French and British empires (Asiwaju, 1970; Crowder, 1968; Miles, 1994). In addition, arguments about the effects of indirect rule on state capacity and development diverge. Mamdani (1996) famously argues that policies of indirect colonial rule empowered local elites to become untamed 'decentralized despots'. As a consequence, indirect rule through uncontested local elites promoted bad postcolonial governance, corruption and conflict (Acemoglu, Reed and Robinson, 2014; Blanton, Mason and Athow, 2001; Lange, 2004, 2009).[4] Others argue the opposite. In particular, Iyer (2010) finds that

3 Dell and Olken (2018) find similar positive effects of the colonial sugar industry in Java.
4 More recently, Lechler and McNamee (2017) find that indirect rule in Namibia is associated with worse democratic attitudes and lower turnout today.

indirect rule of so-called "Princely States" in India led to more responsive governance after independence. This coincides with the literature on precolonial institutions (Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013*b*), which argues that colonial indirect rule over centralized precolonial states constitutes the main channel of their enduring positive impact.

The 'historical renaissance' in quantitative research on Africa has added much to our knowledge on the impact of the historical legacies inherited by African states. Notwithstanding the substantive scientific progress of the past decades, two challenges stand out. First, the current literature almost exclusively examines the impact of either colonial or precolonial factors on today's socio-economic and political outcomes. By examining the relationship between data from two points in time and neglecting the socio-political process that connects them, the modal study compresses historical processes beyond recognition (Austin, 2008). This is not only a matter of depicting historical developments accurately. More importantly, knowing about the paths that connect the past to the present is crucial for our understanding of the mechanisms that drive the effects uncovered by the literature. This limitation affects in particular studies on the impact of precolonial institutions and colonial modes of rule. Despite historians' emphasis of variation in colonial empires' treatment of precolonial institutions (e.g. Crowder, 1968), we currently lack systematic evidence about how different colonizers ruled with or against the pre-existing institutions they had conquered. Going beyond arguments of mere path dependence, a focus on historical pathways highlights the diversity of interactions between the state and non-state elites that are at the heart of processes of state building.

Second and related is the scarcity of comparative data on colonial state building in Africa. More and more scholars have digitized colonial statistics – in particular on finances (Frankema and Waijenburg, 2013; Frankema and van Waijenburg, 2014; Frankema, 2011; Xu, 2018), education (Jedwab, zu Selhausen and Moradi, 2018; Ricart-Huguet, 2016), and trade (Tadei, 2013). However, geographically disaggregated data on colonial states' strategies of local rule are rare (see Huillery, 2009 for an exception) and not comparable across empires. As a number of ongoing research projects have recognized,[5] incomplete colonial data severely limit our understanding

5 In particular the collection of late-colonial cash crop and mineral production data by Roessler et al. (2018).

of colonialism in general and extractive colonial state building and indirect rule in particular. Both processes are local phenomena best studied with disaggregated data.

2.2 STATES' GEOGRAPHY

A second influential school of thought argues that African states are weak because of their geographic design. The most prominent features of natural and human geography that have been linked to state weakness consist in the natural environment of states, the design of their borders, as well as the ethnic diversity of the population they rule over.

First, the natural environment can impede the operation of states. In particular, rough and inaccessible terrain can hinder state control because of difficulties for the state to interact with and control its population. While Nunn and Puga (2012) argue that rugged terrain has been a blessing in Africa because it provided shelter against slave raids, there is substantive evidence that locations farther away and inaccessible from states' capitals experience more conflict (Buhaug and Rød, 2006; Raleigh and Hegre, 2009; Tollefsen and Buhaug, 2015) and feature lower levels of development (Henn, 2018; Krishna and Schober, 2014). These patterns are not unique to Africa. However, here they are compounded with very low population densities due to hostile climatic conditions and endemic diseases (Herbst, 2000). Difficult-to-control and dispersed populations might have indeed been the reason for why centralized states have been common only in few areas of the continent before colonization (Bates, 1983, ch. 2), a claim that Osafo-Kwaako and Robinson (2013) contest.

Second, state borders drawn during the colonial 'Scramble' for Africa exaccerbate the natural impediments to state rule on the continent. Herbst (2000) argues in this regard that some African states have inherited colonial borders that enclose dispersed populations with local concentrations far away from states' capitals. In between, large and sparsely inhabited regions make it almost impossible for states to achieve state control. In a similar vein, Green (2012) shows that precolonial population density is a key predictor of African state's size: less populous regions such as the Sahara desert, the Sahel, or the Congo River basin are governed by larger states. States with difficult-to-control populations have not only inherited less capacity from their colonial predecessors. They have also invested the least in transport infrastructure that makes their populations accessible and did not increase their capacity through citizenship or monetary reforms (Herbst, 2000; Thies, 2009). Similarly, Alesina, Easterly and Matuszeski (2011) and Michalopoulos and Papaioannou (2016) highlight the impact of "artificial" borders on development and conflict, and report that colonial borders that cut through ethnic groups have impeded development and increased the risk of civil war.

Third and partly as a result of colonial borders that do not align with ethnic geographies, most African states face high levels of ethnic diversity in their population (Englebert, 2000). Such diversity is generally associated with heterogeneous interests of the population and inefficient communication, both of which may reduce states' capacity for central control, policy-making, and public goods provision (e.g. Easterly and Levine, 1997; Alesina, Baqir and Easterly, 1999). In more concrete terms, ethnically diverse countries oftentimes feature political cleavages organized along ethnic identities. When these are translated into unequal access to political power, governments tend to favor their ethno-regional constituents (e.g. Burgess et al., 2015; Franck and Rainer, 2012; Hodler and Raschky, 2014) and fighting for power may become a viable strategy of the disempowered and -advantaged (Cederman, Gleditsch and Buhaug, 2013; Cederman, Weidmann and Gleditsch, 2011; Wucherpfennig, Hunziker and Cederman, 2016).

While highly relevant for exhibiting the constraints to state capacity in many regions of the continent, explanations rooted in states' natural and human geography exhibit two main shortcomings. First, they neglect states' ability to mitigate the challenges they face. To address the difficulties posed by their geography, states invest in infrastructure that makes territory and people accessible. States can also design their administrative geography such that headquarters are located where the people live and incentivize dispersed citizens to move towards administrative centers and cities. Similarly, social engineers might attempt to reduce ethnic diversity, for example through centralized education systems that foster a common language and national identity (Gellner, 1983; Miguel, 2004; Bandyopadhyay and Green, 2013). In fact, activities that mitigate the constraints to centralized rule constitute the main core of what state building is about. States that face a spread-out, multi-ethnic population and an inaccessible terrain can grow reasonably strong and stable, as the United States and Switzerland have demonstrated.

The inability of the geographical fundamentals of states to explain *processes* of state building is rooted in a second weakness of the respective literature. Mainly focusing on the impediments to and costs of state rule, it sheds no light on the benefits and thus motivations for strengthening states' capabilities. With this, the literature offers few insights to where we would expect states to overcome the hurdles they face, and where they would not attempt to do so. The latter category of places is of course of greatest practical importance, since it is here where we expect the population to be neglected by their state and potentially in need of outside assistance.

2.3 STATES' ENVIRONMENT

The international environment constitutes the third factor singled out as a culprit for African states' comparative weakness. Building on Tilly's (1975) influential hypothesis that states make war and war makes the state, Herbst (2000) argues that the stability of African borders disincentivized rulers from building capable states. Whereas European rulers had to fear each other for centuries,[6] the 'Addis rule'[7] has cristalized an international consensus that African borders cannot be changed. This relieves states and their leaders from the fear of conquest and reduces their incentives to invest in their capacity to survive. The non-competitive international environment thus secures states' juridical statehood but does not require empirical statehood. It lets "quasi-states" (Jackson, 1990) survive, nourishing them with strategic assistance and development aid where need be (Bates, 2001, p. 82).[8]

While offering an appealing perspective on the impact of the international environment on state building, this account has two main shortcomings. First, its empirical basis is relatively weak. Thies (2007) notes that there are indeed quite some international rivalries, although the number is lower than in other world regions. Consistent

6 On war and state building in Europe, see Tilly (1985, 1990); Besley and Persson (2010), Dincecco and Prado (2012), and Gennaioli and Voth (2015). For similar evidence on the bellicist theory from Latin America, see Thies (2005) and Centeno (2002) who argue similar to Herbst (2000) that limited war-making in Latin America contributed to weak statehood in the region.

7 Referring to the Charter of the Organisation of African Unity, the predecessors of the African Union, signed in Addis Ababa in 1963.

8 There is a considerable literature on the effects of foreign aid on governments' tax effort which is generally taken as one indicator of state capacity (see Hendrix, 2010), with the main debate focusing on whether and which types of aid reduce government efforts to collect revenue, or strengthen their institutional capacities to do so. See, for example, Bräutigam and Knack (2004), Goldsmith (2001), Moore (2004), Moss, Pettersson and van de Walle (2006), and Morrissey, Prichard and Torrance (2014).

with Tilly's original argument, Thies (2007) shows that these rivalries are associated with greater efforts of states to collect taxes. Similarly, Dincecco, Fenske and Onorato (2015) report that high levels of historical conflict are associated with more fiscal capacity of African states today, but at the same time also come with more contemporary civil conflict. Beyond this empirical evidence, there are major theoretical concerns that weaken Herbst's argument. In particular, it is unclear whether most states in Africa are simply (still) too weak to violently challenge each other on a large scale. States that cannot make full use of their own territory will have few incentives and resources to invade that of their neighbors (see also Centeno, 2002). Thus, weak states such as those in the Sahel zone are rather challenged by internal rebels than by international rivals. Such internal rivalries might pressure rulers to build stronger states just as external ones (e.g. Thies, 2004, 2007), while at the same time impeding state building in regions affected by civil wars. Lastly, even very secure rulers may want to increase their own revenue and thus invest in their states' capacity.

2.4 SUMMARY

This brief review of the literature on the origins of African states' capacity has shown that we know quite a bit about states' constraints that originate in their geography, history, and international environment. At the same time, the review has raised three major, unresolved issues.

First and with regard to the historical legacies of today's states, there is a need for better understanding the historical processes through which states have reshaped the political structures they have inherited from their colonial and precolonial predecessors. In particular, there is only limited quantitative evidence on the extent to which colonial states incorporated precolonial institutions in schemes of indirect rule and where they have replaced them with their own institutions.

Second, and beyond knowing that geography posed and still poses a significant challenge to colonial and postcolonial states, we know little about states' actions to reduce these barriers. European and colonial history teaches us that states incur heavy investments to make populations and territories governable – but why do they do so and where do they allocate their efforts?

The third major gap in the literature relates to the first two and consists in the lack of historical data on African states' capacity, their interactions with non-state centers of power, and their efforts to mitigate the historical and geographic hurdles they face. Because state capacity is a local phenomenon and exhibits substantial variation within countries (e.g. Boone, 2003; Buhaug and Rød, 2006; Lee and Zhang, 2017; Soifer and vom Hau, 2008), such data should be spatially disaggregated. In order to allow the investigation of state *building*, it must also capture variation over time.

On the level of theory, this dissertation addresses these gaps by developing a model of state building that incorporates not only the constraints to states' rule, but also the incentives of state builders to invest into their capacities. Building on theories of predatory governance and going beyond the arguments that see competition between states as the main driver for state building, I argue that rulers build their states to maximize their profits. In particular, rulers invest into increasing their bargaining power over society by pushing aside internal competitors and reduce the transaction costs they face by making people and territory accessible through transport infrastructure. To be able to test the theory's empirical implications, I present detailed data on direct and indirect rule in British and French colonies as well as time-variant geographical data on African transport networks in the 20$^{\text{th}}$ century.

Combined with historical and geographically disaggregated data, the theoretical model of state building does not only allow for better depicting how states were built in Africa. It does also lead to an improved theoretical and empirical understanding of how states' capacity affects development and conflict on the continent. By highlighting the power and transaction costs of states when bargaining with society over resources, the argument lets us trace the local developmental effects of states' efforts to extract revenue. In addition, by focusing on the state and its internal challengers, it opens a new perspective on how states' control over its population relative to that of its competitors shape opportunities for political violence.

3

A THEORY OF EXTRACTIVE STATE BUILDING

A state is a social organization used by its rulers to extract revenue from its territory and population. To do so, the state and its leaders have to bargain with society over the resources it produces and the services the state provides in return. In this tug-of-war, the rulers aim to maximize their profits by extracting the most revenue at the lowest price, that is, the lowest level of service provision. Rulers' relative bargaining power and the costs of transactions with their people determine the profits they can make. State building tilts both factors in their favor.

Because challengers to state rule and resources to extract are distributed unevenly in space, rulers' incentives to invest in state building vary geographically. Rulers respond to the cost of confronting their challengers and augmenting their bargaining power by choosing between direct or more indirect forms of local rule. And they focus their investments to lower transaction costs on those areas from which they can extract most revenue, in particular by making these areas accessible through transport infrastructure. The resulting geographic variation in bargaining powers and transaction costs subsequently shapes the terms and extent of trade between the state and local populations. Both affect local development through the public services provided in return for taxation. In addition, reductions in transaction costs through transport networks foster a state's ability to keep the peace but may also increase the ability of potential rebels to mobilize and challenge the state.

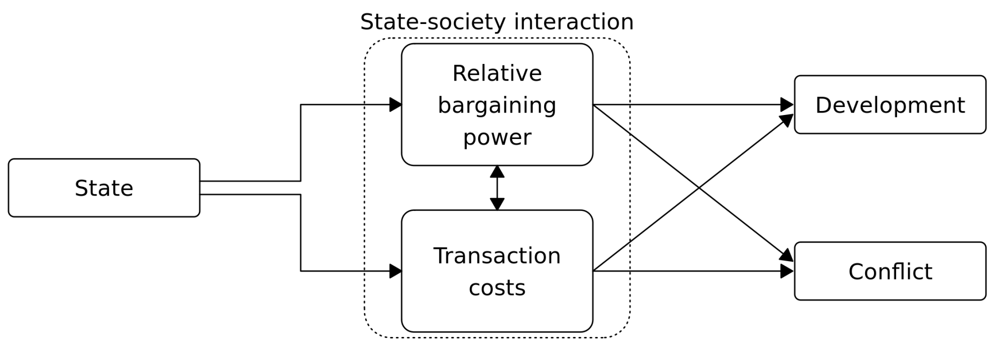


Figure 3.1: Structure of the argument

The theory of extractive state building summarized in this nutshell and structurally mapped in Figure 3.1 builds on two seminal approaches to state-society interactions and state building. The first consists in Levi's (1988) theory *Of Rule and Revenue* according to which rulers trade public goods and services for tax revenue and are constrained by their bargaining power and transaction costs. To gain analytical leverage over dynamic state building that aims to change the bargaining powers and transaction costs of rulers, the second building block of the theory parallels Besley and Persson's (2009; 2013) insight that governments invest to increase their fiscal capacity when the cost of doing so is below the expected increase in revenue. Combining this reasoning with Levi's focus on rulers' constraints, I argue that rulers invest to increase their bargaining power by either replacing or co-opting their internal opponents and to decrease transaction costs by making their people accessible.

Inspired by historical accounts of state building in Africa and Europe, the argument and its empirical application captures a simplified and ideal-type logic of how rulers build their states. This comes with three important limitations. First, the theoretical argument for the most part treats investments to increase bargaining power and decrease transaction costs in relative isolation. While I acknowledge that there are many potential interactions between both processes, I only highlight what I consider the most important complications. Second, I neglect the role of rulers' discount rates, assuming that these are low enough to make them behave as 'stationary' that invest rather than 'roving bandits' that only predate (Olson, 1993).

Third, the idealized character of the theoretical argument does not and should not imply that state building always and exclusively follows extractive purposes. On the contrary, international dynamics (Tilly, 1985, 1990), ideological factors such as nationalism (Hechter, 2000), and political institutions also affect how states develop. Without discounting their importance, the focus on material drivers of states' efforts to grow stronger aims to keep the theory derived from first principles that give rise to testable empirical implications. In addition, the extractive logic that powers the argument seems particularly well suited to explain state building in Africa, in particular during the colonial period. With few exceptions, the foundations of modern states on the continent were laid by colonial conquerors that primarily followed material interests.

With these limitations and scope conditions in mind, I will provide the flesh to the argument on how rulers build their states to maximize extraction. I first outline the setting of the argument, taking note of the characteristics of states as social organizations and states' leaders as revenue-maximizing individuals. The discussion then focuses on rulers' bargaining power and the transaction costs they face when dealing with society. I discuss both constraints as well as rulers' strategies that mitigate them by establishing direct and indirect forms of rule and investing in transport infrastructure. I then derive the effects of these two dimensions of state building on development and conflict. While this chapter outlines the general thrust of the argument, the related empirical chapters delve deeper into the respective mechanisms and provide quantitative empirical evidence of their workings.

3.1 THE SETTING: STATES AND EXTRACTION

3.1.1 *Properties of the state*

Any analysis of states, their evolution and effects on societies, has to start from a definition of what a state actually is. The definition of a state must not rely on properties that vary over time, such as its monopoly of violence or ability to collect taxes. If it did, we would risk losing empirical track of states as they develop and change in how well they fit the definition. It is therefore useful to differentiate mostly stable and institutional properties of states from the varying functional ones (Mann, 1984). When Weber (1919, p. 9) defines the state as an organization with a monopoly of violence over a distinct territory, he draws on the institutional aspect of the state as a territorial organization and its function, the monopolization of violence. Most seminal definitions of the state rely on a mix of institutional and functional properties (e.g. North, 1981; Levi, 1988; Mann, 1984). As we will see in the following, institutional attributes are the husk of states and differentiate modern states from non-state organizations. Functional attributes are at their core. They constitute the dimension in which states are being "built" and evolve over time.

The institution of modern statehood is characterized by three attributes.[1] First, a state is a centralized organization, typically headed by a ruler or a collective of

1 Note that the institution of modern statehood is not the same as states' institutions that determine states' functioning.

leaders. They are the main principals of the hierarchical state administration with its functional arms that extend from the state's capital, via the regional down to the local level. The second attribute is the state's territory, delineated by borders within which the state claims to rule (Mann, 1984). Lastly, modern states are sovereign actors in the international system of states. As special cases, colonial states are members of that system through the sovereignty of their colonizer (Russet, Singer and Small, 1968). With these institutional characteristics, I treat states as actors that are best personified by the elites that head the state organization and have most influence over and draw most benefit from the state's activities (Mann, 1984).[2]

Beyond the three institutional attributes of states, most definitions of states draw on functional aspects of states. While Weber (1977) defines states as claiming the "monopoly of legitimate physical violence", Mann (1984, p. 188) goes further and highlights their "monopoly of *authoritative binding rule-making*" as a defining element of states. In a somewhat different vein, North (1981, p. 21) lets the territory of a state be determined by their "power to tax constituents." A cursory look at the history of many states makes clear that most states have not fulfilled these criteria most of the time. Rather, states' degree of "stateness" (Nettl, 1968) varies over time and within their nominal territory. Because I aim to explain the origins and effects of that variation, the definition of states used in the following only draws on their core institutional features.

States' degree of functional stateness is typically conceptualized and measured in terms of their capacity. As an inherently multi-dimensional concept, state capacity consists of a state's military capability, its bureaucratic and administrative capacity, and the quality and coherence of its political institutions (Hendrix, 2010). On a more fundamental level and cutting across these dimensions, states' capacity depends on their level of social control. For Migdal (1988, p. 22), "[s]tate social control involves the successful subordination of people's own inclinations of social behavior or behavior sought by other social organizations in favor of the behavior prescribed by state rules." As such, social control is equivalent to Mann's (1984, p. 189) concept of infrastructural power, "the capacity of the state to actually penetrate civil society, and to implement logistically political decisions throughout the realm."

2 While I am fully aware of interactions among members of the state organization, the argument for the most part abstracts from such intra-state dynamics and focuses on the state organization as the main tool at the disposal of rulers.

State capacity so broadly defined determines what a state can actually do. It sets the limits to rulers' capability to extract revenue and implement policies. More fundamentally, it determines their ability to shape and enforce the broader institutions that govern the behavior of the state, citizens, and the interactions between them. State capacity is therefore a main determinant of the extent to which it is possible to enforce state institutions, be they considered 'good' or 'bad' (e.g. Huntington, 1968; Levitsky and Murillo, 2009).

With their success depending on their capacity, states have historically used a wide variety of tools to expand their social control. They have increased their power over society by crushing or co-opting internal competitors (Hechter, 1975, 2000). They have established specialized sub-organizations to carry out the varying tasks needed to keep the peace, punish offenders, and tax their citizens. Modern states have taught their people how to read orders and have standardized currencies and weights to facilitate taxation (North, 1981, p. 26). And they have built transportation and communication infrastructures that shrink the space between their agents and citizens (e.g. Mann, 1984; Weber, 1977).

However, most states have not attained the capabilities of an all-powerful Leviathan and even those considered 'strong' exhibit spots of weakness (Soifer and vom Hau, 2008). The next pages inquire why some states in some areas attain the ideal of a well-functioning and strong core that lets them monopolize violence, enforce institutions, tax citizens, and provide public services, and why other regions are left with weak statehood.

3.1.2 *States, rulers, and extraction*

Why and where do states grow strong? Why and where do state leaders invest to strengthen their agencies, to foster education and standardization, to expand physical infrastructure? I argue that the rent-seeking behavior of state leaders drives the expansion of states' means of social control. Using the state to profit from its population, rulers invest to augment their capacity to extract revenue wherever the benefits of additional extraction outweigh investment costs.

Rulers aim to maximize the profits of revenue extraction from society. But they cannot prey on society as they wish. Rather, they have to trade the extracted revenue for benefits they provide to the population (Timmons, 2005).[3] These benefits consist in services for which the state has comparative production advantages, most importantly the keeping of peace, the enforcement of property rights, and the provision of public services (Levi, 1988; North, 1981). The terms of trade rulers can achieve are constrained by their bargaining power relative to the population and competing elites. The transaction costs of the process of bargaining and implementing a bargain determine which of the possible bargains will be ultimately struck. Using the language from above, a state's social control is determined by its bargaining power and the transaction costs it faces when dealing with society. Both constrain the predatory instincts of its leadership.

The relative bargaining powers of the state and society are functions of the degree to which both depend on each other's actions. Military, economic, and political resources at the disposal of each actor can create such mutual dependence (Levi, 1988). With regard to military resources, the state is typically the "specialist in violence" (Bates, Greif and Singh, 2002) and the most powerful actor in society (Mann, 1984). However, non-state actors can overcome collective action problems and mobilize the population against the state. Economic resources are most often concentrated in the hands of economic elites and citizens and demanded by the state. But states wield economic influence as well, through macroeconomic policy and by controlling vital infrastructure and services (e.g. Bates, 1981). Lastly, the political power of each actor depends on the political institutions that hold state elites in power. While, for example, colonial governors were appointed independently of the preferences of the population they ruled, citizens wield more political power over their rulers where these are elected democratically. These examples are far from exhaustive. With the multitude of potential sources of power, the relative bargaining power that affects the distribution of the gains in each bargain between the state and society are highly context-specific.

Besides the state's bargaining power, the transaction costs associated with state-society bargains constrain its ability to extract revenues from the population (Levi, 1988; North, 1981). Transaction costs mainly arise in three domains. First, there is the cost of bargaining between the state and society. Here, costs ensue from the parties' gathering of information about the preferences of each other and the implementation

3 For recent experimental evidence on how taxation sparks public engagement with and demands towards the state, see Weigel (2019).

of the bargaining process. Dubbed "political transaction costs" by North (1990), such costs are dependent on formal and informal institutions in which bargaining over policies takes place. While formal institutions such as parliaments create rather efficient markets with low transaction costs (North, 1990), informal bargaining between the state and society often works through more costly public discourses, political mobilization, and, in some cases, violent conflict.[4] Beyond the costs of bargaining, transaction costs arise from the need to monitor the partners of the bargain and to enforce it in case of misbehavior (Levi, 1988, p. 28). While monitoring requires information about the behavior of individuals and the state, implementing requires the need to police this behavior, if necessary by force, and give the population access to the public services produced in return for revenue.

Faced with the constraints imposed by their relative bargaining power and the costs of transacting with society, rulers, as indeed all other political entrepreneurs, may proactively invest to increase their gains from extraction (e.g. Besley and Persson, 2009, 2013). Investments into their social control enables rulers to forego consumption today to increase extraction and potential consumption tomorrow. In the stylized setting of this argument, it is this promise of future revenue that motivates them to wage war against competitors, set up administrations, and build physical infrastructure. Many such investments into social control are expensive in the short run, but yield long-term benefits.[5]

A focus on states' investment into their extractive capacity lets us derive predictions on how and where rulers attempt to build their states. Investment into social control are expensive, which is why rulers realize them when their costs are lower than the future rents they promise and if they fit into the current budget. Because the potential for extraction varies in the economic and geographic landscape, revenue-maximizing rulers will focus their investments on the most profitable corners of society. Budget constraints, in particular in the early days of a state, will cause state building to

4 For a comprehensive treatment and country studies of processes of tax bargaining, see Prichard (2015).

5 There is an interesting parallel between the investment behavior of firms and states. Just as states, firms' profits from trading with their customers are determined by their bargaining (i.e. market) power and transaction costs. Firms invest to increase their bargaining power by acquiring competitors. They reduce transaction costs through technological innovations and, in the retail business, increasing market penetration with new stores. The reader may note similarities to the investment behavior of states discussed over the following pages.

proceed slowly. Temporally and geographically uneven development of statehood results.

The theory of predatory rulers so presented bears important similarities and differences to Levi's seminal work "Of rule and revenue" (1988). In particular, it adopts her argument that rulers are revenue maximizers constrained by their relative bargaining power and transaction costs (see also North, 1981).[6] However, Levi's static argument does not accommodate rulers (and other social actors) who aim to change the parameters that govern exchanges between the state and society. Explicit accounting for the possibility of rulers to invest can overcome this limitation. The theory of predatory state *building* can then explain why and where many states grow stronger over time.[7]

With its emphasis on investments into state capacity, the argument parallels to some degree one mechanism in Besley and Persson's (2009; 2013) seminal treatment of the reciprocal relations between state capacity, development, and conflict. As one thread of their framework, they argue that governments invest into their fiscal capacity to increase citizens' costs of avoiding taxation. Marginal investments into fiscal capacity are conducted up to the point where they equal governments' marginal gain from increased taxation. I build on this logic and apply it more specifically to the spatial allocation of governments' investments into increasing their bargaining power and decreasing the transaction costs they face.

In comparison to Levi (1988) and Besley and Persson (2009, 2013) and for the sake of simplicity, my theory of extractive state building leaves aside the impact of ruler's discount rates. While I acknowledge that high discount rates lead rulers to invest less, I assume that their discount rates are low enough to invest at all, making them behave as stationary rather than roving bandits (Olson, 1993). I furthermore assume that differences in central rulers' discount rates below this minimal threshold

6 Note that Levi also develops the idea that rulers can increase their revenue through inducing "semi-voluntary compliance" in citizens, which reduces the necessity of monitoring and policing them. The following argument does not depend on such dynamics.

7 Though outside the bounds of her theoretical argument, Levi is certainly aware that rulers can invest into their extractive capacity. As she notes in 1981 (p. 461), "to acquire new sources of revenue usually means a commitment of funds to scientific and market research or a commitment of military resources to campaigns of exploration of conquest. Such policies involve the ruler in risks that may or may not pay off, but the returns can be very high indeed. A ruler can also improve the means of measurement and monitoring, which would enable him to increase the tax on property already within his jurisdiction." However, she does not further engage in how this possibility to invest changes rulers' policy choices.

create variation in their overall level of investment into state capacity, but not in its distribution within the state's territory.[8]

Rulers not only make investments to increase their absolute future revenue, but also to prevent it from falling, or in the worst case, from running dry completely. This is where this predatory theory of state building converges with the argument that states grow because of external (or internal) threats. Most pointedly in this regard, Tilly (1975, p. 42) argues that "War made the state, and states made war." From the perspective of the revenue-maximizing ruler, external and internal threats endanger their current wealth and future revenue and therefore demand investment into their capacity to survive (e.g. Dincecco and Prado, 2012; Tilly, 1990). Of course, the possibility to wage war against a neighbor can also promise future riches, which in turn justify investments into a state's war-making capabilities.

Based on the assumption of revenue-maximizing behavior of rulers as the main driver of state building might seem far-fetched for some historical episodes where ideological motives of rulers might have prevailed. The critical reader may however note that I am not arguing that rent-seeking behavior is the *only* driver of state building. But even the fiercest ideologues need resources to implement his ideal of a state, and more revenues allow them to do so faster (Levi, 1988).

More importantly, a theory of predatory governance seems particularly well suited to analyze patterns of state building in (colonial) Africa. While ideological factors might have had some influence on how African states were built, most authors agree that the colonial states were mainly geared towards resource extraction, an orientation their postcolonial successors inherited (Cooper, 2002; Mamdani, 1996; van de Walle, 2009; Young, 1994). Furthermore, with state borders defined by the colonial powers and not changed much ever since (Herbst, 2000), state building in Africa during the colonial and postcolonial eras was primarily driven by processes internal to states. An additional, mostly empirical advantage of focusing on Africa is that colonial powers brought the state to the continent, so that we can distinguish state building from processes of state *formation*. Both occurred simultaneously in other world-historical regions such as early-modern Europe, where the state as societal organization was invented and built at the same time. This combination of historical features makes the African continent a good testing ground for the theory of extractive state building.

8 Wherever the empirical analyses allow for doing so, I deal with differences in discount rates and overall levels of investment between rulers and over time with country-year fixed effects.

While providing a relatively clean set of cases to study state building, a number of particularities distinguish modern states in Africa and processes of state building within them. First, with the exception of Ethiopia founded as colonies, modern African states had from the outset a primarily extractive nature. Driven by the material interests of rulers, African state building is thus different from cases in which states were built in response to popular demands for a stronger state. Second, due to the large military advantage of colonial armies, their relative bargaining power vis-à-vis society, at least during the colonial period, was substantial in comparison to states in other regions and historical epochs (Crowder, 1971*b*). In other instances of state building, this advantage is less pronounced, leading to more equal struggles between the state and its challengers, which, at times, take over the state. Third, the military advantage of colonial conquerors stood in contrast to their sparse territorial reach (e.g. Kirk-Greene, 1980). Their lack of proper administrative and transport infrastructure pushed up transaction costs with society to levels unseen in other states of the time.

With these limitations and scope conditions in mind, the remainder of this chapter applies the theory of rulers' investment into extractive capacity to explain patterns of state building and its impacts on development and conflict. I will first explain rulers strategies to increase their bargaining power vis-à-vis their competitors, focusing in particular on colonial conquerors' dealing with pre-existing polities. I then turn to states' efforts to lower transaction costs, highlighting the impact of physical distance between rulers and their sources of revenue and the ensuing need for transport infrastructure. The last part of the argument subsequently explores the consequences of state building along these two dimensions for development and conflict.

3.2 DEALING WITH INTERNAL COMPETITORS

When states expand, they do so by increasing their bargaining power over their competitors and society in general. This is in particular true for territorial expansion such as colonization, by which a state takes power away from the territory's previous set of rulers, be they states or not. It is also true for internal state expansion, when states try to extract revenue from hitherto uncaptured areas within their territory and thereby weaken local elites.[9] Such expansions require a choice between crushing

9 But note that such internal state building is done in a political topography already structured by differing relations between the state and local elites that have resulted from previous rounds of state

and replacing pre-existing centers of social power through investments into direct rule or co-opting them through cheaper but less lucrative indirect rule. States' resources, administrative structure, and the institutions of the alternative rulers they confront determine the choice between these two options.

3.2.1 *The problem*

The main problem of an expanding state is that very few people and territories are 'ungoverned.' Wherever one looks, individuals are under the social control of some social organization, typically headed by powerful elites (e.g. Migdal, 1988). As in other instances of territorial state expansion, the colonial empires in Africa conquered a rich panoply of pre-existing polities, most of which were organized along ethnic lines. There were large populations organized at the local level only, either hierarchically through village chiefs, or more democratically, through councils. At the opposite end of the spectrum of centralization were precolonial states with specialized, hierarchical administrations controlled by powerful obas, emirs, and kings (Murdock, 1967).

Though most precolonial states in Africa resisted conquest fiercely, they were unable to withstand the superior military technology of the European imperialists, with the notable exception of Ethiopia which successfully avoided colonization. Weaponry such as the Gatling and the Maxim guns far surpassed the firepower of African armies even if they outnumbered the troops led by European generals by an order of magnitude (Crowder, 1971*b*).[10] However, military domination by relatively few European military officers and their local conscripts was far from enough to achieve political domination and efficient extraction of revenue. Institutions that had hitherto governed the newly conquered territories stood in the way of colonizers. They had to deal with them, by either co-opting or crushing them.[11]

Crushing and replacing pre-existing institutions refers to a direct style of rule, whereas co-opting pre-existing structures of rule equals indirect rule. In most general

building (Boone, 2003). The non-existence of such relations in cases of territorial expansion of a state constitutes a crucial difference between the two types of state building.

10 As a result, colonial occupation was relatively cheap for the conquerors. Huillery (2014) shows that the French colonization of West Africa amounted only to 0.24 percent of the country's annual expenditure between 1844 and 1957.

11 The "native problem", colonial governments' difficulty of ruling over indigenous populations was thus unsurprisingly considered one of the primary problems of colonial rule (Buell, 1928; Hailey, 1945; Lugard, 1965).

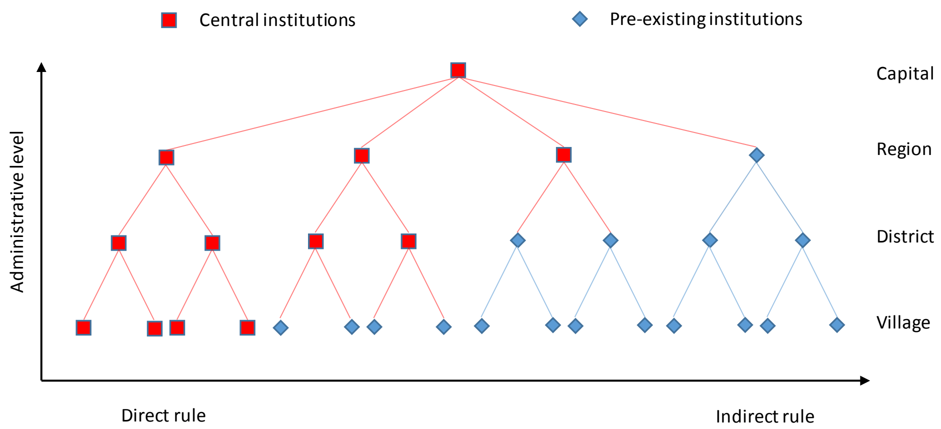


Figure 3.2: Conceptualization of indirect rule.

terms and at the heart of the definition of indirect rule, I distinguish between the integration of pre-existing institutions into the expanding state, and new institutions created by this state (Figure 3.2). Full indirect rule defines a case in which the state integrates pre-existing institutions at all administrative levels below the central government. The more levels of hierarchy between the state center and its subjects consist of institutions created by the state itself, the more direct the mode of rule becomes (Gerring et al., 2011). Thus, full direct rule is a scheme of governance in which the government creates and controls all institutions that reach down to its subjects. Between full indirect and direct rule exists a continuum of modes of rule that depict the fraction of administrative levels of the state that consist of pre-existing institutions. This conceptual framework puts the institutional basis of local rule upfront: Whether it is dependent solely on the central government or has ties to the local population. With this perspective, the approach to indirect rule allows for distinguishing those administrative intermediaries that are embedded in pre-existing institutions from those that are not.

Schemes of direct and indirect rule have varying price-tags and expected benefits. Their payoffs depend on the centralization of the pre-existing polities as well as on the capacity of the central state to crush and replace or co-opt. The following lays out the factors that determine the costs and benefits of either option and thus rulers' preferred mode of local governance.

3.2.2 *Costs of crushing and coopting*

The cost of establishing direct rule varies from that of indirect rule in two main dimensions. First, direct rulers need to disempower their predecessors by violent means and police attempts at their resurrection. Second, they have to invest in building administrative architectures that replaces that of their predecessors.

The first obstacle to direct rule is the social control held by the competitors of the state. Breaking it usually requires costly military action: the old elites have to fall, their networks of allegiance have to go with them, and the people have to be convinced that the old order is no more. Hechter (2000, p. 60) concludes that "[p]olitical centralization thus entails fearsome conflict." Less fear encumbers the old elites where they are offered a scheme of indirect rule. They can then maintain large chunks of their accustomed legislative, judiciary, and executive powers in exchange for passing on a share of their revenue to the center. Of course, offers of indirect rule are not free of violence, but made by the state while threatening violence should the old elites not abide by its terms.[12]

The costs of crushing the old political order is dependent on the degree of its centralization. More violence has to be targeted at areas that were under the rule of a centralized authority with an army before conquest (Huillery, 2010).[13] In contrast, it is comparatively cheap to suppress rulers where political authority was territorially decentralized, executed from within each individual village. Here, conquerors do not have to deal with one cohesive enemy and, after defeating it, all its subsidiaries, but can take the territory village by village.[14] However, the latter type of conquered society cannot by definition be ruled in an indirect manner because the conquering state has to implement new administrative centers that reach down to the level on which the pre-existing institutions operate.[15] Even though these institutions can be

12 See Padró i Miquel and Yared (2012) for a formal model of how indirectly ruling governments 'optimally' intervene militarily to align the incentives of co-opted local rulers with their own interests.

13 See also Garfias (2018) who finds that a weakening of local elites in Mexico during the Great Depression offered a window of opportunity for the Mexican government to crush them and centralize political power.

14 Note however, that acephalous societies in West Africa oftentimes fought the colonizers in a decentralized manner (Crowder, 1971*b*, p.4). However, using data on hostilities towards the French from official reports, Huillery (2010) shows that centralized precolonial kingdoms offered much stiffer resistance.

15 For example, the Annual Report on Southern Nigeria in 1903 explains in a derogatory description of local communities that "[i]f it is required to establish a foothold, say, in an outer portion of a large district, it is necessary to deal with each and every "town" therein. Though it may be that the most

preserved, the resulting scheme of rule is more direct than one in which a pre-existing hierarchical kingdom, which operates over multiple levels of administrative hierarchy is integrated as a whole into the conquering state (Gerring et al., 2011).

Where rulers crush centralized institutions to implement direct rule, they have to build new institutions, filled with their agents that observe and implement the new ruler's laws and executive orders (Gerring et al., 2011). The establishment of the new administration, its bureaucracies, security agencies, and judicial institutions, constitute the second major component of the costs of direct rule. Only a state that possesses sufficient and trained agents to run the state at the local level will be able to replace the old order it has destroyed. Even with the sufficient resources, this replacement necessarily takes time, during which the local abilities of the state to rule and extract revenue will be limited.

3.2.3 *Benefits of crushing and coopting*

Given the high costs of successfully implementing direct rule over pre-existing centralized institutions, why would states implemented direct rule as the by now predominant form of governance? Why wouldn't they stick to the rule "if it ain't broke, don't fix it" invoked by Gerring et al. (2011, p. 385) as a reason for the survival of centralized political institutions in schemes of indirect rule? They may do so because implementing direct rule promises high returns on investment in the future. These returns occur primarily because successful direct rule assures the state greater bargaining power and facilitates the implementation of the ruler's preferred policies. Furthermore, in particular in already highly centralized states, direct rule may be preferable to the state because it eliminates frictions that occur when local indirect rule clashes with nominally uniform state institutions.

By eliminating power centers in society that compete with the state for extraction of revenue, direct rule increases the total bargaining power of the state vis-à-vis society. This allows the state to extract a larger share of the economic surplus produced within its territory. European experiences at state building came with numerous examples for this prime motivation of direct rule. As Tilly (1990, ch. 4) notes, the establishment of direct rule in European states in the 18^{th} and 19^{th} century was spurred by an ever

important "town" is won over in the first instance, it creates no certainty that another "town," ten miles away, is going to follow suit."

greater need for resources to man and equip standing armies needed to fight off the threat of international annihilation by war. Through direct rule, rulers could tax and conscript the population directly, without relying on powerful intermediaries that would keep part of the revenue for themselves. However, these returns on investment typically occur only once the new edifice of local power has been erected and properly grounded in the population. Before then, local rule is in a limbo where the old elites are gone and new ones are not yet established. Because the length of this period decreases in the amount of resources used to implement direct rule, governments that are short of resources may not even try rule directly because they may risk to get stuck in this non-profitable limbo.[16]

Direct rule also facilitates the implementation of central policies that increase the power of the state reciprocally. Again, Europe serves as an important example for this process. In the process of state- and nation-building, rulers implemented more and more policies that served to homogenize the population of their states and increase their power. They did so, by establishing, for example, uniform education systems, setting up industrial policies, and unifying tariffs and controls of cross-border movement (Hechter, 2000). Such efforts to homogenize life in states' realm was much more difficult to implement under schemes of indirect rule where intermediary rulers with their own interests tweaked central policies. Policy making thus involved a series of bilateral bargains, resulting in a diverse array of partly inconsistent outcomes.

A third and related benefit of direct rule is that it reduces the frictions within the administrative machinery of the state. The costs of such frictions differ for every state. Some states have a uniform administrative structure throughout their territory, whereas others resemble a patchwork where local administrations vary across regions. While a patch-worked state has the institutional abilities to deal with a diverse set of local structures of rule, a uniformly centralized state lacks the capabilities necessary for doing so. Furthermore, allowing one territory to be ruled by a different set of institutions may spark demands in other regions of the same state, thus opening Pandora's box, which was hitherto closed by uniform direct rule. As a result, highly centralized states will prefer to make the administrative setup in newly conquered territories consistent with its uniform structure.

16 See, for example, Gerring et al. (2011, p. 412) on the failure of direct US-rule in Iraq in areas where they had destroyed pre-existing government functions.

3.2.4 *Empirical implications*

The previous discussion of the costs and benefits of direct and indirect rule gives rise to three observable factors that affect whether a ruler implements the one or the other. Because the empirical analysis focuses on state building in Africa, I primarily focus on the modes of rule over conquered territories. However, as I argue above, the same reasoning governs the choice of establishing direct rule in an area that has been under indirect rule for some time.

First, areas under decentralized political authorities lack the institutional structures needed to rule them indirectly. They will therefore feature more direct rule than regions that were hitherto under the control of a centralized political organization. Second, direct rule over centralized competitors will only be implemented where the conquering state possesses enough resources to crush the old and establish a new, more efficient administrative infrastructure. Third and lastly, states that have already successfully implemented direct rule in their core territory are more likely to establish direct rule in conquered territories than states that rely on more fragmented and indirect institutions of rule at home.

In Chapter 4, I test the argument that the constraints of the expanding state together with the character of the pre-existing institutions interact in determining the directness of state rule. In particular, I provide systematic evidence that sheds new light on the seminal debate on whether French colonial governments implemented a more direct style of governance than British colonial administrations (e.g. Crowder, 1968; Gerring et al., 2011; Herbst, 2000). The French empire exhibited two of the characteristics that made direct rule more profitable than in the British colonies. In particular, French colonial rulers had more administrative resources at their disposal and the republic's rigidly centralized administration gave less room for decentralized experiments on indirect governance. Both contributed to French colonialists' incentives to crush precolonial polities twice as often as British conquerors. Elucidating the importance of pre-existing institutions for the implementation of indirect rule, I provide evidence that British colonial governments ruled precolonial states in a more indirect manner than acephalous regions without centralized political authorities. The colonial mode of rule and the bargaining power left in the hands of old elites was thus a result of an interaction of constraints internal to the conquering states and the precolonial institutions that governed the populations they conquered.

With this, the empirical analysis does not touch on the effects of resource potentials on the model of local colonial rule. In principle, resource-rich areas are states' preferred targets of direct rule (e.g. Boone, 2003). However and as argued above, if the state is out for quick returns or constrained by a lack of administrative means to successfully implement direct rule, indirect rule over resource-rich might appear more promising for state builders. As an additional complication in the case of colonial rule in Africa, the foundations for direct and indirect rule were laid during the Scramble for Africa and the conquerors' treatment of precolonial elites at a time when the economic potential of local resources was often unknown and not fully exploited. It was only after colonial conquest had established the broad strokes of the modes of local governance that resource production took off.[17] With that, I leave the effect of resource potentials on the choice between direct and indirect rule open to future inquiries.

3.3 LOWERING TRANSACTION COSTS

While the bargaining power of the state vis-à-vis society determines how much revenue the state can extract, the bargains that underpin such extraction are associated with transaction costs (Levi, 1988; North, 1981). As outlined above, transaction costs arise from the costs of bargaining and the costs of implementing a bargain that relate to monitoring and enforcing the behavior of each party. More concretely, transaction costs refer to having administrators survey tax potentials and collect taxes, and the costs of local populations to access the government services they receive in return for being taxed. Because these costs ultimately diminish rulers' net revenue, they have large incentives to minimize them.

I argue in the following that physical accessibility is a key determinant of a state's ability to acquire information about the preferences and behavior of its subjects, police its realm, and supply public services.[18] To tear down the natural barriers between the people and its rulers, states have powerful incentives to invest in transport and

17 For the case of West Africa, (Hopkins, 1973, p.172) notes that "[...] the period of colonial rule gave a considerable boost to an economy which [...] was already beginning to expand but which appeared to have reached its ceiling in the last quarter of the nineteenth century."

18 This extends previous perspectives that highlight political institutions as important determinants of the transaction costs between the state and society (e.g. North, 1990).

communication infrastructure. They thereby focus their attention on those regions where they can eventually strike the most lucrative bargains and thus maximize their revenue. With an emphasis on transport infrastructure and road networks in particular, the following discusses the problem of states that are unable to reach out to their population, and highlights profit-maximizing investment strategies that reduce rulers' transaction costs. The discussion also highlights mixed effects of indirect rule on rulers' incentives to build transport infrastructure. On the one hand, rulers would prefer to concentrate their efforts on directly ruled areas where they can keep more revenue. On the other hand, local elites under indirect rule profit from low transaction costs as well and will therefore pressure the government to invest, or build local infrastructure on their own.

3.3.1 *The problem*

Even without powerful competitors, states' character as centralized organizations that rule over fixed territories can inhibit revenue extraction and policy making. To rule, the state and its leaders have to extend their reach from administrative centers towards the population that lives scattered in space. The obstacle human geography poses to centralized governance is not limited to the state and its direct agents alone, but equally affects local elites integrated into schemes of indirect rule. They as well have to reach out to the population they govern and tax. Compared to direct rule, indirect rule thus shifts the incidence of transaction costs towards local rulers, without however affecting the total transaction costs that come with the extraction of revenue.

The space between rulers and subjects makes it expensive, at times impossible, for the former to monitor and police the latter. Indeed, rulers' agents – bureaucrats, tax collectors, police officers, doctors, and teachers – need physical access to the population they are in charge of. Moving up the organizational hierarchies of the state organization, the state must secure functioning lines of communication between its agents, located in district, regional, and national capitals. To do so, mere communication without physical contact does not suffice. Personnel and resources have to move between administrative levels, and every agent has to be monitored to reduce information asymmetries and shirking.[19]

19 See, for example, Olken (2007) on the effect of audits on corruption in local administrations.

Faced with high transaction costs arising from the need to reach out to spread out populations, states typically invest in infrastructure that makes their people and territory accessible (Mann, 1984). Indeed, from the Roman roads built to bind peripheries to the imperial center (Dalgaard et al., 2018) to the royal Inca paths that spanned the Andes (Jenkins, 2001), intricate communication and transport infrastructures have marked the path of eminent empires in history. This was no different in modern Europe. As Weber (1977, p. 195) notes, state building in 19$^{\text{th}}$ century France came with "roads, roads, and still more roads." Similarly, the rapid development of railroad networks was a hallmark of state building in Europe during that episode (e.g. Wimmer, 2018).

Against this backdrop, it is not surprising that spread out populations and transport infrastructure proved and still prove to be a major bottleneck for state rule in Africa (Herbst, 2000). Before colonization, centralized governance was mostly confined to areas fertile enough to carry high population densities, for example along the Nile in the North, the Niger in West Africa, and the Great Lakes in Central Africa (e.g. Bates, 1983). Modern means of and infrastructure for cheap transport could have mediated these impediments but were virtually non-existent on the continent (Chaves, Engerman and Robinson, 2013). The wheel was not in use for partly unknown reasons (Law, 1980), and the Tsetse fly inhibited the use of pack animals (Alsan, 2015) outside the Sahel and Saharan Desert where camels were valuable alternatives. Goods were thus mainly carried by human porters who were relatively slow and expensive (Chaves, Engerman and Robinson, 2013; Lugard, 1965). Similarly and partly related, most transport infrastructure consisted only of paths. Roads only crossed a few regions, such as the Buganda and Asante kingdoms (Reid, 2002; Wilks, 1975).

Faced with this major constraint to economic development and their administrative capacity, colonial governments pushed for a transport revolution early in the 20$^{\text{th}}$ century, powered by the steam engines of railroads, and later the diesel motors of cars (Lugard, 1965, pp. 461-476). Both transport technologies required heavy investment into rails and roads, which soon became the physical backbones of colonial states. Yet, infrastructure investments in colonial Africa came not even close to those made by state builders in Europe. 100 years later, African states are still constrained by inadequate transportation systems (Herbst, 2000). As a result, a public official in the

Central African Republic notes that "[t]he State [s]tops at PK 12 – i.e. 12 kilometres from the capital, Bangui" (quoted in Bierschenk and Olivier de Sardan, 1997, p. 441).

The geographic distributions of the population is of course not the only challenge to cheap and efficient state extraction. Ethno-linguistic diversity figures prominently as well. Where people speak a plethora of language, states have a hard time communicating with them and thus monitoring their behavior. Linguistic standardization through centralized education systems has thus been an important tool for gaining state control over the population (e.g. Gellner, 1983). Similarly, the standardization of economic exchange took on an important role in states' efforts to gain a handle on observing and steering individuals' behavior. To achieve this goal, states centralized currencies and standardized measures (e.g. Mann, 1984; North, 1981; Scott, 1998).

Again, not only in Europe have states standardized languages and economic interactions. The European colonizers have transferred these tools with more or less success to their colonial territories. They fostered education, by private means through missionaries or public means of the colonial state itself, which facilitated the interaction between rulers, their agents, and the population. Julius Nyerere followed up on these efforts and established Swahili as the national language of postcolonial Tanzania (e.g. Miguel, 2004). Similarly, colonial states in Africa have since their inception tried to regulate the means of economic exchange, in particular by regulating and at times prohibiting the use of currencies competing with their own, state issued currency, such as the British Pound or the French Franc (Hopkins, 1973, pp. 206-209).[20]

3.3.2 *Roads to rule: Making people and territory accessible*

But investments that lower the costs of state-society transactions are typically expensive, in particular when aimed at building roads and rails that facilitate transport and physical access to the state's population. Faced with their cost, rulers make strategic decisions about where in their territory they expand their reach. The following paragraphs highlight three factors that affect their calculus. First, there is a direct effect of facilitating transactions between citizens and their rulers, be they embedded in schemes of direct or indirect rule. Second, lower transaction costs have

20 The colonial dependence on European currencies has however persisted until today, with many African currencies still pegging their currencies the British Pound or the French Franc. This lack of nationalized monetary policies is at the same time a contributor to and symptom of weak state power in parts of the continent (Herbst, 2000).

spillover effects that spur exchanges between citizens, which central and local rulers under indirect rule try to benefit from. Lastly, and contrary to the rulers' interest, lower transaction costs among citizens may give rise to opportunities for political organization in parallel and at times against the state. State builders will prioritize investments that maximize the net benefit of these effects.

To maximize the direct benefits of lower transaction costs between the state and citizens, rulers invest into infrastructure that facilitates their bilateral interaction by allowing administrators to survey and tax while giving citizens access to government services. In the case of physical communication and transport infrastructure, this requires building star shaped networks that connect the population with the respective local, regional, and national capitals. Proverbially, all roads will thus lead to Rome, a pattern also observed by Weber (1977) when writing on the evolving networks of administrative highways in early modern France (see also Scott, 1998, pp. 73-76). Responding to the uneven spatial distribution of revenue potential, states make areas of valuable and easily taxable economic activity better accessible than those with low or hard-to-tap tax potential. This does not only entail building the respective roads, but also building administrative headquarters in areas of high tax value.[21]

The evolving star-shaped transport network is of good use for tax collectors and police officers who fan out from the state's administrative headquarters. It can also be used by citizens to access the government services they harbor, in particular hospitals, higher schools, and courts. However, heavily centered on a few locations, such centralized networks will not much facilitate decentralized communication and economic exchange. Because such trade can spur economic growth and expand the tax base, rulers have incentives to design transportation networks that incentivize a broad array of taxable economic activities, in addition to fostering state-citizen interactions.

Transport infrastructure as a main driver of national and international market integration is covered by a large literature in economics (for a comprehensive review, see Donaldson, 2015). By lowering the costs of trade between producers and buyers,

21 See also de Juan, Krautwald and Pierskalla (2017) who show that the German colonizers in South Africa located police stations more often in mineral-rich areas. The same applies to proto-states that establish stationary presence more frequently around valuable resources that are easily taxable (Sanchez de la Sierra, 2018). In contrast, Pierskalla, de Juan and Montgomery (2019) find that the location of German colonial headquarters in German East Africa mostly located their administrative headquarters in response to local resistance against colonization. However, their empirical results might be biased by controlling for the presence of local roads, which are part of the administrative response to local conditions.

better roads and rails typically lead to increased trade volumes, spur economic growth, and increase welfare (Donaldson, 2018; Donaldson and Hornbeck, 2016; Faber, 2014). This general pattern holds true for the African continent as well. Colonial investments into railroads had large and persistent effects on the organization of economic activity (Jedwab and Moradi, 2016; Jedwab, Kerby and Moradi, 2017), and trade on the road network of the continent fosters urbanization (Jedwab and Storeygard, 2018) and economic growth (Storeygard, 2016).

But the state cannot tax all trade and economic activity equally well. Among the most taxable economic activity is the production of export produce. Because most exports pass through central ports, the assessment of their value and the collection of fees is much cheaper than decentralized collection of taxes levied on individual incomes or property (e.g. Tilly, 1990).[22] A revenue-maximizing state will therefore build transport infrastructure that lowers transportation costs between resource-rich areas and the ports. This comes at the expense of fostering internal trade which must be taxed in a more decentralized and costly manner. Following this strategy, states become "gatekeepers" that focus their revenue collection at central hubs (Cooper, 2002; Roessler et al., 2018).

Combining these two arguments with the greater payoffs from extraction under direct rule suggests that states may invest more into transport infrastructure in areas under direct rule. However, two mechanisms work in the opposite direction. First, roads also enable greater control and revenue collection by local rulers under indirect rule. These may therefore either build roads themselves or pressure the central government to do so. Second, at least in the early days after implementing direct rule, governments may have a harder time building roads in areas where they have just set up new governance institutions than in regions under indirect rule. Here, local rulers can more easily mobilize the resources needed to build transport infrastructure.[23] The net effect of these diverging mechanisms is theoretically unclear.

While investments into infrastructure can increase the control and revenue of the state, this implies that it can also increase the capacity of its local competitors. In particular roads are public goods that are neutral in a way that, beyond their physical

22 Ertman (1997) contests and notes that in early modern Europe taxing trade required highly specialized bureaucrats, whereas land taxes could be easily outsourced to local notables (see also Brewer, 1989).

23 One example of this is the case of the indirectly ruled Buganda kindom in Uganda, which could draw on the *Luwalo*, a precolonial public-work tax, to build roads (Hailey, 1945, p. 1583).

structure, one cannot influence what types of transaction they facilitate.[24] Roads can thus be used by non-state actors to reach out to local populations and mobilize them for their own purposes. This double-edged character of technologies for social control has been noted by Mann (1984, p. 193) who observes that none of them "necessarily changes the relationship between a state and its civil society". As discussed in more detail below in Subsection 3.4.3, the structure of transport networks determine their effects on the relative power of the state and its competitors.

The potential of backfiring investments that lower transaction costs with and within society has not only been recognize by Mann (1984) but also by dictators who feared mobilization against them. Stressing the danger of building roads, the long-term despot of Zaire (now the DR Congo), Mobutu Sese Seko, told his Rwandan peer Juvénal Habyiarama: "I told you not to build any road. [...] Building roads never did any good. I've been in power in Zaire for thirty years and I never built one road. Now they are driving down them to get you" (quoted in Englebert, 2000, p. 135). Few years later, however, rebels led by Laurent-Désiré Kabila used what was left of Zaire's road network to march from the East of the country to Kinshasa in the West and upended Mobutu's dictatorship.

3.3.3 *Empirical implications*

Mobutu's advice to Habyiarama illustrates the rulers' dilemma: How to build infrastructure that smooths the interaction between the state and its citizens, oils a growing and taxable economy, but minimizes the opportunities for challengers to mobilize segments to threaten the government?

The discussion above suggests that the solution to the problem lies in the structure of the transport network rulers build. Rulers who want to maximize their revenue and long-term survival will invest in connecting resource rich areas with their administrative capitals and ports. So they can increase their ability for policy-making and incentivize export production that can be taxed at a low cost. At the same time, rulers will try to refrain from excessively connecting areas from which potential challenges to their rule could arise. Examining the extractive character of colonial and postcolonial transport

24 This is different for railroads that require specialized vehicles, and telecommunication infrastructure such as telegraphs that feature central nodes that can be used as filters or at least for monitoring purposes.

infrastructure in Africa (1900-2015), Chapter 5 examines the first two arguments and additionally analyzes the net effect of indirect rule on transport infrastructure. Chapter 8 studies in the impact of transport networks on the ability of states to repress and its challengers to mobilize for rebellion, a relationship that I explore in more theoretical detail below.

3.4 THE STATE, DEVELOPMENT AND CONFLICT

Revenue-maximizing rulers build their states to increase their bargaining power through direct and indirect rule and decrease transaction costs with society by investing in transport networks. As outlined in the following section, these patterns of state building have three important implications for local development and conflict. The first argument focuses on the effects of states' bargaining power on their terms of trade with the population. Rulers profit more from trading revenue for public services where they have greater bargaining power. Thus, compared to indirect rule, direct rule reduces the developmental benefits local populations receive in return for revenue extraction. Second, the extent of state-society interactions decreases in the transaction costs associated with them. Because the trade of state revenue for public services brings benefits to the state and the people, local levels of development decrease with the costs of transport between them. Third and lastly, states efforts to build roads to reach their society can have spill-over effects on the mobilizational capacity of their challengers. These increase the risk of conflict when they assist the state's challengers more than they increase governments' control over the population. Over the next pages, I will outline the theoretical arguments that underpin these three effects of states' efforts to grow stronger. Each is treated in greater theoretical depth in the respective empirical chapters.

As the reader may notice, these three avenues of inquiry do not exhaust the array of all possible questions to be asked about the effects of local state building. First, I leave the effects of the choice between direct and indirect rule on conflict aside, noting that it is well covered by a growing literature on the topic.[25] Second, the empirical

25 In particular, there is increasing interest in the effects of traditional institutions and indirect rule on conflict in Africa. Wig (2016), Wig and Kromrey (2018), and Depetris-Chauvin (2014) all suggest that strong precolonial and contemporary ethnic institutions have pacifying effects. Some of the underlying historical mechanism might indeed run through colonial indirect rule of centralized ethnic groups which strengthened their ability to bargain with the state. Accordingly, Mustasilta (2019)

analyses neglect central governments' revenues that form the other side of the coin of the trade of taxes for government services. With few spatially disaggregated data on African states' finances, a comprehensive analysis of their revenue falls outside the scope of this study.[26]

3.4.1 *Indirect rule and local development*

The benefits of state extraction for the local population depend on its bargaining power and that of elites at the multiple levels of states' administrative hierarchy. Compared with indirect rule, the establishment of direct rule decreases the bargaining power of local elites vis-à-vis the central state, and that of the local population confronting its local rulers.

Local elites under indirect rule have greater bargaining power vis-à-vis the central state than those under direct rule. Indirect rule leaves local elites with their own administrative apparatus, which allows them to control the population and mobilize against the state if need be.[27] Under direct rule, these elites have by definition lost their power and, in the many cases, even their lives. Thus, given the same level of extraction at the local level, local elites under indirect control of the state can keep a larger share of revenue (Perham, 1937).

Equally important for the provision of local public services, populations under indirect rule enjoy greater bargaining power vis-à-vis their local rulers than under direct rule. Local elites in schemes of direct rule are appointed by their administrative superiors who incentivize them to act according to the preferences of the state's ruler. In particular under colonial rule, this impregnated them against following up on popular preferences.[28] In contrast, indirect rule left pre-existing institutional arrangements that held local rulers in power intact. Although only in few cases democratic, they were rooted in the local patronage networks that connected rulers to their constituents. Such local embeddedness of local elites creates "an 'encompassing'

finds that countries with a stronger integration of traditional authorities into the state administration experience less intrastate conflict.

26 Beyond the need for data on direct taxation, this would warrant comprehensive data on the incidence of indirect taxation. Both do currently not exist.

27 For the power of traditional chiefs to organize local collective action in contemporary Africa, see Baldwin (2016).

28 See, for example, Afigbo (1972) and Martin (1988) for elaborations on unresponsive and corrupt "warrant chiefs" that were lifted to power in Southeastern Nigeria.

interest in the material well-being of their communities" (Baldwin, 2016, p. 52) and incentivizes more redistribution of revenue to the local population than attained under direct rule.

Note that there is an important difference between the incentive structure of local rulers under direct and indirect colonial rule as described here and the arguments laid out by Acemoglu, Reed and Robinson (2014). The latter find that more competition among local chiefly families fosters chiefs' accountability and efforts to provide public services. Such competition is, however, outside the scope of my argument, which rather focuses on the difference between local elite's appointment by the colonial government or through local precolonial institutions. Furthermore, my discussion of direct and indirect colonial rule does not necessarily travel to democratic settings. Fairly elected local politicians have stronger incentives to service the population than tradition leaders because elections offer the people more power than the patronage networks on which unelected traditional leaders typically depend. In contrast, colonial direct rule was undemocratic and steered by metropolitan interests far removed from those of the local population, making indirect rule comparatively responsive.

Beyond having the means and incentives to provide local public services, indirect rulers have a crucial information advantage about the preferences of the local population as compared to appointed officials in schemes of direct rule. In particular in the early days of direct rule, local administrative elites will have difficulties to "read" the population, since they lack the social ties that supply them with relevant information about the population they rule.[29] This may lead them to provide services that do not fit local needs and demands. However, this disadvantage of direct rule may wither away as the ties between the local population and the direct administration grows stronger.

In sum, if local rulers under indirect rule can retain a greater share of local revenue, know more about the preferences of the population, and have incentives to act on them, indirect rule fosters the translation of state revenue into public services and local development. This argument is rolled out in more detail in Chapter 6. The respective empirical analysis shows that revenue potential from cash crop production

29 Under direct colonial rule, the inability of colonial officers to communicate in the local language oftentimes compounded this problem, in particular in French colonies where officers were not required to speak the local language and French was a mandatory requirement for chiefs (Cohen, 1971*b*; Crowder, 1971*a*).

led to higher levels of public service provision in areas under indirect colonial rule as compared to areas under direct control.

3.4.2 *Transaction costs, state reach and development*

Besides the bargaining power of the state, the costs of transactions between the state and society affect the revenue of the state and the services it provides in return. Above, I argue that states increase their reach and revenue by investing in physical transport infrastructure. The bargaining logic of the model implies that higher levels of revenue come with more services provided by the state. Because successful bargains produce benefits for all involved parties, society's profit and thus local development increases with this expansion of trade with the state. This effect of lower transaction costs runs through three main channels that consist in a better enforcement of law and order, higher levels of public service provisions, and improved access to private rents handed out by the state. Lastly, transport networks facilitate revenue extraction by the state, which reduces private welfare. I briefly explore these in the following.

First, low transport costs between states' headquarters and the population enables bureaucrats and police officers to enforce the 'rules of the game' of their state. While institutionalists (Acemoglu, Johnson and Robinson, 2002, 2001; North, 1981) emphasize the benefits of 'good' institutions, in particular secure property rights, less attention has been paid to the need to enforce them. Even if the state's institutions are inclusive and guarantee individual freedoms and rights on paper, limited and uneven reach of states creates weak spots where they are unable to enforce their laws (Campante and Do, 2014). Areas inaccessible to state agents are not necessarily riven by "bad institutions". Rather, either no institutions, but more plausibly a set of inconsistent institutions prevail. In the latter case, various competing social actors try to govern over the population (e.g. Migdal, 1988). The insecurity of such an environment curbs development, for example by discouraging investment. To echo Huntington (1968), the degree of local state governance might thus be at least as important as its character.

Second and beyond setting the rules of everyday life, states provide a multitude of public services such as education and health care, many of which cause individual development. However, to provide state services of good quality, states must be capable of monitoring demand, investing the necessary resources, and controlling the agents that run services locally. Local state reach therefore increases the level and quality of public service provision (e.g. Krishna and Schober, 2014; Henn, 2018). Low transport costs between citizens and their state also improve their access to services not provided at the local level. Hospitals, specialized education facilities, and courts are typically located in administrative capitals and thus harder to reach when traveling from far away or on bad roads.

Similarly, smooth local accessibility increases the possibilities for citizens to access the coffers of the state privately. Private rents given out by state agencies consist, for example, in jobs in the public sector and formal or informal subsidies. Such handouts are more accessible to citizens where they interact with state agents regularly.[30] Paying salaries or social subsidies, states and bureaucrats expect a certain behavior in return. Again, monitoring costs are lower where they can travel to, thus increasing the supply of private rents. Lastly, in some areas a state may be too incapable to even give out rents, because it simply lacks access their receivers. These factors do not only affect patterns of local development, but also contribute to comparatively high levels of migration towards capital cities (Ades and Glaeser, 1995).

These three pillars of development-inducing state services come with citizens' need to pay direct or indirect taxes. As argued at length above, the physical accessibility of an area facilitates direct tax collection, while transport infrastructure also spurs trade that can be taxed elsewhere. Taxation in and off itself has a negative effect on citizens' welfare.

The bargaining logic of the model implies that the net effect of the trade of taxes for services is positive for citizens' welfare. Because the trade between citizens and the state becomes more extensive with lower transaction costs, I expect that decreases in travel times to administrative capitals have a positive effect on local development. I test this expectation in Chapter 7 with time-variant data on travel times to regional and national capitals. The panel-design of the analysis focuses on within-location variation in state reach and thereby addresses concerns about the cross-sectional endogeneity of state reach.

30 Part of this mechanism runs through information on these rents available to citizens (see e.g. Banerjee et al., 2018).

3.4.3 *Violence between states and their internal challengers*

Beyond fostering economic development through increased interaction between the state and its citizens, transport infrastructure has an important, but double-edged impact on conflict. On the one hand, roads lower the costs of the state to monitor the population and repress rebels. On the other hand, however, they decrease transaction costs within society and make it easier for rebels to mobilize it against the state. The following section provides the logic underlying these arguments and derives a prediction about the effects of the structure of road networks on violent conflict.

From a state-centric perspective, good transport networks that connect the state to its population allow the state to monitor the behavior of the population and deploy its security apparatus to keep the peace. In line with this argument, a large literature links subnational inaccessibility to low levels of state reach and high risk of conflict. Thus, rugged or otherwise inaccessible terrain is generally associated with high levels of conflict (e.g. Fearon and Laitin, 2003; Tollefsen and Buhaug, 2015). Similarly, areas distant from states' capitals are found to experience comparatively high levels of conflict by Buhaug and Rød (2006), Raleigh and Hegre (2009), and Tollefsen and Buhaug (2015). These findings mirror Herbst's (2000) argument, that favorable geographies are a pre-condition for peace and strong states. Less consistent are the findings on the association of violent conflict with local levels of road density. While Buhaug and Rød (2006) find that areas with dense road networks experience less conflict, Raleigh and Hegre (2009) report that better roads are associated with more conflict.

However, and potentially explaining the mixed findings on the link between roads and local conflict, focusing only on the state's ability to access a region neglects the perspective of the state's challengers. Because conflicts are dyadic and fought between states and their challengers, both perspectives are crucial (Cunningham, Gleditsch and Salehyan, 2009). Once we widen the theoretical picture accordingly, we see that states' challengers face very much the same problem and incentives as the state. Just as the central government, prospective rebels need to access 'their' territory and population in order to extract material resources, mobilize soldiers, and control their troops. To do so, rebels, just as the state, need to be able to monitor the behavior of the population and punish defection, a logic stressed by a growing literature on rebel governance (e.g. Weinstein, 2007; Mampilly, 2011; Arjona, 2014). The lower the costs of interaction between the local population and a potential challenger, the greater the opportunity and motivation for the challenger to emerge and mobilize against the state and other actors that together compete for control over people and resources. This is why rebels, who oftentimes mobilize on the basis of ethnic identities (Fearon and Laitin, 2000), are as dependent on roads as is the state.

These two perspectives highlight that most roads run both ways, allowing the state to police society and enabling challengers to mobilize society and increase their power. With that, the net effect of transport networks depends on their structure. If a given set of roads facilitates interaction between the state and its citizens more than it lowers their costs of interactions with the state's competitors, the state's relative power will increase. In contrast, if a set of roads raises challengers' ability to mobilize the population more than it improves the state's ability to police, it increases the relative power of the challenger. The structure of transport networks thus determines its effects on the power of the state relative to its competitors and, by extension, the risk of conflict between them.

To capture this dual perspective, Chapter 8 introduces the concept of *relational state capacity* as the difference between an ethnic group's accessibility from the state's capital and its internal connectedness, both measured on road networks. It then tests the main prediction that higher levels of relational state capacity decrease the probability of violent conflict in Africa.

3.5 SUMMARY AND OUTLOOK

To derive the theory of state building and its effects on development and conflict outlined above, I argue that the state is a hierarchical organization that governs a nominal territory and is headed by a profit-maximizing ruler. States extract revenue from their population in return for services that they produce with a comparative advantage: peace, justice, and public services such as education and health care. Rulers' profits are constrained by their bargaining power vis-à-vis society and the transaction costs that pertain to each bargain. To increase their revenue, rulers invest to increase their bargaining power through direct or indirect rule and to decrease transaction costs through transport infrastructure that makes their people accessible.

Both investments result in strengthening the extractive capacities of the state and constitute what I understand as state building. They are the focus of the first empirical part of this dissertation.

Rulers of expanding states can increase their bargaining power either through costly direct rule over their local competitors or through cheaper but less lucrative indirect rule. The choice between both strategies is co-determined by the level of centralization of pre-existing institutions that may be too decentralized to be ruled indirectly, and by the administrative resources and institutional constraints of the expanding state. Drawing on variation between and within the French and British empires in Africa, Chapter 4 provides empirical evidence that supports these arguments.

Beyond coping with their competitors, states build transport infrastructure to reduce the cost of transacting with their population. Maximizing prospective profits from revenue extraction, rulers invest in roads and rails that tap into resource-rich areas and connect them to ports and administrative capitals. To assess this argument, Chapter 5 analyzes the structure and development of transport networks in Africa between 1900 and 2015.

Altering the distribution of power between the state and society and contracting the costs of transactions between them, the theoretical argument predicts fundamental effects of these two dimensions of state building on development and conflict. These are the subject of the second empirical part.

Chapter 6 follows up on the argument that indirect rule leaves local populations with greater bargaining power and thus increases the benefits they receive in return for providing the state with revenue. I test this prediction by drawing on the differences in the application of indirect rule within and between the French and British colonial empires, soil suitability for cash crop agriculture as a proxy for revenue extraction, and data on local colonial education rates.

I then turn towards the impact of transaction costs between the state and society on local development. As citizens are more closely connected to their administrative headquarters, the costs of providing them with public services decreases. So does the cost of extracting revenue. Both factors lead to increased trading between the state and society, which increases welfare on the side of citizens (as well as the ruler). Chapter 7 tests this argument with time-variant data on travel times to regional and national capitals and local levels of socio-economic development since the 1960s.

However, lower transaction costs on good road networks do not only facilitate interactions between the state and its citizens. They also lead to greater opportunities for alternative rulers to mobilize parts of the population, increase their power, and challenge local state rule. Chapter 8 shows that, depending on the structure of road networks, this increase in the mobilizational capacity of potential rebels can outweigh the ability of the state to police them, thus exacerbating the risk of conflict.

BUILDING STATES IN AFRICA

4

CONTINUITY OR CHANGE: (IN)DIRECT RULE IN BRITISH AND FRENCH COLONIAL AFRICA

[...] institutions and methods, in order to command success
and promote the happiness and welfare of the people,
must be deep-rooted in their traditions and prejudices.
Lord Lugard (1965, p. 211), Governor-General of Nigeria

[...] suppress the great native polities
which are nearly always a barrier between us and our subject.
William Ponty (1910), Governor-General of French West Africa
quoted by Suret-Canale (1988, p. 150).

After conquering Africa in the late 19th century, colonial state builders had to confront the question of how to rule indigenous populations and how to treat their elites. Debates about the benefits and disadvantages of direct and indirect rule dominated much of the general discussion on colonialism after the 'Scramble for Africa'. This did not change much after the establishment of colonial rule, and the topic marks scholarly debates and research until this very day. It is thus remarkable that we lack systematic data and evidence on the extent to which and where colonial governments ruled indirectly.

This chapter analyses the strategies of British and French colonial governments to augment their bargaining power vis-à-vis the population and its precolonial elites. Building on the theoretical model of extractive state building and the historical literature, I argue that direct rule was a more attractive choice for French colonizers, who tried to mold their colonized subjects into the French republic and commanded more administrative resources than the British. British colonial governments in turn oftentimes chose a path of co-optation and indirect rule where precolonial institutions were sufficiently centralized. New data on the (dis-)continuation of the lines of succession of precolonial polities show large differences in the mode of local governance between the French and British colonizers. Data on local administrations provides evidence that British governments ruled most indirectly where they could build on centralized precolonial institutions.

Just as strategies of direct and indirect rule marked European state building (Hechter, 1975; Tilly, 1975), they shaped the nature of colonial state building. Arguments about variation in the application of direct and indirect rule in colonial Africa roughly follow two lines. The first is concerned with differences between, in particular, the French and British empires. While some argue that both relied to the same extend on local intermediaries (Gerring et al., 2011; Herbst, 2000; Mamdani, 1996), others claim to see marked differences in their treatment of pre-existing institutions (Asiwaju, 1970; Crowder, 1968; Miles, 1994). Second, a historical literature on within-colony variation of indirect rule stresses the role of precolonial institutions. These were either sufficiently centralized or too fragmented for integration into schemes of indirect rule (Fortes and Evans-Pritchard, 1940; Gerring et al., 2011). Despite focusing on the very center of variation in colonial governance, the debate on indirect rule so far lacks comprehensive data on variation in its application within and between French and British colonies.

Testing the arguments around these two axes of variation in the indirectness of colonial rule does not only speak to the theory of extractive state building developed in the previous chapter. It is also important for better understanding the political and socio-economic consequences of British and French colonialism in Africa. In particular, indirect colonial rule is considered one of the prime historical pathways through which precolonial factors persistently affect outcomes until this day (e.g. Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013*b*). Similarly, local variation in the character of colonial rule likely determines the effect of colonialism as a whole (Bruhn and Gallego, 2012; Iyer, 2010; Lange, 2009; Lankina and Getachew, 2012; Mamdani, 1996). And because indirect rule has a strong ethnic basis, local variation in its application might shed light on the historical (trans)formation of ethnic inequalities (Wucherpfennig, Hunziker and Cederman, 2016), identities (Ali et al., 2018; Ranger, 1997), customary institutions (Baldwin, 2016), and land rights (Boone, 2003; Firmin-Sellers, 2000).

For the empirical analysis, I draw on a variety of systematic historical data sources. To examine differences in the indirectness of rule in the French and British empires, I collect encyclopedial data from Stewart (2006) on the lines of succession of 124 colonized polities in Africa. Taking the end of a polity's line of succession as a proxy for its demise, the data show that only 30% of the polities colonized by the French but 70% of those colonized by the British survived colonial rule. This large difference holds across plausibly exogenous French-British borders that run perpendicular to the West African coastline.

I then assess the effect of precolonial institutions on the indirectness of rule by analyzing newly collected data on local administrations in 8 British colonies. These archival data from official reports shed light on the colonial and indigenous dimensions of local governance. The colonial dimension concerns the administrative effort exerted by the British. Proxied by the size of districts and the number of European administrators, the data show that the British exerted less effort where they could rule through precolonially centralized institutions. Data on indigenous governance in British colonies demonstrate that chiefs in precolonially centralized regions presided over bigger budgets and enjoyed a higher status. These patterns are absent or even reversed in comparable data from French West Africa.

4.1 INDIRECT RULE IN THE LITERATURE

A crucial dimension of local governance arrangements in territories under alien dominance is the degree to which the imperial power cedes authority to the subordinate units it rules (Gerring et al., 2011). Not surprisingly, the struggle for effective local rule was no less of an issue for early modern state-builders in Europe (Hechter, 1975; Tilly, 1975) than for the European imperialists in Africa (Buell, 1928; Hailey, 1945; Lugard, 1965). Colonial powers aimed to secure their hegemony over the conquered populations with minimal effort constrained by "the thin white line" (Kirk-Greene, 1980), the small number of European officers available to administer the vast territories colonized through the 'Scramble for Africa' that followed the Berlin Conference in 1884-1885.

To bridge the social, organizational, and geographical distance between the colonial centers and local populations, all colonial powers relied to a significant degree on local intermediaries (Gerring et al., 2011). These middlemen provided the crucial link between the European rulers and the indigenous population. They thus became "decentralized despots" in a bifurcated colonial state that revered the "customary" to uphold its rule (Mamdani, 1996). Extrapolating the precolonial basis on which these intermediaries were at times appointed, Herbst (2000) claims that colonial empires relied on the great variety of existing institutions to keep the costs of occupation at a minimum. For Herbst (2000, p. 84), "[t]he Africans were unimpressed with the extent of the administrative reforms made by the white man." This perspective of ubiquitous continuity of precolonial governance arrangements stands against one of wide-spread disruption and change. It is apparent in Young's (1994) description of the Belgian "Crusher of Rocks" in the Congo, or Achebe's (1958) novelist depiction of British colonialism in Southeastern Nigeria where 'things fell apart.' These accounts suggest that colonialism did indeed overthrow governance arrangements at the local level.

The divergent views on the continuity and change of local governance arrangements can be reconciled through a lens focused on the nature of local institutions during the colonial period. As highlighted in Chapter 3, I distinguish between traditional, precolonial institutions integrated into the colonial state, and new institutions created by the colonial state. Under full indirect rule, the state integrates pre-existing institutions at all administrative levels below the center. The indirectness of governance decreases in the administrative levels with institutions created by the state (Gerring et al., 2011). This reasoning focuses less on whether a local ruler or middleman is European or indigenous. Rather, it puts the institutional underpinnings of local governance upfront and asks whether a ruler is dependent solely on the colonial government or has ties to the local population. With that, the approach differentiates colonial intermediaries that were embedded in precolonial institutions from those that were not.

The literature on local colonial governance has explored two main axes of variation in the degree to which it integrated pre-existing institutions. The first focuses on the identity of the colonizer. The second relates to variation explained by the strength of precolonial institutions themselves.

First, a comparative literature analyzes whether differences between colonizing empires affected their dealings with pre-existing institutions. In particular, discussions of the difference between the French and the British styles of local rule permeate officials' and researchers'[1] discussions on colonial rule since the colonial days. Marking his stance, the Governor of the Côte d'Ivoire, Deschamps, confidently asserted that "one can scarcely detect the French administrative policy previous to 1945; it differed

1 Note that the two communities heavily overlap during the colonial period, reasons for which many accounts are impregnated by a biased perception of the 'own' system.

from [Britain's] [...] only in its more familiar style and less clearly defined goals" (cited in Herbst, 2000, p. 82). Many have followed Deschamps' claim and hence argued that every colonial state ruled through local intermediaries and thus indirectly (e.g. Gerring et al., 2011; Herbst, 2000; Mamdani, 1996). In contrast stand scholars who maintain that, although both empires relied on local intermediaries, the French approach to local rule was more direct than the British (Asiwaju, 1970; Crowder, 1968; Hailey, 1945; Miles, 1994). In this dichotomy, the British are described as having co-opted pre-existing institutions where they existed. The French in turn were comparatively hostile towards them, oftentimes replacing them with their own institutions.

Second, a substantive body of historical case studies suggests that precolonial factors affected the feasibility of indirect rule. In particular, many argue that traditional, precolonial institutions best facilitated indirect rule where they were sufficiently centralized to be integrated into the colonial administration (e.g. Gerring et al., 2011; Hicks, 1961; Tignor, 1971). In acephalous, stateless societies, colonizers had to make up for the absence of readily available hierarchical political structures and establish institutions foreign to the local population. Here, "[t]he colonial government cannot administer through [...] political segments, but has to employ administrative agents" (Fortes and Evans-Pritchard, 1940, p. 15). This is when the traditional organization of small-scale societies fell apart in Southeastern Nigeria, where British administrations struggled to create institutions effective at ruling the population.

Although there has been much debate about the heterogeneity of local governance arrangements within and across colonial empires, no systematic and disaggregated evidence exists on where colonizer ruled indirectly. Raising the same complaint, Herbst (2000) measures the directness of colonial rule via the density of colonial road networks. This use of aggregate road network data may produce misleading conclusions because it does not capture within-colony variation in indirect rule. More importantly however, even local road measures are no reliable proxy for indirect rule, because local rulers under any governance scheme have incentives to reduce the transaction costs they face when governing and extracting revenue (see Chapter 5). It is, for example, not at all obvious why the cash crop producing Buganda kingdom in Uganda could not feature a combination of dense roads and indirect rule.

The best cross-empire proxy for indirect rule so far consists in the number of European administrators employed by colonial states.[2] On this metric, in 1938, French colonial governance employed 250 Administrators per million inhabitants in French West and Equatorial Africa that compare to a pale 29 administrators per million employed in British colonies in 1939 (Herbst, 2000; Kirk-Greene, 1980). However, these aggregate figures do not tell us much about the territorial organization and institutional underpinnings of colonial rule. Because indirect rule is a local phenomenon and varies within colonies, colony- and empire-level statistics cannot precisely capture it.

If data on the directness of colonial rule is scarce across colonial empires, it is non-existent at the sub-colony level, at least for Africa.[3] Although there is a wealth of case-studies on French and British strategies of local rule, this has inhibited quantitative evidence on the link between precolonial and colonial institutions. It is, however, at this local level that one has to investigate the effect of precolonial institutions on colonial indirect rule to avoid the fallacy of ecological inferences.

Scarce data and evidence also constitute an important obstacle to research on the historical roots of postcolonial politics and development in Africa that builds on assumptions about the incidence of indirect rule. For example, Blanton, Mason and Athow (2001) argue that British indirect rule increased the risk of ethnic competition and civil war as compared to French direct rule (see also Ali et al., 2018). Similarly, the research design of Wucherpfennig, Hunziker and Cederman (2016) relies on the assumption that French rule led to concentrated political power of ethnic groups settled along the coast, while British indirect rule caused more variation in who rose to power after independence. Examining the origins of postcolonial development, Gennaioli and Rainer (2007) as well as Michalopoulos and Papaioannou (2013*b*) argue that one main reason for an enduring positive effect of centralized precolonial institutions consists in colonial indirect rule. Such arguments, while consistent with the respective empirical results, have so far lacked systematic evidence to sustain their assumptions about patterns of direct and indirect colonial rule.

In sum, the existing research on indirect colonial rule and its consequences is underspecified in two important ways. First, no systematic evidence on the difference

2 Lange (2004) proxies for the indirectness of rule in various British colonies by relying on the colony-level proportion of court-cases handled by native courts. Restricted to the British empire, the data cannot be used for cross-empire comparisons.

3 For India, see Iyer's (2010) work on princely states.

between the indirectness of colonial rule in the French and British empires exists. Second, no systematic and spatially disaggregated evidence sheds light on the effect of precolonial institutions on indirect rule. This limits our understanding on how colonial rulers built their states in collaboration with or at the expense of their predecessors. After laying out the respective theoretical and historical arguments, I provide both types of evidence.

4.2 LOCAL COLONIAL RULE: FRENCH AND BRITISH STYLE

As the chapter's introductory quotes of French and British Governor-Generals in West Africa illustrate, the official stance towards indigenous authorities differed substantially between the two colonizing powers. Especially in the formative early years of colonial rule,[4] the French strove to establish a rather uniform system of direct rule, a system based on French, rather than pre-existing institutions.[5] In comparison, the British approach to colonial rule was, at least in theory but constrained by local conditions, based on the ideal of regional and local self-government (Crowder, 1968).[6] These differences originated in diverging French and British governance ideologies as well as in lower numbers of metropolitan personnel available to British colonial governments that ruled over a much bigger empire.

In the republican spirit of the French revolution (Cohen, 1971*a*), the French colonial administration strove to mold their colonial subjects into the body of '100 million Frenchmen' (Lewis, 1962, also Crowder, 1968; Crowder and Ikime, 1970). Wherever possible, the power of precolonial elites was crushed (Conklin, 1997; Weiskel, 1980), leading to widespread resistance by the most powerful polities (Huillery, 2010). Notwithstanding this hostility, the French colonial administration depended on native intermediaries to collect taxes, enlist forced labor, recruit soldiers, and maintain the

4 Note that colonial rule in areas with large settler communities did not necessarily follow the logic presented below. Large European settler communities came necessarily at the expense of indigenous polities and thus with much more direct forms of colonial rule. However, causation might have also run the other way around: areas without strong and surviving precolonial institutions might have attracted larger number of settlers, a pattern that Huillery (2010) finds in French West Africa.

5 At least until the end of WWI. Conklin (1997) suggests that the later "politique d'association" was more attentive to local institutional conditions, which where, by the time of that change, however largely destroyed (on the Baule in Côte d'Ivoire, see Weiskel, 1980, also Crowder, 1968, pp. 189-190).

6 Beyond the literature cited below, a number of single and comparative case-studies support this account (e.g. Crowder and Ikime, 1970; Hailey, 1945), including well-designed accounts of French direct and British indirect rule over the same ethnic group living on both sides of a colonial border (Miles, 1994; Asiwaju, 1970).

local infrastructure. The responsible local chiefs were oftentimes appointed based on their loyalty to the French Empire rather than their precolonial status (Crowder, 1968). Stripped of their traditional authority, they were converted into colonial agents under the supervision of the *commandants de cercle* (Cohen, 1971*a*,*b*; Suret-Canale, 1988; Roberts, 1929). This supervising French administrative staff had to change position often enough to be agnostic of local languages and customs (Cohen, 1971*b*; Crowder, 1968). Instead, becoming a chief required the knowledge of French, a policy that inhibited the continuous functioning of indigenous institutions (Crowder, 1971*a*) and furthered the standardization of local governance.

The British choice of local governance institutions was different. Aiming to use the legitimacy of local rulers for their own purposes, the British collaborated with indigenous institutions, left them with much of their accustomed executive, legislative, and judiciary powers, and integrated their structure and personnel into the colonial state (Hailey, 1945; Lange, 2009). British district officers had a primarily consultative role vis-à-vis indigenous rulers, encouraged self-government, and provided technical assistance (Crowder, 1968; Lugard, 1965).

However, not all precolonial polities could be ruled indirectly. The degree to which British administrations could integrate traditional institutions was primarily a function of their degree of precolonial centralization and pre-existing hierarchies (Fortes and Evans-Pritchard, 1940; Gerring et al., 2011; Hicks, 1961). The idea of local self-government proved practicable where the British could co-opt centralized political institutions. Where political power was too decentralized and in the hands of fragmented institutions foreign to the British, they pragmatically set up new governance schemes under the direct control of the administration that connected precolonial village-level elites to the center (Crowder, 1968; Hicks, 1961; Tignor, 1971).

Centralized precolonial institutions with multiple layers of administrative and political hierarchies characterized the Kingdom of Buganda in Uganda (Reid, 2002), the Fulani Emirates in Northern Nigeria (Miles, 1994), or the Ashanti confederation in the Gold Coast (Wilks, 1975). The colonial administration was able to make cheap use of these institutions by letting their rulers choose between collaboration and death or exile (Gerring et al., 2011). As a result of such bargaining under the threat of violence, prior elites shared their rent from ruling with and implemented policies for the British. At the same time, they were able to preserve much of their accustomed power and autonomy. The resulting indirect governance schemes featured two main characteristics. First, the British employed only a minimal amount of administrative resources to the indirectly ruled regions – just enough to monitor the actions of local elites and collect the rents of the colonial state. Second, the local elites continued to enjoy many of their accustomed powers and presided over local governance entities that encompassed much of the institutions, hierarchies, and the territory of 'their' pre-existing polity (Crowder, 1968; Perham, 1937).

Other regions lacked centralized political structures. Before the colonial conquest, they were instead ruled in a decentralized manner, for example by village councils prevalent in the acephalous parts of Southeastern Nigeria or the Northern parts of Kenya and Uganda (Mair, 1977). Administering such areas posed substantial problems to British administrators who tried to implement a system of indirect rule. Due to the lack of institutions above the village level, it was not possible to simply coerce one powerful ruler to gain control over a large population and territory. Because ruling each village from the colonial capital would have been costly and inefficient, the colonial state had to build its own administrative system to link the colonial capital, via the region- and district-level with each village (Crowder, 1968; Fortes and Evans-Pritchard, 1940; Hicks, 1961; Tignor, 1971).[7] Whether staffed with colonial administrators or neo-"traditional" authorities such as the "Warrant Chiefs" lifted to power in Southeastern Nigeria (Afigbo, 1972; Perham, 1937), the new system amounted to direct rule. The newly installed state agents were largely independent and partly agnostic of the populations they ruled over. They came to power at the whims of the British colonial government and did not depend on institutionalized ties to the population they ruled (Hicks, 1961; Tignor, 1971).[8] In effect, this led to governance constellations that were similar to those observed in the French colonies. Two main characteristics describe the resulting mode of direct local rule. First, the

7 Even more difficulties arose in areas inhabited by pastoralist groups without centralized institutions such as the the Turkana and Karimojong in Kenya and Uganda, because of the need of the colonial state to have territorial stability in the location of capitals and borders of districts (Mair, 1977, ch. 11).

8 A certain tendency to confound traditional and non-traditional authorities as actors in schemes of indirect rule permeates the literature on indirect rule in Europe and Africa (e.g. Hechter, 2000; Mamdani, 1996). From a theoretical standpoint, it is unclear how indirect rule, which rests on traditional institutions, their elites and powers, can build upon non-traditional institutions which lack the means of (violent) coercion to govern a local population and instead rely on the central state for the provision of these means. The provision of such resources, e.g. police forces, administrators, etc., amounts to direct rule. If the center loosens the control over these resources, such rule can over time become embedded into the local community and take on an ever more indirect character.

British colonial government employed a substantial amount of personnel and resources in order to run the administrative infrastructure that linked the colonial center with local populations. Second, the low-level indigenous elites presided over native governance entities that were much smaller in territorial and substantive scope than the indigenous governance units under indirect rule.

Of course, exceptions to the French and the British approach to local colonial rule existed. The French, for example, never succeeded to fully subject the Mossi Empire in Upper Volta, today's Burkina Faso. They finally settled on a cooperative relationship but reserved substantive administrative and judicial powers for themselves (Skinner, 1970). Similar deviations from the 'pure' British model marked the colonization of the Ashanti kingdom in Ghana. The kingdom was, after its violent conquest in 1896 and the exile of the Asantehene, first put under direct rule. However, continuous nationalist mobilization of the Ashanti population convinced the British of the downsides of that model. Between 1919 and 1935, the traditional authorities were gradually allowed to resume their positions (Crowder, 1968, pp. 230-233; Tordoff, 1968), thus establishing indirect rule over Ashanti.

Why did the strategies of local rule between the French and the British colonial empires differ? The theoretical argument developed in Chapter 3 suggests that the answer consists in differences in the costs and benefits of direct rule faced by the respective governments. Two differences between French and British colonial governments stand out in this regard. One concerns the colonies' fit into the respective metropolitan administrative systems, whereas the other relates to the administrative resources at the disposal of their governments.

First and most prominent in the literature, the administrative architecture of the colonizing power shaped the one implemented in its colonies. Here, the French were greatly influenced by the centralized governance and republican spirit of the Île-de-France (Cohen, 1971*a*; Conklin, 1997). Already the military officers that conquered the French colonies established a strictly hierarchical system of military administration. Assuming their role, the later civil administrators brought with them the centralizing tendencies of the French government and, as republicans, despised the hereditary aristocrats that were still around (Crowder, 1968, p. 188). A policy of indirect rule would thus have stood against the French metropolitan blueprint, thus lowering the comparative price of direct rule.[9] In contrast, Great Britain's approach to governance was more diverse, including self-rule in the settler colonies of Canada and South Africa, and solving the 19th century conflict over Irish 'Home Rule' through Southern Irish autonomy and the creation of the Parliament of Northern Ireland (Bogdanor, 2001). This diversity and the earlier experience with indirect rule in India made its application in the British colonies in Africa less costly.

The second difference in the costs of direct and indirect styles of rule concerns the administrative resources at the disposal of colonial governments. We have already seen that French governments employed nine times as many European officers than their British counterparts (Herbst, 2000; Kirk-Greene, 1980). This was not necessarily by choice. Instead, the reason might consist in the size of the British colonial empire, which exceeded that of the French by an order of magnitude. While France with its 40 million inhabitants ruled over 55 million colonial subjects across the globe in 1921, 44 million British citizens ruled over approximately 400 million colonized subjects (Roberts, 1929, xvi). Because indirect rule required less European administrative personnel at the cost of less efficient and lucrative rule, the difference in size of the empires relative to the imperial population offers a swift explanation for the heavier reliance on indirect rule in the bigger empire. If both empires could draw on a similarly big pool of well-educated potential colonial administrators in their metropolitan population, it comes as no surprise that French colonial administrations employed nine times more administrators per African subject than British colonial governments. This made indirect rule a pragmatic response to the lack of resources needed to establish and maintain direct control everywhere (see also Lugard, 1965, p. 141).

The summary of historical evidence on the mode of local rule in British and French non-settler colonies in Africa suggests two main axes of variation in the indirectness of colonial rule. First, the accounts suggests that French colonial governments were comparatively hostile towards precolonial institutions and aimed at replacing them with institutions that resembled the blueprint of the Île-de-France. The less republican and more resource-constrained British colonialists in turn championed local self-governance through pre-existing institutions to complement the central colonial government in a Lugardian scheme of 'dual rule':

9 One might even go so far as to ask about spillover effects to the metropolis itself, where examples of local self-governance in the colonies might have raised demands for more local autonomy.

H 4.1: *French colonial rule was more direct than British rule.*

Second, the British were not able to establish schemes indirect rule where they confronted decentralized and fragmented precolonial institutions.

H 4.2: *The indirectness of British colonial rule increased in the political centralization of precolonial political institutions.*

The following empirical sections present empirical evidence for both hypotheses, first studying the survival of rulers under French and British colonial rule, and then turning to variation in indirect rule within British colonies.

4.3 THE SURVIVAL OF PRECOLONIAL INSTITUTIONS

To test Hypothesis 4.1 that French colonial rulers more often crushed and replaced precolonial political institutions than British colonial governments, I exploit data on the continuation of the lines of succession in 124 colonized African polities in the 19^{th} and 20^{th} century. A survival analysis shows that in each year, lines of succession of precolonial polities under French rule had a four times higher risk of being terminated than those of polities under British rule. This difference persists in comparisons of polities across arguably arbitrary colonial borders.

4.3.1 *Panel data on precolonial polities*

To analyze the survival of precolonial institutions under colonial rule, we need panel data on colonized polities. Currently used data on precolonial institutions in Africa, most prominently Murdock's (1959; 1967) *Ethnographic Atlas*, are valuable for its detailed cross-sectional and geographical information, but lack the dimension of time. To fill this void, I digitize historical data on 124 African states before and during French and British colonial rule, collected by Stewart (2006) in his encyclopedia of *African States and Rulers*. First published in 1989 and updated since then, the encyclopedia enlists indigenous, colonial, and postcolonial states in Africa. Each entry comes with a short account of a state's history and date of colonization as well as a detailed enumeration of its rulers and capitals.[10] These data originate from a

10 In each year, a state has only one capital, but some capitals are relocated over time. I geocode all capitals via the `geonames.org` and `maps.google.com` APIs.

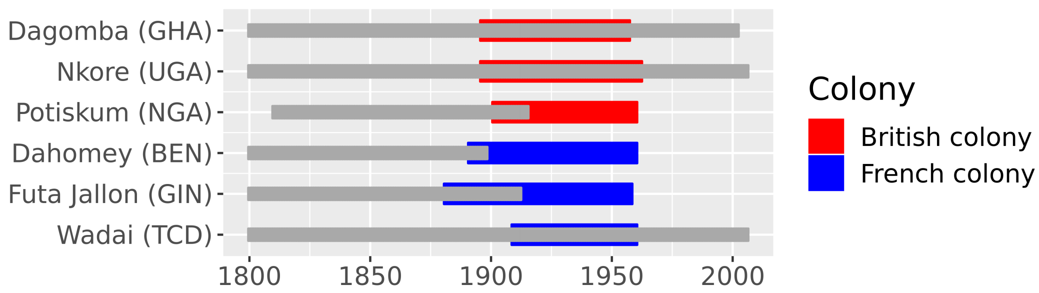


Figure 4.1: Lines of succession in six precolonial states under colonial rule.
Note: Censored before 1800. Grey rectangles denote the observed lifespan of each line of succession. Abbreviations of colonies' postcolonial name in parentheses.

comprehensive list of sources, among them historical case studies, the Journal of African History, as well as other encyclopediae such as the Cambridge History of Africa.

The main information used in the empirical analysis is the continuation of a polity's line of succession in each year of the colonial occupation by either the British or the French empire. This data is available because Stewart continues to enlist polities' rulers – so they existed – throughout the colonial and postcolonial period (Figure 4.1). I take advantage of this coding and take the continuing line of succession in a precolonial state as a proxy for its institutional survival under colonial rule. In particular, I code the end of the line of succession in the year after which Stewart enlists no further rulers for the respective polity.[11] While the survival of institutions is not always equivalent to the survival of its personnel, the *dissolution* of a political dynasty is a prominent indicator for the dismantling of the institutions they presided over. This is particularly relevant for precolonial states in Africa, most of which were traditional regimes that derived authority and legitimacy from hereditary rule or customary appointment (Weber, 1958).

The states covered by Stewart (2006) overwhelmingly belong to the class of centralized precolonial polities. As Appendix A.1 demonstrates, settlement areas of ethnic groups that were coded as precolonial states (acephalous societies) by Murdock (1959), feature a polity in Stewart's data in 60% (4%) of all cases. This is not surprising, given that Stewart was primarily interested in collecting polities' rulers, which are hardly identifiable in acephalous societies. Furthermore, acephalous societies lack the

11 This particular coding ensures that polities do not 'die' if there is an interregnum without a ruler, as there sometimes was.

institutions to produce traces that could be uncovered by historians who produced Stewart's main sources (see e.g. Scott, 2017).

This type of bias makes the data unsuitable for providing a representative description of all precolonial polities in Africa. However, valid inferences about differences in polities' survival rates under British and French colonial rule are possible if the type of colonial rule does not bias the coding. Stewart's data show few signs of such bias. A set of analyses in Appendix A.1 suggests that areas and ethnic groups colonized by the British do not have a significantly different probability of comprising a state coded by Stewart, and do not feature a significantly different history of such states. If anything, *less* information on precolonial polities seems to be available from French colonies. Information likely got lost from those polities that did not survive colonization. This would bias the analysis against Hypothesis 4.1, that is towards a higher probability of survival under French rule.

4.3.2 *Analysis*

Figures 4.2 and 4.3 provide first descriptive evidence for a large difference in the survival rates of polities under French and British rule. Zooming in on West Africa, the map in Figures 4.2 suggests that in French West Africa only a few polities reached independence unbeheaded. Those that did where mostly located in the far-away regions of Niger (Zinder) and Chad (e.g. Wadaï), or were too strong to be subjected, like the Mossi kingdoms mentioned above. The picture looks different for the British colonies in the same area. In the Gold Coast, all coded polities survived, albeit not always unscathed as the Ashanti kingdom illustrates. In Nigeria, where the 19$^{\text{th}}$ Fulani Jihad created a large number of emirates, less polities withstood colonial conquest and rule, but proportionally many more than under French rule. A simple comparison of the proportions of polities that reached independence supports this impression: 26% of polities colonized by the French and 60% colonized by the British survived colonial rule (see Figure 4.3).

This difference remains stable once I model the end of lines of succession during colonial rule in a Cox Proportional Hazard Model. The Cox model includes a set of covariates geographically attributed to polities via the location of their capital. In particular, I first include a vector of (1) baseline controls comprising the local

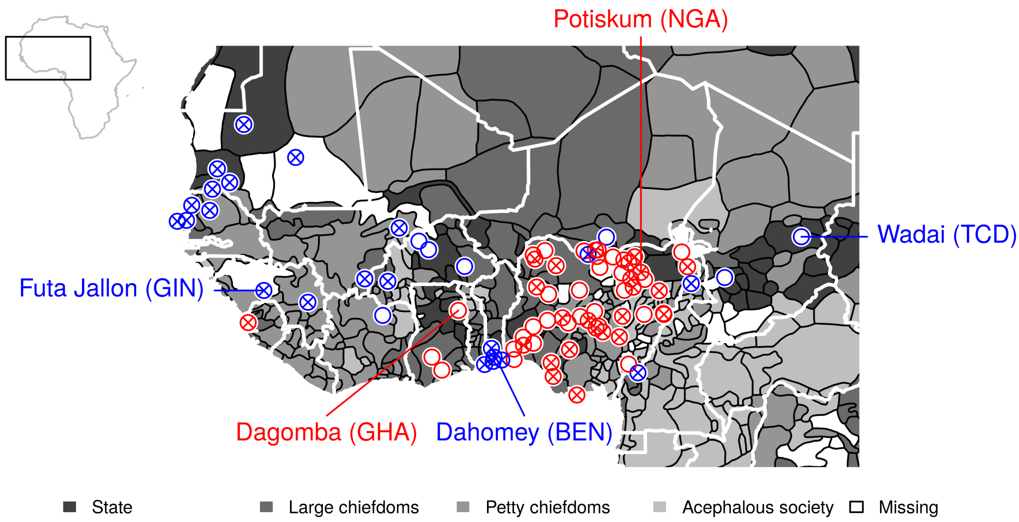


Figure 4.2: Map of colonized polities in West Africa. Polities marked by a cross saw their line of succession terminated before their countries gained independence. For a full map of Africa, see Appendix A.1.

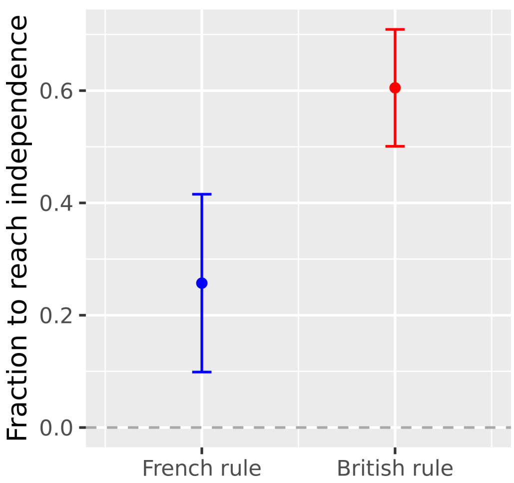


Figure 4.3: Fraction of colonized lines of succession to survive until the year of independence from colonial rule. Error bars denote 95% confidence intervals from a linear regression without controls. For a full set of cross-sectional analyses, see Appendix D.1.2.

population density, the polity's age, its distance to the coast and nearest navigable river,[12] and a simple linear time trend. To control for observable differences between the regions colonized by the French and the British, I add a vector of natural characteristics around the area of a polity's capital,[13] and finally a vector of ethnic characteristics[14] of the ethnic group settling around it.

Table 4.1: British vs. French rule and the demise of precolonial polities: Cox Proportional Hazards

	End of line of succession		
	(1)	(2)	(3)
British rule	-1.461^{***}	-1.519^{***}	-1.785^{***}
	(0.352)	(0.385)	(0.600)
Population/km^2 (1880, log)	-0.016^{**}	-0.031^{***}	-0.012
	(0.008)	(0.010)	(0.015)
Distance to coast (log)	-0.228^{**}	-0.215^{**}	-0.102
	(0.089)	(0.095)	(0.134)
Distance to river (log)	-0.134^{*}	0.035	0.202
	(0.077)	(0.143)	(0.239)
Polity age (log)	0.062	0.028	0.121
	(0.091)	(0.115)	(0.157)
Year	-0.424^{**}	-0.681^{***}	-0.764^{***}
	(0.188)	(0.175)	(0.281)
Nature controls:	no	yes	yes
Ethnic controls:	no	no	yes
Observations	5,208	4,902	4,581
R^2	0.009	0.011	0.009
Max. Possible R^2	0.073	0.068	0.055
Log Likelihood	-174.720	-146.030	-108.440

Notes: Cox Proportional Hazard models. Standard errors are clustered on the polity-level. Nature controls consist of median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls consist of the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

Table 4.1 reports the results. The transformation of the coefficients of British rule into hazard ratios shows that the lines of succession under British rule had, in every

12 Data on rivers come from Jedwab and Moradi (2016).

13 In particular, its altitude, ruggedness, temperature, average precipitation, evapotranspiration, the ratio of its evapotranspiration and its precipitation, as well as its suitability for cash crop production (all from FAO, 2015) and agriculture in general (Ramankutty et al., 2002). See also discussion below.

14 These are the reliance of local ethnic groups on agriculture and pastoralism, as well as the intensity of their agricultural activities (Murdock, 1959).

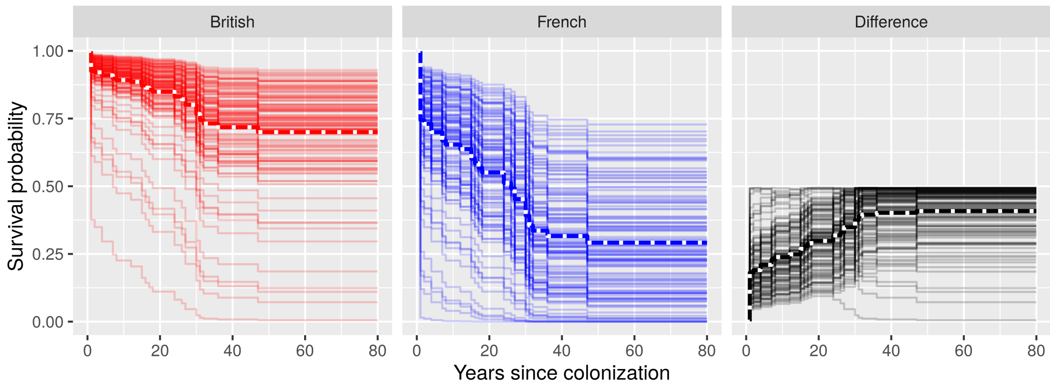


Figure 4.4: Survival curves as predicted for all polities under either British or French rule. Based on Model 1 in Table 4.1, the Figure plots the predicted survival of every polity under British and French colonial rule. The third panel plots the polity-level difference between these two predictions. Thin lines plot the polity-level predictions, bold lines plot the average across all predictions.

year, a probability of ending that was about a quarter of that of a polity under French rule. The gap between the empires increases in Models 2 and 3 with additional controls. This suggests that, if at all, the British settled in areas with a disposition for more frequent extinctions of precolonial polities. The differential yearly hazard rate between the two empires translates into a large and increasing toll precolonial polities took from French colonization, visualized by the survival curves in Figure 4.4. As the imperial domination of the continent ended after approximately 80 years, the model predicts only 1 out of 3 polities under French rule to have survived. Under British rule the toll is also substantive but much lower with a 70% chance of survival, largely equivalent to the raw comparison discussed initially.

To test the robustness of these findings, I conduct a series of sensitivity analyses in Appendix D.1.1. In particular, I test whether the results are driven by (1) a potentially overwhelming weight of some colonies (e.g. Nigeria with its many Fulani emirates) by giving each colony equal weight, (2) by differential temporal dynamics of the French and British colonization by stratifying the regressions by year, (3) the local disease environment, (4) by miss-specified clustering of standard errors, clustering instead not at all, on the colony, and ethnic group level, and (5) by the chosen functional form of the models, instead using a simple linear hazard model. All these variations lead only to small deviations from the estimates reported above. A final analysis (6) focuses on the effect of British and French colonial rule on the average tenure time of rulers as compared to tenure times before colonization in the same polity. The effect associated with French colonization on the yearly risk of a ruler's deposition or death is 1.5 times the effect associated with British colonization (see Appendix D.1.3).

So far, the main identifying assumption of the model is that colonization by either the French or the British was as-if random. This assumption may be difficult to uphold in the face of the geographic patterns of colonization. A series of further analyses in Appendix D.1.4 therefore successively limits the sample of compared polities to ever-smaller regions, first polities in West Africa and second to those in the coastal colonies in West Africa only. Finally, I exploit arguably arbitrary borders in West Africa that run perpendicular to the coast line as a result of the quick scrambling for territory after 1885 (Cogneau and Moradi, 2014; Wesseling, 1996). By estimating a Cox Proportional Hazard Model stratified by polities' closest perpendicular empire-border, the model compares polities only across these borders.[15] These three sets of analysis yield estimates of the hazard ratio that are generally larger than the one reported at baseline and statistically significant.[16] These findings suggests that the baseline results are, if at all, downward biased.

The unique data from Stewart's (2006) encyclopedia of African states and rulers show that precolonial polities' demise was much more frequent under French than under British colonial rule. This result supports Hypothesis 4.1 that the French ruled more directly and against pre-existing institutions than the British, who favored local self-government through traditional institutions. However, the focus on the survival of centralized polities masks variation in the schemes of colonial governance set up in regions with and those without such pre-existing institutions. This variation is the subject of the following empirical analysis.

4.4 PRECOLONIAL INSTITUTIONS AND INDIRECT RULE

Building on the demonstration that more precolonial institutions survived British than French rule, this section turns to Hypothesis 4.2 that British indirect rule was constrained where precolonial societies lacked centralized political institutions that could be readily integrated into the colonial state. To test whether the indirectness

15 Note that the sparseness and clustering of the data in space does not allow for estimating a clean discontinuity in survival rates at the border.

16 P-values of the cross-border analysis drop to $p < .1$ and the size of the coefficient increases substantively as I add the vectors of control variables.

of rule increased with the level of precolonial centralization, I present archival data that shed light on various dimensions of local rule in British colonies. In addition, the empirical analysis exploits the difference between the French and the British empires where comparable data from French colonies exists. In British colonies, the level of indirect rule substantially increases in the centralization of local precolonial institutions. Consistent with the previous finding that the French relied on direct rule, such a relation does not exist in comparable data from French West Africa.

4.4.1 *Data*

UNIT OF ANALYSIS: Analyzing colonial indirect rule in British and French colonies in Sub-Saharan Africa, the main unit of analysis is the colonial district (or cercle), the second-level administrative unit in the colonies. I collect and digitize district-maps from eight colonies: Sierra Leone, the Gold Coast, Nigeria, Uganda, Kenya, Tanganyika, Nyasaland, and Northern Rhodesia.[17] For French West Africa, I draw on district maps from Huillery (2009).

MEASURING INDIRECT RULE: As outlined in the theoretical discussion, there are two observable facets to indirect rule, the colonial power's administrative effort and local traditional institutions' power. To provide evidence on indirect rule as complete and systematic as the available data allows, I shed light on both dimensions. All data comes from archival administrative reports for the British colonies and from Huillery (2009) for French West Africa. Appendix C.1 lists additional details on the sources and coverage of the data.

Table 4.2: Data on the indirectness of colonial rule: Overview

	Gold Coast	Kenya	Nigeria	Northern Rhodesia	Nyasaland	Sierra Leone	Tanzania	Uganda	French West Africa
District size	yes	yes	yes	yes	yes	yes	yes	yes	yes
European administrators	–	–	yes	–	–	–	–	yes	–
Local budgets	yes	–	yes	–	yes	–	–	yes	yes
Class of chiefs	–	–	yes	–	–	–	–	–	–

17 See Appendix C.1. These are the same eight colonies for which the road network data in Chapter 5 exist. With that, the sample does not cover the settler colonies in Southern Africa.

The first dimension of indirect rule concerns the local administrative effort employed by the colonizing government. More indirect rule comes with less administrative effort, measured in two ways:

1. *Size of districts:* This is the simple area of districts, net of water surface ($N_{GB} = 294$; $N_F = 114$). Each district needs a minimal level of administrative resources, most importantly a district officer or commandant de cercle. Thus, dividing a region into more and thus smaller districts requires more administrative resources. Furthermore, smaller districts come with a lower distance between headquarters and the population (e.g. Grossman and Lewis, 2014; Grossman, Pierskalla and Dean, 2017). Given the importance of administrative tours (Herbst, 2000), this implies more frequent visits of any village and more direct rule.

2. *European administrators:* This measure draws on data on the number of British administrators employed at the local level. Unfortunately, such district-level officer lists are scarce, so the sample is reduced to Nigerian provinces and districts in Uganda (N=35).

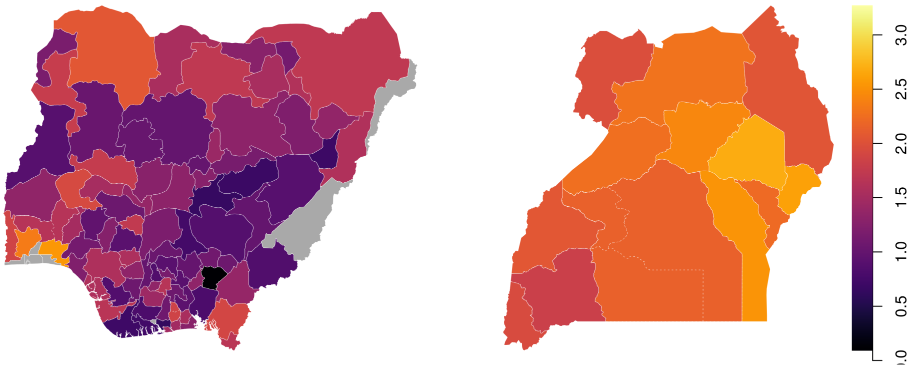
Two measures cover the flipside of indirect rule, the power of the traditional institutions through which schemes of indirect rule were carried out:

1. *Native treasury budgets:* As argued above, British colonial governments co-opted strong and hierarchical institutions because they generated rents in a centralized manner and allowed for relatively cheap top-down implementation of policies. Thus, indirectly ruled areas should feature native treasuries with larger budgets. The respective data comes from the British "Annual Departmental Reports" for the Gold Coast (Ghana), Nigeria, Nyasaland (Malawi), and Uganda (1931–1956).[18] The reports include budget totals as well as detailed breakdowns of revenues and expenditures into subcategories (see Appendix D.3.1).

   In total, the collected data cover native treasuries in 148 districts across the four colonies and a varying number of years. To avoid biases emerging from an unbalanced panel,[19] I match native treasuries to the colonial districts introduced

18 Note that the data collection proceeded in two stages. I first processed the scans of the tables to extract their content automatically. I then cleaned each entry in every table to ensure that no errors affect the results. See Appendix A.2.1 for details.

19 Because reporting standards of the British Administrations varied across colonies and over time, the length of the time series available for each colony varies considerably (see Figure A.4). Furthermore,



(a) Nigeria 1931-1939 (b) Uganda 1934-1956

Figure 4.5: Per-capita revenues of native treasuries (logged; 2016 £). Aggregated to the district level and averaged over all observed years. Dotted lines indicate borders along which I aggregate districts for the analysis of local budgets (see discussion in text).

above and average the budget data for each district across all years (see Figure 4.5).[20] If a budget in one year covers more than one district (e.g. the budget of the Kabaka of Buganda, which covered an entire region), I aggregate the data to the highest spatial 'denominator' in all years, again for reasons of spatio-temporal consistency.

Table 4.3: Summary of native treasury data

Colony	Districts	Start	End	No. of years	Avg. revenue	Avg. expenditure
Gold Coast	29	1949	1951	3	9.92	9.18
Nigeria	86	1931	1939	9	3.12	3.04
Nyasaland	19	1934	1955	17	1.29	
Uganda	13	1934	1956	22	8.86	10.60

Notes: Note that the number of observations in the data might be smaller than the number of existing districts, because some budget reports report numbers above the district level (e.g. Buganda, Uganda).

2. *The power of Nigerian chiefs:* The second indicator of the power in the hands of local authorities consists in the rights and recognition received by chiefs in Nigeria, measured through their official 'class'. "First Class" chiefs possessed of the greatest powers and authority over subsidiary chiefs. "Third Class" chiefs had a limited realm in terms of the population they ruled and the rights they

native treasuries were at times newly created or merged, leading to a variation in spatial units that is difficult to track over the years, since no time-varying information on native administrations' boundaries is available.

20 I also explore two alternative ways to deal with this caveat, (1) weighted panel regressions, and (2) hierarchical modeling. See below and Appendix D.2.1.

enjoyed (Lugard, 1965, p. 212 and Appendix A.3). Based on a list compiled by the British War Office in 1929 (pp. 95-96),[21] the class of the highest-ranking chief per district serves as an indicator for the power of indigenous institutions.

While the data encode diverse aspects of local colonial governance, the four outcomes correlate with each other (correlation coefficient of between .38 and .65; see Appendix A.3). For example, larger districts feature fewer European administrators per capita, more revenues by native treasuries and have chiefs with more power. These patterns suggest that the indicators indeed conjointly capture the concept of indirect rule.

PRECOLONIAL INSTITUTIONS: To test Hypothesis 4.2 that the local level of precolonial political centralization determined the degree of indirect colonial rule, detailed cross-sectional data of local institutions at the time of colonial conquest are needed. The best source for such data still consists in the Ethnographic Atlas compiled by Murdock (1967). Based on early ethnographic research, the Atlas classifies the levels of administrative hierarchies of ethnic groups' precolonial political institutions. The coding ranges from 0 to 4 levels, from *no political authority beyond the community*, via *petty chiefdoms* and *larger chiefdoms*, to *states* and *large states*. Using the spatial information from Murdock's ethnic map (1959, Nunn and Wantchekon, 2011),[22] the average precolonial centralization of each districts' area measures the number of administrative levels that could be used for indirect rule by the colonial power.

With this coding of hierarchical levels, the data is better able to capture the ease with which different political systems could be ruled indirectly than other data sets, such as maps of state histories (e.g. Depetris-Chauvin, 2014) or Stewart's (2006) encyclopedial data of precolonial polities and their rulers. The latter come with the additional disadvantage that only the coordinates of polities' capitals but not their borders are known. This makes their attribution to relatively small spatial units such as districts subject to large errors where precolonial states were carved up into multiple districts subject to an overarching native authority.[23] Furthermore and as discussed above, Stewart's (2006) data are likely incomplete, shown by the fact that only 60% of groups coded by Murdock (1967) as a large precolonial state feature

21 The list covers Northern Nigeria for the year 1928 and Southern Nigeria in the year 1924.
22 The matched data is available here: `http://worldmap.harvard.edu`. Michalopoulos and Papaioannou (2013*b*) provide a slightly different match between the two data sources. Using their mapping does not changes the results (see Appendix D).
23 E.g. the multiple districts ruled over by the Kabaka of the Buganda in Uganda.

a polity in Stewart's data. I do however check the consistency of the results with Steward's data below.

4.4.2 *Empirical strategy*

With this data on district-level colonial and traditional rule at hand, I estimate the effect of precolonial centralization on the various indicators of indirect rule in a simple linear modeling framework:

$$\mathsf{y}_{i,p,c} = \alpha_c + \beta_1 \mathsf{precol.\ centralization}_i + \boldsymbol{X}_1\boldsymbol{\Lambda}_i + \boldsymbol{X}_2\boldsymbol{\Omega}_i + \boldsymbol{X}_3\boldsymbol{\Psi}_i + \epsilon_{i,p} \tag{4.1}$$

In particular, I reduce the variation in $\mathsf{precol.\ centralization}_i$ exploited by each model to within-colony variation by using colony-fixed effects α_c. In specifications in which patterns of indirect rule in British and French colonies are compared, the models include the interaction term $\mathsf{French}_c \times \mathsf{precol.\ centralization}_i$. Since precolonial centralization is not randomly assigned to districts i, I successively include three vectors of control. First, vector $\boldsymbol{\Lambda}_i$ captures only the baseline characteristics of a district: the average spatial density of the population of the district and the ethnic groups that inhabit it (Goldewijk, Beusen and Janssen, 2010),[24] as well as a district's' distance from the coast and closest navigable river, all logged.[25] Second, $\boldsymbol{\Omega}_i$ controls for what is subsequently called the 'natural' attributes of a district: its altitude, ruggedness, temperature, average precipitation, evapotranspiration, the ratio of its evapotranspiration and its precipitation, as well as its suitability for cash-crop production[26] (all from FAO, 2015) and agriculture in general (Ramankutty et al., 2002). Third, I add a vector of controls $\boldsymbol{\Psi}_i$ for the socio-economic characteristics of districts that might cause strong local governments and precolonial centralization. These are the reliance of local ethnic groups on agriculture and on pastoralism as well as the intensity of their agricultural activities (Murdock, 1967). I project

24 This is the mean population density in the settlement areas of the ethnic groups whose settlement areas overlap with a district. The mean is weighted by the area of overlap.

25 All may directly relate to indirect rule and the level of precolonial centralization: for example, in densely populated areas, we would expect more centralization and smaller districts that keep the size of the population constant. In areas farther away from the coast, colonization came later and less forceful, causing more indirect rule. Data on navigable rivers comes from Jedwab and Moradi (2016).

26 I create the index of cash crop suitability by taking the local maximum of soils' suitability for the eight most important cash crops: cocoa, coffee, cotton, groundnut, oil palm, sugarcane, tea, and tobacco.

these variables onto a spatial grid and then aggregate them to the district level by taking their means. French-British models include interactions of all controls with a French dummy. Lastly, standard errors are clustered on the provincial, thus first-level administrative unit level p to account for potential dependencies among neighboring districts subject to the same regional authority.

4.4.3 *Results*

The presentation of the results follows the structure of the data. A first set of analyses finds that administrative effort, measured through the size of districts and the local number of British administrators, decreases in the centralization of precolonial institutions. A second set of analyses completes the picture and shows that the power of indigenous, that is "native", administrations increases in the level of precolonial centralization. While these relationships are overall robust for British colonies, the data from French colonies exhibit no significant or even opposite associations. Together, the results support the argument that precolonial centralization was a strong determinant of British indirect rule.

Evidence on administrative effort

THE SIZE OF DISTRICTS Districts were larger in areas with strong precolonial institutions in British but not in French colonies. The estimated effect of precolonial centralization on the size of British districts is substantial: a move from an acephalous society to a precolonial state, that is, an increase in the value of precol. centralization by 3, is associated with an increase in the size of districts by between 35 and 54 percent (Table 4.4). The reverse relationship appears in French West Africa, where the data suggests that an increase in the level of precolonial centralization by the same three levels *decreased* districts' size by about 45 percent (Figure 4.6).[27] If larger districts correspond to lower levels of administrative effort exerted by the colonizers, the evidence thus supports the argument that pre-existing institutions facilitated British indirect rule.

This relationship between indirect colonial rule and the size of local governance units remains robust to a number of permutations of the model. A set of sensitivity

27 Because district sizes are logged, these percentage changes result from calculating $(exp(\beta * 3) - 1) * 100$.

Table 4.4: Precolonial centralization and the size of districts

	log(District Area)		
	(1)	(2)	(3)
Precol. centralization	0.143**	0.098**	0.131***
	(0.065)	(0.048)	(0.049)
Precol. centralization × French	−0.304**	−0.294***	−0.334***
	(0.120)	(0.108)	(0.106)
Colony FE:	yes	yes	yes
Baseline controls:	yes	yes	yes
Nature controls:	no	yes	yes
Ethnic controls:	no	no	yes
Mean DV	9.11	9.14	9.14
Observations	404	400	400
Adjusted R^2	0.673	0.717	0.721

Notes: OLS models. Standard errors are clustered on the province-level. Baseline controls include the local population density, ethnic groups' population density, and the distance to the coast as well as the closest navigable river. Nature controls consist of the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls are the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Additionally, all covariates are interacted with 'French rule'. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

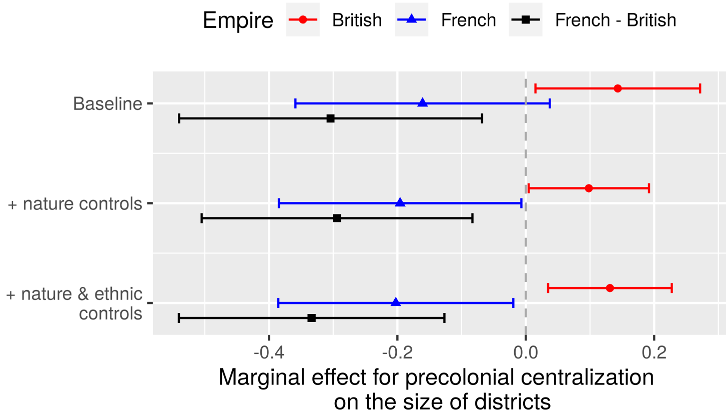


Figure 4.6: Marginal effect of precolonial centralization on district size. Based on Models 1-3 in Table 4.4.

analyses yields that the results are (1) not driven by very big or very small districts, (2) not due to a potential over-weighting of colonies with many districts such as Nigeria, and (3) robust to including a vector of co-variates that control for the disease environment of a district, which might relate to precolonial statehood and colonial administrative difficulties. Lastly, the results are consistent if I use the coding of precolonial centralization from Michalopoulos and Papaioannou (2013*b*) or the newly collected data on precolonial polities as a measure of precolonial centralization: Districts that featured a capital city in 1885 are approximately 65 percent bigger in British colonies, but of average size in French colonies.

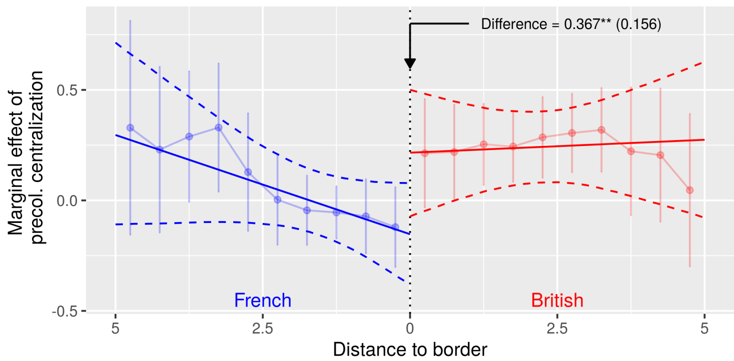


Figure 4.7: Regression discontinuity plot of the effect of precolonial centralization on districts' size at French-British borders perpendicular to the West African coastline.
Solid lines illustrate the results from a linear trend-model with a bandwidth of 5 decimal degrees. Transparent point estimates plot the effect of precolonial centralization in .5 decimal degree bins of the distance towards the French and British sides of the borders. See Appendix D.2.2 for all details.

Imbalances in covariates suggest that the divergence of the effects of precolonial centralization in British and French colonies might be driven by endogenous colonization choices of both empires. To address this caveat, I exploit the arguably arbitrary borders between the two empires that run perpendicular to the West African coastline. Fully presented in Appendix D.2.2, the regression discontinuity design identifies the difference in the effect of pre-existing institutions on districts' sizes at the French-British borders. Using grid cells as the unit of analysis[28] and improving but not fully securing balance on pre-treatment covariates (see Table D.9),[29] the analysis

28 I use grid cells as the unit of analysis here as the number of districts in an area by definition decreases with their size. In the RD-design, this would lead to a jump in the number of observations at the border, which may bias the results. Grid cells have a size of .0833 decimal degrees in the baseline RDD-analysis, and a varied in a robustness check.

29 At a bandwidth of 5 decimal degrees around the borders balance is best with imbalances affecting grid cells' distance to the coast, altitude, and suitability for agriculture.

supports the conclusion drawn from the baseline model. As Figure 4.7 illustrates, the estimated difference is statistically significant ($p < .05$) and shows that precolonial centralization had an effect on district sizes .37 log-points smaller in French than in British colonies.[30] Combined with additional regression discontinuity specifications,[31] this suggests that the differential patterns observed in the French and British colonies are not the result of spatial sorting of the colonizers.

EUROPEAN ADMINISTRATORS In addition to district sizes, I draw on data on the spatial distribution of British administrators to measure the extent of indirect rule. Because such data is less abundant, I have only access to the number of British administrators in the 22 Nigerian provinces and 12 Ugandan districts. The data supports the argument. For example, the large Kano Province in Northern Nigeria, a well-institutionalized precolonial emirate with 2.3 million inhabitants, was ruled by only 14 British administrators in 1927. In comparison, Ogoja's .6 million inhabitants, living in the acephalous Southeastern part of the colony, were governed by 21 administrators. A simple linear model yields a very similar association (Appendix D.2.3, Table D.11). An increase in the political centralization of a province/district is associated with a decrease of about 3 British administrators per million inhabitants ($p < .05$). While adding only the vector of ethnic co-variates does not change this result, adding the vector of eight 'nature' controls decreases the size of the coefficient and turns it insignificant. Although this is reason for concern, this might well be due to the very small sample size. None of the additional covariates is associated with a statistically significant effect or increases the fit of the model.

Evidence on the devolution of power

After highlighting the negative relationship between precolonial centralization and local administrative effort by British but not French colonial governments, the next set of analyses focuses on the second facet of indirect rule: the power of indigenous authorities. Turning back to Hypothesis 4.2, I expect that precolonial institutions

30 The regression discontinuity design identifies only this difference but not the baseline effect of centralization on districts' size in British colonies.

31 In particular, I test the robustness of the result under varying distance-to-border cutoffs, with additional control variables to counter the remaining imbalances in the data, and using various grid-cell sizes. The latter two variations do not significantly affect the results. Across all possible bandwidths, the baseline results become statistically insignificant below a cutoff of 1.5 decimal degrees to the border and are stable at larger cutoffs.

increase the power devolved to local authorities. To probe this argument, I analyze the size of budgets of native treasuries in four British colonies as well as data on local administrative finances in French West Africa from (Huillery, 2010) and the status of chiefs in Nigeria.

NATIVE TREASURIES' BUDGETS I estimate the effect of precolonial centralization on the amount of revenues and expenditures of native treasuries under British rule in a cross-sectional manner. Because the previous analysis suggests that large districts are a consequence of precolonial centralization, the following considers the absolute and per-capita size of native treasuries separately. To that intent, the first set of regressions includes the full set of controls and takes absolute budget values as outcomes. The second set uses per capita budget values as outcomes and additionally controls for districts' logged population and size.

In line with the expectation of greater powers devolved to precolonial states, they are associated with much larger native treasuries, in absolute and per-capita terms. An increase in the precolonial level of political hierarchy by one level – moving from an acephalous society to petty chiefdoms, or from a large chiefdom to a state – is associated with an increase of total budgets by between 65 and 73 percent and of per-capita revenues and expenditures by around 28 percent (Table 4.5).[32] The respective coefficients are precisely estimated and consistent across the revenue and expenditure sides of local budgets. In sum, the results show that native authorities in British colonies presided over larger, more powerful, and more effective local governments where they could build on pre-existing, centralized institutions.

I conduct a series of robustness checks, fully reported in Appendix D.3.1. Following the previous analysis, I test whether the disease environment, the unequal weight of colonies, or outliers bias the results. Furthermore, I report alternative specifications of the model that do more justice to the panel character of the original budget-data, moving from the cross-sectional analysis to a district-weighted panel and hierarchical models. The estimated association between revenues per capita and precolonial centralization remains stable in size and statistical significance. I also re-estimate the main model using the alternative measures of precolonial centralization from Michalopoulos and Papaioannou (2013*b*) and a dummy for districts that feature a capital of a precolonial polity in 1885. Districts with a capital in 1885 exhibit

32 Again, because of the logged outcomes, I calculate these percentage changes as $(exp(\beta) - 1) * 100$.

Table 4.5: Native treasuries under British rule: per-capita revenues and expenditures (logged 2016 £)

	Revenues		Expenditures	
	total	per capita	total	per capita
	(1)	(2)	(3)	(4)
Precol. centralization	0.503***	0.242***	0.546***	0.257***
	(0.125)	(0.078)	(0.127)	(0.088)
Pop. density 1880 (log)	0.246	−1.958***	0.202	−2.009***
	(0.178)	(0.469)	(0.242)	(0.665)
Ethnic pop. density 1880 (log)	0.357	0.494**	0.493	0.594**
	(0.305)	(0.205)	(0.379)	(0.257)
Distance to coast (log)	0.053	−0.139*	0.113	−0.071
	(0.105)	(0.077)	(0.107)	(0.078)
Distance to river (log)	0.057	−0.050	0.081	0.035
	(0.122)	(0.088)	(0.139)	(0.097)
Population (log)		1.600***		1.641***
		(0.402)		(0.597)
Area (log)		−1.674***		−1.672***
		(0.423)		(0.596)
Colony FE:	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes
Mean DV:	13	1.1	14	1.2
Observations	146	146	126	126
Adjusted R^2	0.550	0.624	0.452	0.569

Notes: OLS models. Standard errors are clustered on the province-level. The sample includes the colonies of the Gold Coast (Ghana), Nigeria, Nyasaland (Malawi), and Uganda. Nature controls consist of median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls include the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

larger budgets, but not on a per-capita basis. Although speculatively, this might be indicative of differential effectiveness of indirect rule in rural and urban(izing) areas that developed around the old centers of society. Lastly, I disaggregate the analysis into the main revenue and expenditure lines of the budgets. The above findings are prevalent across almost all budget items.

To explore whether similar or opposite dynamics marked district finances in French colonies, I make use of Huillery's (2010) data on tax collection, public investments, and service provision in 109 French West African cercles. The results in Appendix

D.3.2 suggest that precolonial centralization had no significant effect on tax collections in French West Africa, but a negative effect on investments and spending on teachers and doctors. In per-capita terms, only the number of doctors is significantly lower in centralized districts than elsewhere; all other indicators yield statistically insignificant results. Although in itself not fully conclusive, these insignificant but negative associations provide a foil to compare the results from the British colonies against. This comparison increases the confidence that the positive association between precolonial institutions and native treasuries' resources in British colonies are indeed due to indirect rule of precolonially centralized polities.

THE POWER OF CHIEFS The analysis of British native treasuries consistently shows that native administrations had more financial resources at their disposal if located in areas with high degrees of precolonial centralization. However, chiefs in centralized and decentralized areas might have enjoyed the same status, with the former being able to more effectively use their powers. An analysis of the highest class of chiefs in Nigerian districts shows that this doubt is unfounded (Table D.18). Precolonial centralization correlates strongly with the class of chiefs: Districts in the territory of precolonial kingdoms featured many more "First Class" chiefs than acephalous areas. They were most often headed by "Third Class" chiefs. Adding the additional controls, this association becomes somewhat smaller but remains statistically significant.

This pattern of greater powers remaining in the hands of chiefs that could build on precolonial institutions in Nigeria completes the picture of the second analytical part of this chapter. Drawing on data on districts' sizes, British administrators, native treasuries, and the power of local chiefs, the analyses provide consistent evidence that the British devolved more power to indigenous elites where centralized precolonial institutions offered readily usable structures for indirect rule. This dynamic is not apparent in data from the French colonies, in which, as we have seen, precolonial polities faced frequent demise and downfall.

4.5 CONCLUSION

Following up on the decades-old debate on indirect rule, this chapter has brought forward systematic empirical evidence on the application of indirect rule in British and French colonies in Africa. Drawing on the historical literature on the topic, I argue that the French have ruled more directly and against, rather than through, precolonial institutions. In comparison, British colonial rule built largely on the idea of local self-government. This is in line with the incentives set by the empires' administrative infrastructure at home and their resource constraints. Republican in spirit and governing a comparatively small colonial empire, direct rule was more attractive for the French than for the British. The latter could however only rule indirectly over areas that featured established and centralized precolonial institutions. Elsewhere, in particular in acephalous societies, the British had to establish more direct rule to bridge the gap between the colonial capital and the population.

The systematic evidence presented here supports these arguments. First, French rule was hostile towards precolonial polities to a degree that about 70% of all colonized polities experienced their demise before independence. In British colonies, this figure amounts to about 30%. Second, where strong precolonial institutions prevailed and the British ruled, districts were around 45 percent larger than in areas settled by acephalous societies. They were by the same proportion smaller where the French ruled. Within British colonies, equally substantive differences are apparent in the size of native administrations' budgets, both in absolute and per capita terms. In combination, these patterns strongly support the argument that indirect rule was best realizable and most effective where pre-existing institutions could be co-opted by the colonial state.

These findings have important implications for theories about the historical pathways on which African states were built in the wake of the colonial Scramble. Contrary to a deterministic view that "there is often nothing new out of Africa" (Herbst, 2000, p. 30), the evidence shows that local governance arrangements were transformed by colonialism, but not in a uniform manner. The diversity of colonial institutions[33] interacted with the local precolonial past in shaping colonial and postcolonial socio-political development. Arguments that neglect this heterogeneity by positing direct links between the precolonial past and the present might thus, in some cases, overly compress history (Austin, 2008).

The above presented evidence has implications for important features of the post-colonial state in Africa. Variation in indirect rule between the colonial empires affects

33 These go of course well beyond the dimension of indirect rule, as the impacts of cash crop agriculture (Hopkins, 1973; Roessler et al., 2018) or Christianity (Lankina and Getachew, 2012) suggest.

Table 4.6: Constitutional recognition of traditional institutions in former French and British colonies

	Indigenous group rights	Traditional polititical institutions	Customary law	Concordant institutions
	(1)	(2)	(3)	(4)
Former British colony	0.647***	0.706***	0.882***	0.646***
	(0.114)	(0.115)	(0.078)	(0.134)
Former French colony	0.263**	0.316***	0.105	0.241*
	(0.108)	(0.109)	(0.074)	(0.127)
British - French:	0.384**	0.39**	0.777***	0.405**
	(0.157)	(0.158)	(0.108)	(0.184)
Mean DV:	0.44	0.5	0.47	0.38
Observations	36	36	36	23
Adjusted R^2	0.099	0.102	0.581	0.068

Notes: Difference in means of constitutional recognition of traditional institutions in former French and British colonies in Africa. All outcomes are binary. Outcomes in Models 1–3 are binary and come from Holzinger et al. (2018), data for the outcome in Model 4 come from (Mustasilta, 2019) and are averaged over the years 1989–2012. 'Indigenous group rights' refer to the constitutional acknowledgment and granting of socio-economic or political rights to at least one indigenous group. 'Traditional political institutions' refer to the acknowledgement of traditional or indigenous leaders or governance bodies. 'Customary law' refers to the acknowledgement and further regulation of customary judicial institutions. 'Concordant institutions' refer to the explicit recognition of traditional authorities in a constitution. See the original articles for further details. Robust standard errors in parenthesis. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

states' treatment of traditional authorities until today. Summarized in Table 4.6, former British colonies are much more likely to recognize traditional institutions and grant them with constitutional rights across all indicators compiled by Holzinger et al. (2018) and Mustasilta (2019). Differences in the indirectness of colonial rule within colonies brought about inequalities between local, mostly ethnically defined, governance units and their elites, many of which continued their careers after independence. The array of questions raised by these patterns is long. What effects does colonial indirect rule have, for example, on postcolonial local governance (Acemoglu, Reed and Robinson, 2014; Lange, 2009), ethnic politics and conflict (Cederman, Gleditsch and Buhaug, 2013; Wig, 2016), traditional institutions (Acemoglu et al., 2014; Baldwin, 2016), and land rights (Boone, 2003; Firmin-Sellers, 2000; Honig, 2017)? Since indirect rule belongs to the repertoires of states still today and chiefs oftentimes maintain important roles as brokers (Baldwin, 2014; Kadt and Larreguy, 2018) and auxiliaries

(Baldwin, 2013; Henn, 2018) of politicians and the state, better evidence on its short- and long-term effects on local and national politics is of considerable value.

The evidence on variation in the local strategies of colonial state building also raises important questions on its effects during the colonial period. What was the impact of direct and indirect rule on the lives and livelihoods of the millions colonized and ruled by either district officers, traditional rulers, or warrant chiefs? How did the style of local rule affect the level of extraction committed by the European empires? Using the present characterization of indirect colonial rule as a starting point, Chapter 6 delves into these questions and analyses the effect of indirect rule on public services provided in return for colonial resource extraction.

5

ROADS TO EXTRACTION: NATURAL RESOURCES AND THE PHYSICAL FOUNDATIONS OF STATE CAPACITY

In the beginning, space is the master;
its distances run wild, overwhelming man.
Gradually space is conquered, distances are tamed,
brought to heel or, rather, increasingly to wheel."
Weber (1977, p. 195)

The wheels of railroads and wagons that gradually conquered France in Weber's apt description of 19$^{\text{th}}$ century state building made their way into African colonies at the beginning of the 20$^{\text{th}}$ century. But the wheeled transport revolution did not arrive everywhere. Instead, African states built and still today feature sparse and uneven transport networks (Herbst, 2000). They therefore face high costs of transacting with large parts of their societies. Because taming space is so important for broadcasting power, this lack of transport infrastructure curbs economic development (Jedwab and Moradi, 2016, Chapter 7) and facilitates violent mobilization (Buhaug and Rød, 2006; Fearon and Laitin, 2003, Chapter 8).

The theoretical argument of this dissertation singles out extractive state building as an important driver of the uneven development of transport networks and state reach. This chapter provides evidence that colonial infrastructure investments were heavily biased towards areas with the potential for cash crop production and mining. These investments led to more local roads, but also made resource-rich areas better connected to ports and states' administrative headquarters. For the most part, the effects persist and affect transport networks and local state reach until today.

Addressing the historical roots of the geographical reach of states in contemporary Africa, this chapter extends the literature on the historical origins of (a lack of) state capacity on the continent. Herbst (2000) has prominently argued that patterns of state rule did not change much over the centuries because African states were never forced to extend authority over their large and sparsely inhabited peripheries. Underlining such deep historical roots of statehood in Africa, modern state capacity and economic development has been traced back to precolonial polities (Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013*b*; Hariri, 2012). While exposing important path-dependencies, the literature on the deep roots of state capacity loses sight of the transformations brought about by colonialism. Although focusing specifically on road networks, an infrastructure built by colonial powers from scratch, Herbst (2000) downplays the effects of colonialism and neglects the strategic incentives of colonial states to engage in regionally unequal state building. In contrast, this chapter shows that colonial interests in extracting primary resources shaped the geographical extent of states' reach.

The chapter also contributes to the burgeoning literature on the effects of transport infrastructure on economic development (e.g. Donaldson, 2018; Donaldson and Hornbeck, 2016; Faber, 2014; Jedwab and Moradi, 2016; Tang, 2014). Rather than focusing on roads' and rails' impact on interactions between states and citizens, this literature emphasizes their importance for market access, which in turn spurs economic growth and urbanization. However, the literature treats the origins of transport investments mostly as an econometric problem to be solved, rather than a costly political decision to be studied. The two important exceptions to this state of the literature consist in recent working papers by Jedwab and Storeygard (2017) who study African transport networks at the national level and Roessler et al. (2018) who provide estimates on the association between resource production and major roads and railroads on the continent.[1] Given the persistent consequences and the high costs of building public infrastructure, this leaves an important research perspective unexplored.

In order to test the effect of local resource potential on transport infrastructure investments, I introduce three new data sources, two of which will be also used in Chapters 7 and 8 below. The first data set consists of colonial road networks in eight British colonies between 1900 and 1950 reconstructed from road lists enumerated in British colonial reports. To trace the development of road networks after independence, Philipp Hunziker and I use a fully convolutional neural network to extract road-related information from the Michelin map corpus that covers the entirety of the continent.[2]

1 However, given that the causal relation between transport infrastructure and resource extraction runs both ways, Roessler et al. (2018) are unable to estimate the causal effect of production on transport networks.

2 This digitization project has been conducted in preparation of writing the working paper adapted for Chapter 8.

Both sources of spatial data on transport networks together with railroad data from the colonial period (Jedwab and Moradi, 2016) allow me to build planar networks akin to Google maps for a total of 34 years between 1900 and 2017. With these data, I compute local road densities, and local areas' connectedness to ports, as well as administrative capitals at the national, regional, and district levels. Here is where the third new data source, a collection of all regional borders and capitals from independence to today, enters the analysis.

I use the resulting data on local road infrastructure and transport costs to test the effect of local resource potential on transport infrastructure across the 20$^{\text{th}}$ century. To measure resource potential, I focus on an exogenous measure of the suitability of soils for cash crop agriculture and the presence of mineral deposits. Both indicators have consistent effects on the local density of roads and areas' connectedness to ports and capitals. Showing low levels temporal variation, the effects are stable across a range of robustness checks, including province fixed effects, using different measures of cash crop suitability, as well as varying the size of units of analysis. Testing competing arguments about how indirect rule might have either increased or decreased local transport infrastructure investments, I find no evidence for a positive or negative net effect of colonial indirect rule.

5.1 TRANSPORT COSTS AS CONSTRAINTS TO EXTRACTIVE COLONIALISM

The projection of authority over vast, sparsely inhabited, and ecologically hostile territories proved to be a major challenge for colonial and postcolonial state builders (Herbst, 2000). Transport infrastructure, virtually absent upon colonial conquest (Chaves, Engerman and Robinson, 2013),[3] was key for bridging large distances. However, the colonial state builders' incentives did not call for evenly reaching out to every corner of their colonies. Rather, the colonialists' aim to extract and tax resources provided the grounds for an uneven expansion of transport networks.

With the 'Scramble for Africa' triggered by the Berlin Conference in 1885 European colonizers rushed to the inner parts of the continent to secure their piece of the pie. However, due to the lack of administrative and military personnel, colonial control of many regions existed mostly on paper only. With abundant territory, this pattern

3 As already mentioned, only very few regions such as the Buganda and Asante kingdoms featured roads, but these were not used for wheeled transport (Reid, 2002; Wilks, 1975).

was partly just a continuation of the precolonial absence of state control in peripheral areas (Herbst, 2000). In difference to European states, colonial powers did not have to fear for the security of the conquered territory since the Berlin Conference established the concept of "effective rule" that fostered the peaceful establishment of colonial borders. As highlighted in Chapter 4, they could therefore rule through the "thin white line" (Kirk-Greene, 1980) and project their authority through relatively weak ties that consisted of few administrators and native or invented local elites.

But some inland territories nevertheless attracted the interest of the colonizers. These where in particular the resource-rich spots that colonial governments aimed to control and exploit in order to meet metropolitan demands and to finance the colonial state. In trying to do so, transportation proved to be the main obstacle to the colonial state and European trading companies. The costs of transport in the early colonial days were daunting. Although known to many precolonial societies, the wheel was, for partly unknown reasons,[4] not in use (Law, 1980). Moreover, the Tsetse fly inhibited the use of pack animals in many parts of the continent (Alsan, 2015). Transport was thus powered by humans, bringing about a further increase in demand for labor where labor was already scarce (Chaves, Engerman and Robinson, 2013). More importantly, transporting a ton per mile with porters cost 40 Pence in the 1920s, comparing to a sharply decreasing cost of 20 (5) Pence where trucks (trains) could be used.[5] In short, without modern transport infrastructure, expensive transport translated into high transaction costs for colonial governments, prohibiting effective governance and exploitation of non-coastal areas.

Lowering high transaction costs through investments into transport infrastructure therefore quickly became one of the main priorities of colonial governments when fuel-powered transport systems made their way into the African colonies at the turn of the 19th century (Lugard, 1965, pp. 461-476). To overcome the reliance on human porterage and to put the arriving trucks, cars, and locomotives to work, roads and railroads were urgently needed. Being costly investments[6] undertaken by resource-constrained colonial governments, the expected benefits of connecting local areas to the emerging networks determined where transport investments would be prioritized.

4 The absence of wheeled vehicles can be (partly) attributed to the effects of the Tsetse fly and the lack of good quality roads (Austen, 1983; Hopkins, 1973; Law, 1980).

5 See Figure B.3 in Appendix B.1 and the discussion below.

6 Building roads and railroads was indeed a costly affair in the early 20th century Africa. For example, the 1911 annual report of the Roads Department of Ashanti and the Northern Territories in the Gold Coast puts the cost of building one mile of roads between 515 and 1271£.

Infrastructure investment promised the highest returns for colonial government where it tapped into natural resources. Investments towards resource-rich areas were a precondition to respond to the metropolitan demand for primary resources (Migdal, 1988), which was one of the most important goals of imperialist policies (Roessler et al., 2018). The export of resources enabled by transport infrastructure also provided for the financial backbone of colonial states, which were required to be financially self-sufficient (Frankema and van Waijenburg, 2014; Gardner, 2012; Helleiner, 1966; Young, 1994). This was because taxing exports at the ports was more effective than taxation of internal trade or subsistence activities (Kasara, 2007). Resource-rich areas thus stood out against the 'empty panorama offered by the rest of the colony' (Fanon, 2004, p. 106), areas without the potential for extraction untouched by the state and its tentacles (Boone, 2003; Cooper, 2008; Herbst, 2000; Roessler et al., 2018).

To facilitate resource extraction through transport infrastructure, colonial state makers had to build transport networks that fulfilled three main criteria. At the local level, resource-rich areas required relatively dense transport infrastructure, thus providing producers with direct access to roads or railroads. As a function of the geographic distribution of resources, these investments had to be highly localized where aimed at deposits of minerals extracted by few mines. In contrast, in regions suitable for the production of cash crops, networks were optimally more spread out, since crops were planted in a decentralized manner by a multitude of farmers, oftentimes small holders (e.g. Boone, 2003; Hogendorn, 1978).

H 5.1: *Denser transport infrastructure was built in resource-rich areas.*

Investing in local infrastructure alone would, however, not have triggered the cash crop and mining revolutions they intended to foster. Beyond the local level, resource-rich areas needed efficient access to the main ports where tax collectors stood ready to tax the exported goods. Looking onto inaccessible hinterlands that started few kilometers off the coast, British colonial officers at the turn of the 19^{th} century were well aware of the need for such connections to tap into natural resources. For example, the governor of the Gold Coast, today's Ghana, reported in his Annual Report of 1892 that "splendid" trade in cotton "might easily spring up" with better transport to the coast (p. 31). He also highlighted that the lack of transport infrastructure meant that the gold mining industry suffered from "great hindrances in the development of what ought to be a prosperous and handsomely paying industry" (p.33) and stressed the need to construct railways to mitigate these constraints.[7] These accounts further motivate the second hypothesis:

H 5.2: *Resource-rich areas were better connected to ports.*

The port-focused logic of building transport infrastructure would have resulted in tree-like networks with few internal connections. Such networks, however, would have made colonial governments unable to foster resource production trough the means of administration and policy. Beyond trying to impose law and order at the local level, colonial governments promoted the production in particular of cash crops. With debatable success (e.g. Green and Hymer, 1966), they did so by establishing agricultural resource institutes, promoting the use of best growing practices, coordinating production and marketing processes, distributing free seeds, and draconian measures for pest control (e.g. Danquah, 2003).[8] Because these activities were carried out by officers located in administrative headquarters, I expect that investments into transport infrastructure also aimed at connecting prospective areas of production with national, regional, and local capitals. Moving beyond the focus on transport infrastructure, locating administrative headquarters close to resource-rich areas had an equivalent effect.

H 5.3: *Resource-rich areas were better connected to administrative capitals.*

Because colonial investments into transport infrastructure physically hard-coded their bias for resource extraction into the transport networks, I expect these effects to persist well into the postcolonial area until today. This is consistent with recent findings of enduring and path-dependent effects of colonial infrastructure (Jedwab and Moradi, 2016; Jedwab, Kerby and Moradi, 2017) and of colonial cash crop and

7 Other governors fielded similar reports. In Nigeria, the question of transport was considered "one of the most serious" problems to solve in 1902, with the chief concern being the development of cotton farming in Northern Nigeria, a goal that marked calls for extending the railway in Nyasaland (Malawi) in 1904. This focus on cotton sometimes amounted to wishful thinking: When the railway reached Kano in Northern Nigeria in 1911, no cotton, but a groundnut revolution occurred (Hogendorn, 1978). The annual reports cited here come from the Library of the University of Illinois at Urbana-Champaign (`https://www.library.illinois.edu/`).

8 The Annual Reports of the Agricultural Departments in the British colonies bear witness to a wide array of such activities. For example, the Annual Agricultural Report from Uganda in 1929 enlists, inter alia, three experimental stations, five stations for subsidiary agricultural officers, 49 nurseries for coffee seedlings, as well as staff specialized in Entomology, Mycology, Chemistry, and agricultural education. Transport infrastructure was necessary for the staff to tour their districts and the knowledge and products of the various institutions to spread to the farmers whose agricultural production they intended to foster.

mineral production (Roessler et al., 2018) on urbanization and local development in Africa.

H 5.4: *Extractive biases in transport networks persist over time.*

I empirically test these hypotheses in the following quantitative analysis of colonial and postcolonial transport networks. Introducing new data on road networks between 1900 and today, the results support the hypotheses. In particular, they show that resource-rich areas have profited from more local investments into transport infrastructure and were better connected to ports and administrative capitals of the colonial states. For the most part, these effects persist until the present day.

5.2 DATA ON AFRICAN TRANSPORT NETWORKS

The argument that transport was an important means to promote primary resource production and served the extractive purposes of colonial rule guides the following analysis that draws on two new data sets on road networks in Africa. The first is a reconstruction of road maps from lists of roads published in reports from eight British colonies between 1900–1950. Because railroads played an important role during this period (but not thereafter), I augment these data with time-varying railroad data from Jedwab and Moradi (2016). The second data set consists of 23 digitized African road maps produced by the Michelin tire company. To measure local areas' connectedness to ports and capitals, I transform both sets of road data into planar transport networks. I then compute transport costs (for the early maps) and times (for the Michelin data) to ports and capitals. Table 5.1 provides an overview over the various ingredients to the data sets, described by the following paragraphs. For all technical details, I refer to Appendix B.

5.2.1 *Transport networks in British colonial Africa 1900–1950*

To empirically assess the causes of colonial investment into transport infrastructure, we need geographically encoded and preferably time-varying data on colonial roads and railroads. While railroad data is available from research by Jedwab and Moradi (2016), geographical data on colonial roads does not readily exist. Owing to the lack

Table 5.1: Construction of road-networks and derivatives.

Ingredient	Colonial period (8 British colonies)	Postcolonial period (African continent)
Network resolution	5km	5km
Road data	Blue Books†	Michelin Corpus†
Railroad data	Jedwab and Moradi (2016)†	
Edge weights	Travel costs*	Travel times*
Ports	Blue Books*	Natural Earth Ports v2.2*
National capitals	Cshapes* (Weidmann and Gleditsch, 2010)	Cshapes† (Weidmann and Gleditsch, 2010)
Regional capitals	own collection*	own collection†
District capitals	own collection*	

Note: † time-variant data; * cross-sectional data.

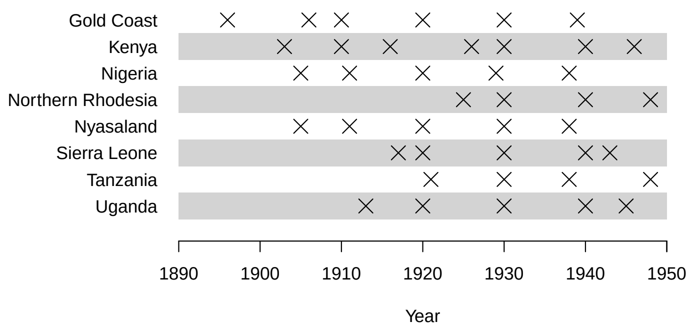


Figure 5.1: Digitized and geographically mapped road lists from the British Colonial Blue Books.

of comprehensive and regularly published road maps prior to WWII,[9] I digitize such data from lists of roads published in the yearly British Colonial Blue Books. These lists allow me to infer the location and route of colonial roads. While the relatively few stretches of navigable rivers were used for transportation (Hailey, 1945, pp. 1539-1546; Hopkins, 1973, pp. 72-73, 197), they did not require large infrastructure investments.[10] I therefore neglect navigable rivers in the construction of transport networks, but control for their presence in the empirical analyses.

9 The most comprehensive series of maps on the African continent was combined into the US Army Map Series 1302. However, this collection does not provide data on transport networks before the 1940s.

10 This is not to say that one could not have improved the navigability of rivers, but such large-scale investments were not undertaken by the Europeans who limited their efforts at acquiring (few) steam vessels (Hopkins, 1973, p. 197).

(a) 1915 (b) 1930 (c) 1950

Figure 5.2: Development of the Ugandan transport network 1920–1940.
Roads retrieved from the colonial Blue Books are mapped in black and railroads from Jedwab and Moradi (2016) in red.

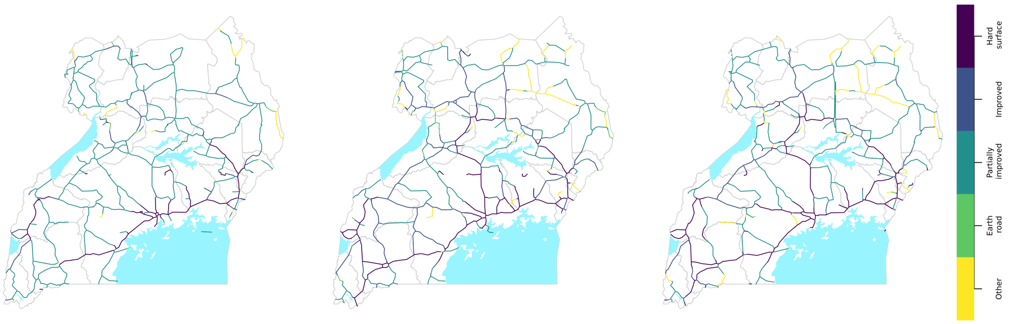
The British Colonial Blue Books are statistical yearbooks prepared by colonies' administrations for the government in London (Banton, 2008). Among a host of different topics,[11] they oftentimes contain information on roads under the administration of the colonial government. In particular, this information features the start and end point of each road. To use this information for reconstructing colonial road maps for 41 colony-years in eight colonies (Figure 5.1),[12] I first digitize the road lists using optical character recognition software coupled with correction by hand (see Appendix A.2.1). I then geocode the start and end points of each road and search for the shortest path between them on today's road network (Appendix B.1.1).[13] This shortest path is coded as being a road in the respective year of origin of a road list and all following years.[14] This results in time-variant road maps of the sort mapped in Figure 5.2 for the case of Uganda. A cross-validation of the final data with Michelin maps from 1966 suggests a high correlation between the two data sources (Appendix B.1.3).

11 Revealingly, the Blue Books devote most of their attention to colonial trade statistics, listing exports and imports at times at the level of the individual vessel. Other topics covered by the reports include colony-level revenues and expenditures (see e.g. Xu, 2018), education statistics (see e.g. Ricart-Huguet, 2016), lists of missionary stations, and judicial information.

12 In principle, the colonial Blue Books provide lists at slightly irregular, yearly intervals so that the temporal density of the data could be increased. However and as the analysis will show, adding temporal detail is unlikely to improve the insights gained from the analysis.

13 Current roads come from the gROADS project (CIESIN, 2013). The main assumption here is that roads almost never disappear so that we can infer the location of past roads from current road networks.

14 This coding decision is based on the assumption that roads once built almost never disappear. They can, of course, degrade, but such information is not consistently available in the Blue Books.



(a) 1966 (b) 1990 (c) 2017

Figure 5.3: Development of the Ugandan road network 1966–2017.
Roads retrieved from the Michelin corpus, colored according to their type.

While providing good coverage over time and across the eight colonies, the resulting data has three main shortcomings. First, the data is only available for the eight British colonies listed in Figure 5.1. Second, the Colonial Blue Books mostly enlist roads under the administration of the central colonial government, disregarding roads managed by native authorities or other actors. Regionally differing policies towards the road-related duties of local governments might thus explain part of the observed variation.[15] Third, the Blue Books do not offer systematic assessments of roads' quality.[16] Hence, all roads have to be treated equal in the data, masking large differences between surfaced and muddy roads.

5.2.2 *Roads in postcolonial Africa 1966-2015*

In order to address the shortcomings in the road data from the Blue Books, I draw on newly digitized data from the Michelin corpus which covers the entire continent between the 1960s and today in a consistent manner.[17] These maps, digitized together

15 Indeed, the case of Northwestern Ghana illustrates this caveat. The Blue Books do not mention roads in the region, although, by 1940, roads did span the territory. Note, however, that the Gold Coast is the only country where I have noticed such regionally clustered discrepancy. Dropping the Gold coast from the analysis or adding province fixed effects that would account for regional differences in the authority over transport infrastructure does not substantially change the results. See Appendix E.3.
16 Information like 'all-weather' or 'tarred' road is sometimes present but absent in most lists.
17 Although there is rich data on contemporary road networks, for example provided by the gROADS (CIESIN, 2013) and `OpenStreetMap.org` projects, none of the openly available data sets covers the continent in a time-variant manner. Also, the data sets feature large quality difference between countries or subnational regions. Other researchers have collected time varying information on roads in Africa, in particular Jedwab and Storeygard (2018), but their collection of data derived from Michelin maps is not (yet) openly available.

with Philipp Hunziker, cover the African continent at a resolution of 1:4,000,000 in intervals of approximately 5 years since 1966. While we are not the first to make use of the Michelin corpus (see Burgess et al., 2015; Jedwab and Storeygard, 2018) we break new ground in how to digitize historical map material. In particular, we develop an automatic map digitization procedure that moves beyond the current practice of coding information by hand, which is both expensive and non-replicable. To do so, we refine and implement a deep-learning model originally designed for object detection in photographs (Shelhamer, Long and Darrell, 2017) to separate road-related information from other features in scanned map images. In combination with a set of post-processing algorithms, this approach allows us to extract Michelin's road-network information with high accuracy at low cost. Box 1 and Appendix B.2 describe the digitization procedure in more detail.

5.2.3 *Constructing planar transport networks*

To be able to measure transport costs and times I construct planar spatial transport networks similar to Google Maps (see Appendix B.3 for details). Because all applications presented below require transport networks that cover geographical space in a continuous manner, I build these networks on the basis of a grid of the African landmass[18] with a resolution of .04167 decimal degrees (≈ 5km).[19] The centroids of all grid cells are connected with 'footpaths' to their eight neighbors to allow seamless travel between any two points on the continent. I superimpose this base network on the road and railroad maps, creating additional vertices (i.e. crossroads) wherever a (rail)road crosses a footpath. In a last step and to calculate transport costs and times on the resulting networks, I weight their edges according to the cost or speed of travel on them. Two data sets provide such information, one for the set of colonial and one for the postcolonial networks.

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18 Note that I drop Madagascar and all other islands from the sample to be able to work with a fully interconnected graph. Furthermore, Madagascar is not covered by the first set of Michelin maps.

19 This corresponds to about 1mm on the original Michelin map scans. Working at an even higher resolution would require more detail in the original maps.

Box 1: Digitizing the Michelin map corpus

To digitize the Michelin map corpus, Philipp Hunziker and I apply a combination of techniques from machine learning and image processing to the georeferenced map sheets. The procedure, explained in full detail in Appendix B.2, follows four principal steps:

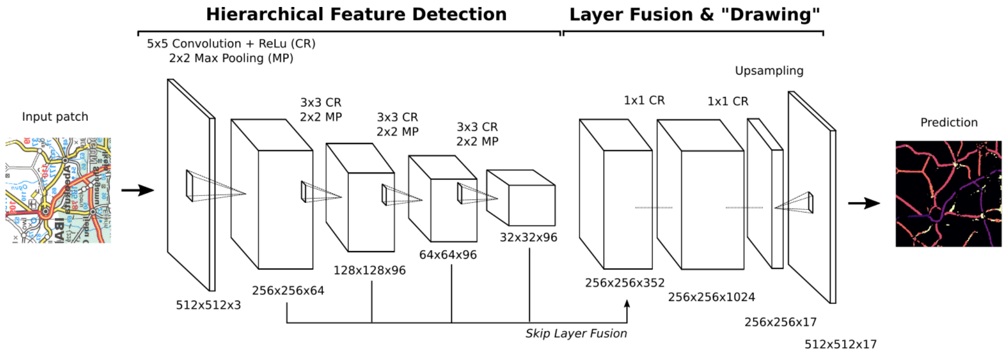


Figure B1: Architecture of our custom fully convolutional neural network

1. Training of the Fully Convolutional Neural Network presented in Figure B1 on 2000 artificially created and automatically labeled 'road maps' that feature lines. This reduces the amount of hand-labeled data necessary in Step 2.

2. Train the pre-trained model on a set of hand-labeled training data that cover all road types and backgrounds in the corpus. This results in a model that distinguishes between background and road pixels and between road types.

3. To transform the pixel-level predictions into lines, we apply first a topological thinning and then a line-tracing algorithm to the predictions.

4. Because small gaps remain where text overlaps with roads, a line-splicing algorithm fills up small, unlikely gaps. We then use a hidden-Markov model trained on the training data to smooth road type information, thus avoiding small stretches of miss-classified roads.

The procedure proves to be accurate. The error rates for the detection of any roads are below 2 percent for type-1 and type-2 errors. Error rates for the detection of precise road-types are somewhat higher (type-1: 10.7%, type-2: 5.3%). Because roads of similar type look similar, most miss-classifications are minor, for example classifying a partially improved as an improved road, rather than labeling a highway as an earth road. Erroneous classifications likely only add random, rather than systematic noise to the data.

For the colonial period, I retrieve data on the costs of transport by human porterage, road, and rail in the mid-1920s from various primary and secondary sources.[20] These indicate that by 1925 transporting a ton per mile cost about 40 Pence by porterage, 20 Pence by road, and 5 Pence by rail. The costs of road transportation are a conservative estimate, since they declined during the 1920s and 1930s, equaling the cost of rail-transport by the 1940s.[21] Combined with the greater density of road networks, this may be one important reason for why railroads have fallen out of use in postcolonial Africa (Gwilliam, 2011).

For the postcolonial period, the data on transport networks consists only of footpaths and roads. Fortunately, the Michelin data codes the quality of each road, which are associated with average travel speeds sampled from Michelin's online road atlas.[22] Footpaths are associated with a speed of 6 km/h (Jedwab and Storeygard, 2018). Dividing the length of each edge by the speed of travel on it results in the estimated time needed to traverse it. Using postcolonial travel times rather than costs, I assume that travel times capture the underlying concept of transaction costs (see also Jedwab and Storeygard, 2018).

5.2.4 *Deriving travel costs and times tor ports and capitals*

With the transport networks at hand, I can now calculate the costs and times of transport from any point in the networks towards their closest port as well as the administrative capitals a point is governed from. To derive continuous raster data of these measures, the points used for this calculation come from the raster that forms the basis for constructing the transport networks (resolution: .04167 degrees; $\approx$ 5km).

Data on the location of ports come from two sources. First, for calculating the distance to ports in the eight British colonies, I use all colonial ports, big and small, listed in the colonial Blue Books.[23] This comprehensive list of ports reduces the risk

20 See Figure B.3 in Appendix B.1. Data on transport costs come from Chaves, Engerman and Robinson (2013), Hailey (1945), and Hopkins (1973). I augment these with data on labor wages from the Annual Reports from the British Gold Coast for the years 1899–1936. I follow Chaves, Engerman and Robinson (2013) and calculate head porterage costs assuming that one man carried 60 lbs. (ca. 27 kg) over 15 miles (ca. 22.5 km) per day.

21 I do not use time-varying measures of transport costs per mile to avoid my estimates to capture changes in these costs rather than investments into transport infrastructure.

22 www.viamichelin.com

23 Note that for the landlocked colonies (Uganda, Nyasaland, and Northern Rhodesia), I choose the points where the railroads exit the countries as ports. This is warranted by the fact that the colonial data do not allow me to construct continent-wide transport networks. The respective constant

of endogenous port locations by which ports might have grown bigger close to areas of resource extraction. For the data that covers all of continental Africa, no such comprehensive list of small and large ports is available, which is why I resort to the data on ports offered by the Natural Earth Project.[24]

To locate administrative capitals on the road networks, I first make use of the Cshapes data (Weidmann and Gleditsch, 2010) for georeferenced information on national-level capitals and the areas they govern.[25] Moving down the level of administrative hierarchy to the regional and district levels, I draw on the time-invariant data on colonial districts and regions introduced above in Chapter 4. For the postcolonial period, I collect geographical data on the boundaries and capitals of a total of 1763 first-level administrative units and their changes since African countries' independence. Appendix C.2 presents the coding procedure, which relies on qualitative accounts of unit changes[26] and georeferenced map material.[27]

5.3 EMPIRICAL STRATEGY

In order to test Hypotheses 5.1–5.4 that colonial state builders constructed transport networks for extractive purposes and that postcolonial states maintained them, I aggregate the data on African transport networks presented above to arbitrary spatial units. Rather than choosing regular, quadratic grid cells as units of analysis, I here construct Voronoi polygons that cover the area of national units in a continuous manner. The Voronoi cells are based on a k-means clustering of geographic space in the respective national units,[28] so that cells have an average size of 2'500 km^2.[29] The procedure, presented in detail in Appendix E.1, improves upon the more common

transport cost between the point of exit from the colony and the port is empirically soaked up by colony-fixed effects introduced below.

24 https://www.naturalearthdata.com/downloads/10m-cultural-vectors/ports/, Version 2.0.0.

25 Note that these data reflect the state of colonies at the end of the colonial period. I have adjusted the borders of Nigeria and Cameroon to reflect the British mandate over the British Cameroons that joined French Cameroon after independence.

26 These accounts come primarily from the statoids.com database.

27 There are no such comprehensive accounts of second-level administrative units for the continent. I have therefore refrained from collecting postcolonial data on districts and their capitals.

28 For the colonial period, I take country borders from the eight British colonies at the dawn of colonialism. To not further complicate the analysis for the postcolonial period, I take the borders of countries in 2000 from Cshapes (Weidmann and Gleditsch, 2010).

29 This is the most commonly used size of grid cells, as for example, used by the PRIO Grid (Tollefsen, Strand and Buhaug, 2012). In further robustness checks, I vary the size of units to 1'250, 5'000, 10'000, and 20'000 km^2

Table 5.2: Measuring extractive bias in road networks: Main outcomes (all logged).

Outcome	Colonial period (8 Brit. colonies)	Postcolonial period (African continent)
Road density	Roads (km)/100 km^2	Roads × quality (km × km/h)/100 km^2
Connectedness ...		
... to port	Cost to port (£/ton)	Time to port (hrs)
... to national capital	Cost to nat. cap. (£/ton)	Time to nat. cap. (hrs)
... to regional capital	Cost to reg. cap. (£/ton)	Time to reg. cap. (hrs)
... to district capital	Cost to dist. cap. (£/ton)	

grid-cell approach in two ways. First, the Voronoi polygons are completely nested within the national units they are sampled from, whereas grid cells have to be cut along borders and coasts which leads to large variation in their size.[30] Second, Voronoi cells are more compact, that is closer to the shape of a circle,[31] than quadratic grid cells.

Taking the Voronoi polygons as the main units of analysis, I then spatially aggregate the transport network data that measures the main outcomes of interest in this analysis. In particular, for each unit, I compute the total density of roads and railroads, and the average travel costs and times to the closest port, and to national, regional, as well as district capitals (see Table 5.2). I calculate all transport costs and times on the 5×5 km raster and aggregate the results up to the Voronoi cells by taking their mean. I do so for every year in which I observe the transport network data. However, because the theoretical argument is time-invariant – I expect a build-up of extractive transport networks over the colonial period and persistence thereafter – I analyze the data in cross-sectional slices, estimating the effect of resources one year at a time.

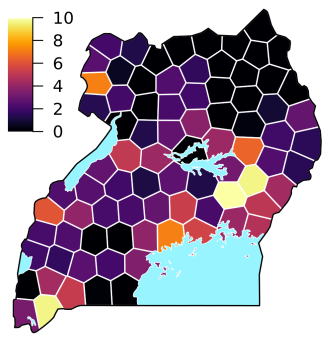


Figure 5.4: Voronoi polygons (2500 km^2) with road density (100km/km^2) in Uganda, 1950

The two main treatments of interest reflect the local availability of natural resources. To capture the potential for cash crop production, I construct a score of the suitability of soils for growing cash crops. In particular, I take the local maximum of soils' suitability for growing the eight major cash crops on the continent: cocoa, coffee,

30 This cutting is necessary to estimate models with country or colony fixed-effects as used below.
31 This statistic can be calculated through the Polsby-Popper score (Polsby and Popper, 1991).

cotton, groundnuts, oil palms, sugar cane, tea, and tobacco.[32] These data are retrieved from the FAO GAEZ database (FAO, 2015), with values ranging from 0 to 1, the highest possible suitability. Suitabilities are modeled as a non-linear combination of a range of climatic, geographic, and soil-related measures that are exogenous to the observed outcomes. To gauge the local potential for mining, I rely on data from Schulz and Briskey (2005), who provide information on the type and location of major mineral deposits around the world. I code a binary indicator to capture the presence of at least one mineral deposit in a Voronoi cell.

With this setup of units, outcomes, and treatments, I estimate the following cross-sectional model:

$$Y_{i,c} = \gamma_c + \beta_1 \textsf{Cash crop suit.}_i + \beta_2 \textsf{Mineral dep.}_i + \boldsymbol{\delta X}_i + \epsilon_{i,c}, \tag{5.1}$$

where i is the Voronoi polygon nested in a country/colony c. In addition to the two main treatment variables measured in a given year and the colony/country fixed effects γ_c, I add a vector of control variables X_i. In particular, I control for the local climate[33] as well as the local geography[34] that determine local agricultural suitability. In order to distinguish soils' suitability for cash crop production from that for general agriculture, I control for the agricultural suitability score from Ramankutty et al. (2002). Furthermore, I control for an area's logged distance to the coast, border, and closest navigable river[35] to avoid that non-resource driven patterns of colonial state expansion affect the results. Lastly, there is the risk that cash-crop suitable areas had a higher precolonial population density, which may naturally lead to more local transport investments. However, direct measures of disaggregated population patterns from that time are non-existent. The best source consists in estimates of local population counts in 1880 from Goldewijk, Beusen and Janssen (2010). However, these estimates are influenced by postcolonial population counts that are affected by transport infrastructure (e.g. Jedwab and Moradi, 2016) and thus post-treatment

32 These crops constituted 51 percent of total colonial exports and 85 percent of agricultural exports in Africa (Hance, Kotschar and Peterec, 1961). Taking the maximum suitability in a grid cell is motivated by the fact that most often only one crop was planted exclusively. Roessler et al. (2018) demonstrate that cash crop suitability is a good predictor of real cash crop agriculture at the end of the colonial period.

33 Mean annual temperature, precipitation, evaporation, and the ratio of precipitation and evaporation, from FAO (2015).

34 The mean altitude and slope of an area, from FAO (2015).

35 Data on navigable rivers comes from Jedwab and Moradi (2016).

outcomes. Conscious of that risk, I choose to conservatively control for the 1880 population estimate and provide a 'robustness' check where I drop the variable. In order to account for the spatial correlation of standard errors that origin form equally clustered resource endowments, I cluster errors on the level of first-level administrative regions.

One important threat to inference comes from potential bias in the data on mineral deposits. Although the data from Schulz and Briskey (2005) describes deposits rather than mines, transport networks may have encouraged search efforts and thus discoveries of mineral deposits observed in the early 2000s. However, in particular during the colonial period, the qualitative evidence cited above points towards a general sequence of events in which a deposit is known first, and transport networks are built to support local mining industries. However, I cannot rule out that reverse causality affects the estimated the impact of mineral deposits.

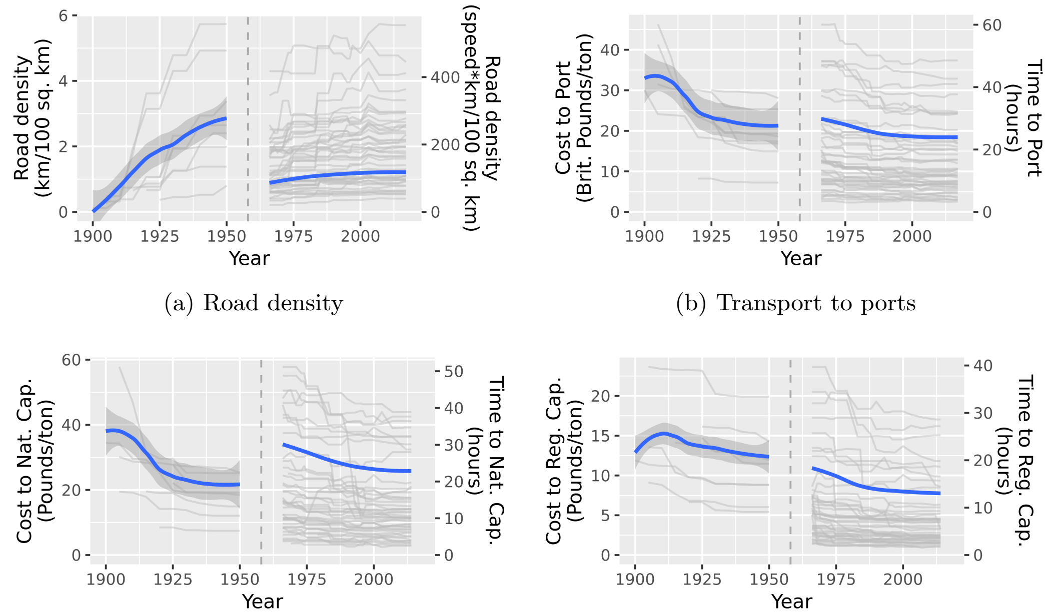
5.4 RESULTS

5.4.1 *Descriptive statistics on African transport networks*

As already highlighted above, modern transport infrastructure was next to absent in colonial Africa at the turn of the century. Its expansion mainly took place after World War I, slowing down after the Great Depression and World War II. But infrastructure was still very limited at the dawn of the colonial period. In the eight British colonies for which I observe colonial road networks, I calculate an average road density of only about 3 km per 100 km^2 in 1950 (Figure 5.5a). The growth of transport network naturally led to decreases in the costs of transport during the colonial era (Figures 5.5b to 5.5d). Averaged over all units of analysis, the cost of transporting a ton to ports decreased by more than a third, and so did the costs of carrying a ton to national capitals, which in some cases were ports themselves.[36] Transport to regional capitals saw a lesser relative decline. This is mainly due to the shorter distances that could be decreased.

Transport networks continued to improve after African countries' independence. However, they improved, as noted by Herbst (2000), at a relatively slow pace. The

36 For example, Lagos, Dar es Salaam, and Accra were important ports.



(a) Road density

(b) Transport to ports

(c) Transport to national capitals

(d) Transport to regional capitals

Figure 5.5: Development of the yearly means of transport-related outcomes over time. Note the break in the time-series between the colonial and postcolonial data, which translates into different scales on the left (colonial) and right (postcolonial) y-axes. Lines in shades of grey are per-country means, countries that are observed in the colonial sample are plotted in a darker shade. The bold, blue lines plot a LOESS-smoothing across all observations.

quality-weighted density of road networks increased by about 25 percent over the postcolonial period. In about the same order of magnitude, the time it took to reach ports and capitals decreased by about 20–25 percent. Beyond better roads, relocations of capitals and unit splits at the national and regional levels drive some of these developments. Chapter 7 delves into more detail on these processes and their developmental effects.

5.4.2 *Main results*

Though important, the descriptive statistics just presented mask great variation within colonies and countries. I hence turn towards the effect of local resource potentials on the density of local road networks (H5.1), the connectedness of local areas towards ports (H5.2) and capitals (H5.3), as well as the persistence of these effects over time (H5.4). In a first step, I estimate the baseline equation for every outcome and every year in which a road network is observed – starting in 1910[37] and ending in 2014.[38] In a second step, I conduct a series of robustness checks on the baseline analysis.

Figure 5.6 presents the results from the main analyses. Each row plots the estimated effect of a Voronoi cell's cash crop suitability and that of the presence of at least one mineral deposit on one of the 5 main outcomes across all years. Because all outcomes are log-transformed, we can directly compare the coefficients within each plot without having to adjust for level changes in the respective variables. Though, two caveats affect comparisons between the colonial and postcolonial estimates. First, colonial transport costs take into account road and railroad data, whereas postcolonial travel times are only based on road network data. Second, for a direct comparison across the break in the time-series we have to assume that travel times correspond to transport costs in a linear manner, which might not always be the case.[39]

As expected and shown in the first row and column of Figure 5.6, areas with a high suitability for growing cash crops feature significantly denser local road networks. In substantive terms, the estimated effects are large. Increasing local cash crop suitability by one standard deviation (0.21), increases the density of roads by 41 [11, 79] percent in 1950 and 35 [9.6, 67] percent in 1966.[40] These effects remain quite stable across the entire century. This suggests that, although networks grew, their relative bias towards cash crop suitable areas remained.

The effects on local road densities find their extension in the impact of local cash crop suitability on an areas' connectedness to ports and administrative capitals (rows 2 to 5 of the first column in Figure 5.6). With regard to transport to the closest port, an increase in local cash crop suitability by one standard deviation is associated with a decrease in the cost of transport by -11 [-18, -4.2] percent in 1950. The effect amounts to only -3.9 [-6.6, -1.1] percent once we move to the Michelin networks without railroads in 1966. This suggests that the colonial results are heavily influenced by railroads that oftentimes went straight from ports to resource-rich areas (Jedwab

37 This is when we observe more than a few roads in a few places. Before, the results are relatively meaningless and associated with large standard errors.

38 This is because the data on regional and national borders and capitals ends in 2016, making the Michelin map from 2017 useless.

39 A third caveat, the difference between the countries the analyses are based on, might in principle affect the results. This worry is however unwarranted: additional results presented in Appendix E.3 shows that the eight British colonies observed in the colonial sample behave similar to the sample average in the postcolonial sample.

40 These percentage changes are calculated as $(exp(\beta * .21) - 1) * 100$. Note that due to the constant added to road densities, the respective effects are larger for small baseline values of the outcome.

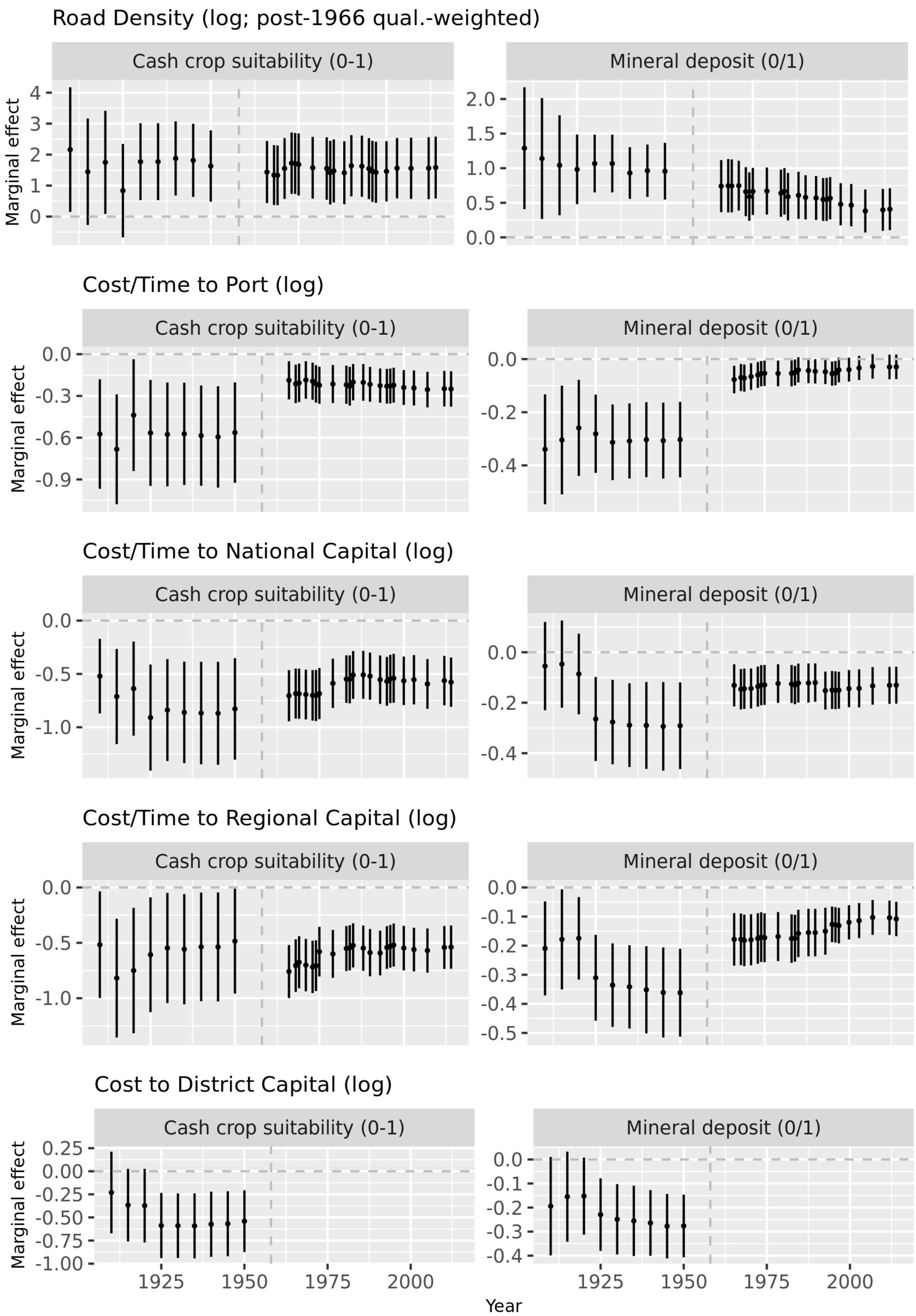


Figure 5.6: Estimated effect of natural resources on transport infrastructure across all years. Note the break in the time series between the colonial and postcolonial data. Each coefficient results from estimating Equation 5.1 with data from the year indicated on the x-axis. Bars indicate 95% confidence intervals.

and Moradi, 2016) where they connected to feeder roads. I find no such railroad-related gap with regard to areas' connectedness to district, regional, and national capitals for which roads were more important. Here, the effects are similar across types of administrative capitals and periods. For example, increases in soils' suitability for growing cash crops by a standard deviation decrease 1950 (1966) transport costs (times) to the national capital by -16 [-24, -7.1] (-14 [-18, -9.3]) percent. As before, we see strong path dependency in the effects, which remain relatively stable across the entire century.

The second column of Figure 5.6 plots the estimated effects of mineral deposits on transport infrastructure. In parallel to the effects of local cash crop suitability, the results show that areas with a mineral deposit received substantially more investment in local road infrastructure than areas without a deposit. The effects on local road densities associated with the dummy are large and amount to 160 [73, 290] (110 [44, 210]) percent in 1950 (1966). However, this effect declines over the second half of the century, a process that may be driven by ceiling effects in mining towns with dense, old networks to which other areas caught up over time.

Railroads are an important driver of the effect of mineral deposits on area' connectedness to the closest port. For the colonial period, I find a stable reduction of transport costs by -26 [-36, -15] percent. The estimate decreases to -7.4 [-12, -2.5] once I estimate the effect of deposits on travel times to ports on the 1966 road networks. This effect further reduces in the years up to 2014, eventually becoming statistically insignificant after 2000.

The last rows of Figure 5.6 show that areas with mineral deposits are better connected to their administrative capitals. In 1955 (1966), mineral deposits are associated with a reduction of travel costs (times) to national capitals of -25 [-37, -11] (-12 [-19, -4.6]) percent. Again, the estimates are consistent and largely similar across district, regional, and national headquarters. Note that the 1925 jump in the coefficients is due to observations from Northern Rhodesia (today's Zambia) which enter the sample in that year. The colony featured exceptionally well-connected deposits because its national capitals (first Livingstone and, after 1935, Lusaka) were located on the railroad towards the mineral-rich copper belt. The coefficients for the colonial period are again significantly larger than for the postcolonial period, suggesting an overproportional impact of railroads. Over the post-colonial period, the

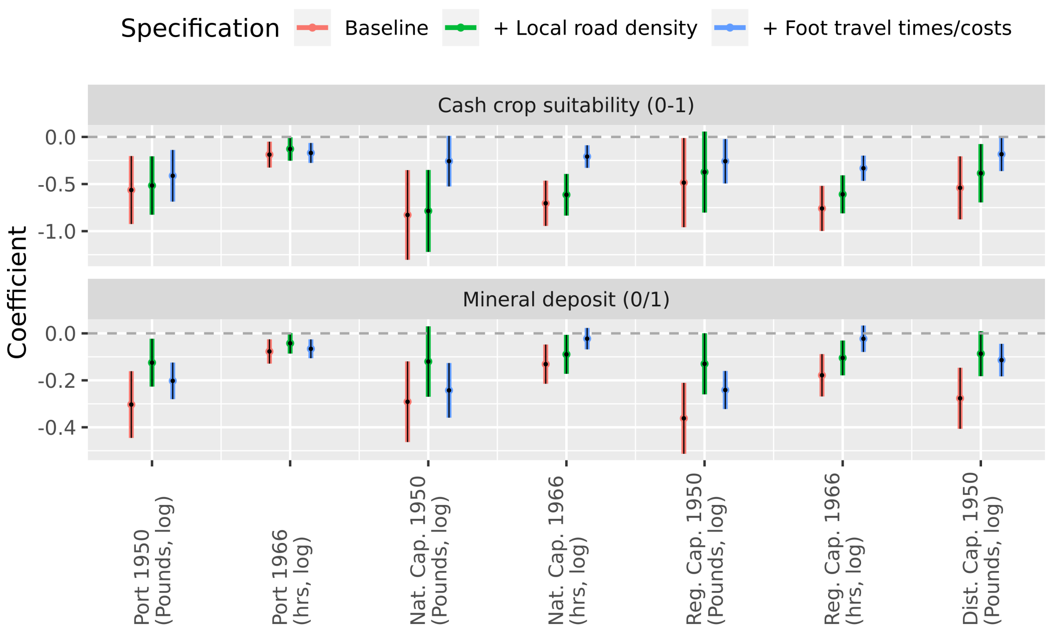


Figure 5.7: Adding local road and railroad density and 'foot-travel' costs and times to the main specification of the effects of natural resource potential on transport costs and times.

Note: All estimates are based on the baseline specification (Equation 5.1). The first set (in red) consists of the pure baseline model. The second (in green) adds measures for local transport infrastructure density. For the colonial period this is a simple railroad dummy and the local road density. For the postcolonial period, this is the local, quality-weighted road density. The third set of estimates is based on estimations that control for 'foot-transport' times and costs to ports and capitals. These are computed by treating all edges in the planar transport networks as if they where foot-paths.

estimates on mineral deposits' effect on the time to national capitals does not change by much, and their effect on the time to regional headquarters decrease by about 30 percent.

LOCAL VS. NETWORK EFFECTS: The baseline models estimated thus far do not allow for distinguishing the effects of local and non-local (i.e. network-specific) investments into transport infrastructure on transport costs and times. To disentangle them, Figure 5.7 reports the results of additional specifications that control for the local density of roads and, for the colonial sample, a dummy for the presence of railroads. These post-treatment controls allow us to gauge whether the effects on travel times and costs observed above are caused solely by local investments, or also by investments into the network as a whole.[41] As one would expect from the fact that

41 Admittedly, this is not a perfect strategy, since the presence of, in particular a railroad, is highly correlated with the presence of a railroad link between an area and, for example, a port.

local roads and rails directly affect travel costs, the size of the estimated coefficients of the resource indicators, in particular the mineral deposit dummy, decrease for most outcomes. However, they remain statistically significant throughout – for the most part well below p-values below the .05 threshold and for three coefficients at the .1 threshold. These results suggest that the design of transport networks in their entirety followed extractive purposes.

EFFECTS OF STRATEGIC CAPITAL PLACEMENTS: A second question relates to the inability of the models to disentangle the effect of capital locations from those of (rail)roads that connect to them. Both may drive reduced distances between resource-rich areas and administrative headquarters. I therefore estimate a set of specifications that account for the local 'foot-travel' times and costs towards ports as well as capitals. Computed on the same transport networks but treating all edges as "footpaths," these variables capture the geodesic distance to ports and capitals and control for their strategic placement. As Figure 5.7 demonstrates, the estimates of the effect of local resource potentials on transport costs towards capitals decrease significantly, that of the effects of mineral deposits on transport costs to national and regional capitals in 1966 even reduce to zero.[42] This indicates that administrative units have to some extent been designed in a response to the local potential for resource extraction. Consistent with the incentive of rulers to reduce the distance between administrative headquarters and resource-rich areas, the regions and districts that govern them tend to be smaller, thus decreasing the distance between administrators and the resources they want to tax (see Table E.4 in Appendix E.3).

Notwithstanding these qualifications, the results indicate that colonial aims of resource extraction have affected investments into extending transport networks and the reach of states in Africa. The resulting biases have, for the most part, persisted over time and still affect African transport networks and states' reach today. The results are thus consistent with earlier findings on the matter (Jedwab and Moradi, 2016; Roessler et al., 2018).

42 Note that the effects of local resource potentials on transport cost and time towards ports remain comparatively unaffected by the inclusion of geodesic distances. This coincides with the fact that the unindented shoreline of Africa left colonizer with few locations protected enough to build a port (e.g. Austen, 1983).

5.4.3 *Robustness checks*

To assess the robustness of the baseline results, I reestimate the baseline specification with all outcomes discussed above, always using the cross-sectional data from the years 1950 and 1966. Appendix E.3 presents all robustness checks in detail. I first add three additional vectors of control variables. The first adds a number of variables that measure additional dimensions of local soils' characteristics.[43] These affect the suitability for growing cash crops, but may have also impacted infrastructure development through other channels. Second, cash crop suitability might correlate with the local disease environment, which was a major impediment to colonial rule (e.g. Acemoglu, Johnson and Robinson, 2001).[44] Third and lastly, I control for the level of political centralization and the economic production system of local precolonial groups. This is not to avoid omitted variable bias, but rather to ensure that the observed effects are driven by a colonial preference for extraction rather than higher levels of precolonial agriculture or stateness. Adding these control vectors does not change the results in a significant manner.

The discussion of the empirical strategy has highlighted that the estimate of population densities in 1880 might be biased by current demographics which in turn might have resulted from (post-)colonial investments into transport infrastructure. Dropping the respective variable leads the estimated effects of resource potentials to increase in size and precision. This supports the view that controlling for the population estimates was a conservative choice.

In addition, one set of specifications adds fixed effects at the province level in order to ensure that the results are not purely driven by variation between areas located in very different and far-away areas in the same state. Such variation might also originate from biased data in the Blue Books or Michelin corpus. This is generally not the case.[45] In a similar vein, weighting the observations so that each colony and

43 In particular, those are: the availability of nutrients and the capacity of soils' to retain them, local rooting conditions, the level of oxygen, excess-salts, and toxic elements in the soil, the workability of the soil, and the degree of physical water scarcity. All data come from the FAO GAEZ database (FAO, 2015).

44 Measured through the suitability for the transmission of the malaria vector between mosquitoes (Gething et al., 2011) and an estimate of the local suitability for the Tsetse fly (Programme Against African Trypanosomosis, 1999). Note that the latter might be biased through the inclusion of post-treatment outcomes, in particular demographic measures.

45 With the exception of the effect of cash crop suitability on transport costs to regional capitals in 1950 in the small colonial sample which remains stable in size but comes with much more statistical uncertainty.

country receives equal weight does not change the results by much, suggesting that no country alone exerts undue influence over the estimates. Clustering the standard errors of the postcolonial analysis on the level of countries or implementing Conley's non-parametric spatial clustering of errors (Conley, 1999; Bester, Conley and Hansen, 2011) does not change the interpretation of the results.

Lastly, I check the robustness of the results towards two rather arbitrary choices made above. First, I estimate the baseline results for Voronoi cells of varying size, choosing sizes in exponential steps between 1'250 and 20'000 square kilometers. With few exception[46] discussed in the Appendix, the results are robust to this variation. Similarly, I have chosen to build the indicator for cash crop suitability by taking the maximum suitability of the eight cash crops. Re-estimating the main specifications with the five major cash crops and taking the mean instead of the max leads to results equivalent to those reported above.

5.5 INDIRECT RULE AND ROAD BUILDING

The analysis of the effect of local resources on colonial investments into transport infrastructure has highlighted the extractive incentives that drove the building of roads and railroads in Africa. In combination with the variation in colonial indirect rule explored in Chapter 4, the results raise the question whether investments into transport networks in general and in resource-rich areas in particular were contingent on the mode of local colonial governance.

Two arguments can be developed from the theory of extractive state building. First, the theory highlights the importance of bargaining power for the terms of trade between rulers and ruled. Because indirect rule increases the bargaining power of local rulers and the population, the payoff of colonial governments from resource extraction rise with the directness of rule. This mechanism decreases the incentives for investing into extractive transport networks in indirectly ruled areas. Because there are few incentives to build roads towards directly or indirectly ruled areas without resources, one would expect the effect of direct rule to increase in local resource wealth.

46 In particular, standard errors increase with the size of units, and the effect of mineral deposits on local road densities becomes smaller with larger units, presumably because of the point-like nature of deposits, that have a very local effect which becomes invisible in large units.

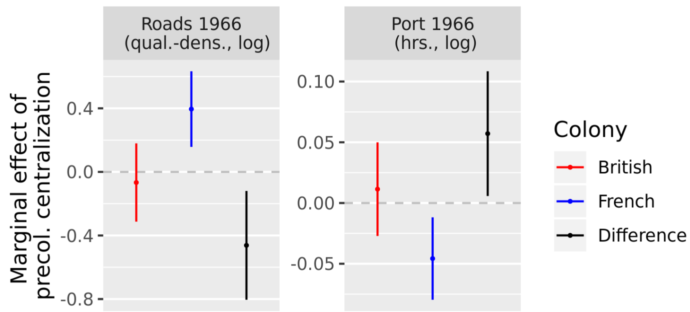


Figure 5.8: Effect of precolonial centralization on transport networks in French and British colonies.
Coefficients result from estimating the baseline specification 5.1 with the measure of precolonial centralization from Murdock (1959, 1967) and ethnic group-level controls (see Chapter 4). All variables are interacted with a dummy for British colonial rule.

However and second, local rulers and populations under indirect rule can use their bargaining power to pressure the central government to invest more into connecting them to ports and capitals. After all, good transport networks under indirect rule are as important for governing, collecting revenue, and fostering economic development as they are under direct rule. This creates local preferences for being well connected. Because they are better able to mobilize their populations, local elites under indirect rule might also be more effective at building roads – either paid by themselves or financed by the central government. One example here is the indirectly ruled Buganda government in Uganda, which levied the precolonial public-work tax *Luwalo* to build roads (Hailey, 1945, p. 1583). In sum, indirect rule might also lead to more investments into transport networks, in particular in resource-rich areas where roads have the largest return on investment. Taken together, the net effect of the two competing mechanisms is theoretically unclear.[47]

To shed empirical light on the issue, I combine the research design of this chapter with that of the previous chapter. In particular, I first estimate the effect of precolonial centralization on the density of local road networks and travel times to ports in 1966 in former French an British colonies. Precolonial centralization acts as a proxy in British colonies where centralized ethnic groups were ruled more indirectly than acephalous populations. This was not the case under rather uniform direct French rule. The results plotted in Figure 5.8 show that indirect rule in British colonies did not affect

47 Because both mechanisms work through unobserved preferences and bargaining processes, the currently available data does not allow for disentangling them empirically.

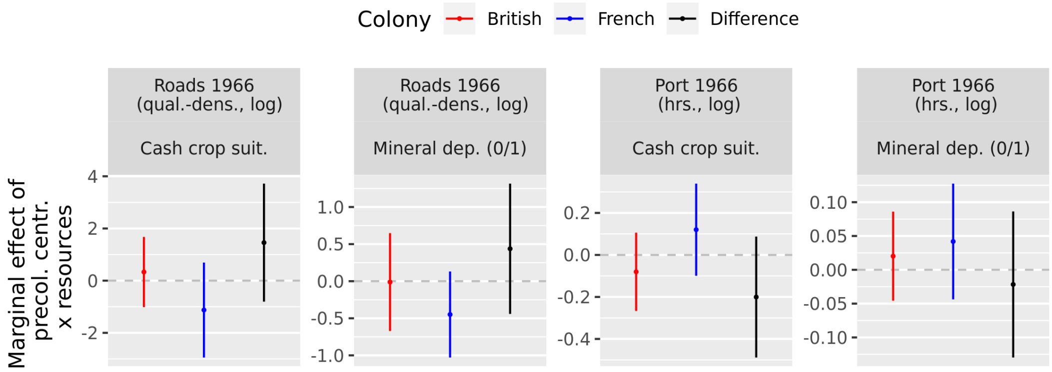


Figure 5.9: Difference in the marginal effect of local resources on transport networks in centralized and non-centralized precolonial ethnic groups under French and British rule.
Coefficients result from estimating the baseline specification 5.1 with the measure of precolonial centralization and ethnic group-level controls (see Chapter 4). Ethnic variables are interacted with the local cash crop suitability and the dummy for mineral deposits. Geographic controls are interacted with the level of precolonial centralization. All variables and interactions are themselves interacted with a dummy for British colonial rule.

investments in road networks. In French colonies in turn, the heavy-handed approach of crushing centralized precolonial polities came with more investments into road networks.

The results from the British colonies speak against more investments into transport infrastructure under direct rule. However, the pattern in the French sample could still be driven by more incentives for extraction under direct rule or, alternatively, a greater need for transport networks to control the suppressed, centralized polities. If indeed resource extraction was the driver of the pattern, we would expect that precolonial centralization and French rule led to more road building targeted at resource-rich areas. Hence, I interact the indicators for local cash crop suitability and the presence of mineral deposits with the measure of precolonial centralization. The resulting coefficients of that interaction, plotted in Figure 5.9, suggest that the effect of local resources on road building does not increase, but rather decrease with precolonial centralization in French colonies. In British colonies, these effects are estimated to be close to zero. This speaks against the argument that French or British governments built more or better roads towards directly ruled areas to extract more resources. However, given the competing arguments, incentives for more extraction in directly ruled areas may have been offset by local demands for road building more effectively voiced in areas under indirect British rule.

5.6 CONCLUSION

States in Africa, just as their precolonial predecessors struggled against space. But they did not evenly conquer it. Instead, the extractive incentives of colonial governments led them to subjugate their territory in a selective manner, bringing modern wheeled transportation primarily to areas rich in natural resources that could be exported and taxed. This chapter has provided ample empirical evidence for this resource-based bias of colonial and, later on, postcolonial transport infrastructure.

Based on new data on colonial and postcolonial transport infrastructure, the empirical analyses show that these biases where substantial. At the end of the colonial period, areas with soils suitable for cash crop agriculture featured road networks that where twice denser than in areas without such potential.[48] Where mineral deposits could be exploited, similar effects prevailed. Beyond the local level, resource rich areas were better connected to ports and the administrative headquarters of the state at the district, regional, and national levels. This not only encouraged export production, but also strengthened the reach of states towards the source-regions of export crops and minerals, thus following the extractive purposes of state builders in Africa. The analyses show few signs that postcolonial governments reduced the spatial inequalities in transport infrastructure and state reach.

Combined with the findings on the establishment of direct and indirect rule presented in Chapter 4, these findings open the way to explorations of the consequences of local state building on development and conflict in Africa. They are the focus of the second empirical part of this dissertation. While Chapter 6 takes on the effects of indirect rule on local development, I return to the physical dimension of state reach in Chapters 7 and 8. More specifically, Chapters 7 inquires the effect of changes in the reach of states towards their citizens on local development. Chapter 8 then analyzes the dual impact of road infrastructure on the ability of states to keep the peace and its challengers to rebel.

48 This calculation is based on moving from a low suitability score of .2 to one of .6.

STATE CAPACITY, DEVELOPMENT, AND CONFLICT

6

INDIRECT RULE, CASH CROP PRODUCTION, AND THE PROVISION OF PUBLIC SERVICES

Colonial states in Africa were built for extractive purposes, increasing governments bargaining power and decreasing their transaction costs where it paid off. As I have shown in Chapter 4, British colonial rulers left substantive bargaining power with precolonial institutions where they were centralized enough to be ruled in an indirect manner. In contrast, French colonial governments ruled in a comparatively direct manner. Chapter 5 has demonstrated that colonizers invested to build extractive transport infrastructure in resource-rich areas. The second empirical part of this thesis builds on these insights to study the effects of state building on development and conflict in Africa. As a first step in this effort, this chapter assesses the argument that the bargaining power of local elites and the population affects their terms of trade with the central government.

Building on the theoretical framework from Chapter 3, I argue that indirect rule left more bargaining power in the hands of local elites and gave them greater incentives to respond to the demands of their people than direct rule. In comparison, local elites under direct rule were appointed and dismissed by colonial governments, making them less responsive to the populations they governed. This difference in the bargaining power and responsiveness of local elites affected the terms of the trade of the population that exchanged taxes on natural resource production, in particular that of cash crops, for public service provision. As a result, local populations under indirect rule were able to secure more public services in return for providing the state with revenue than under direct rule.

Examining this argument, this chapter continues to build the bridge between the literature on the long-term effects of precolonial institutions and research on the impact of extractive colonialism. In particular, I extend the literature on the enduring developmental effects of precolonial institutions (e.g. Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013*b*) and argue that these effects were not only shaped by colonial modes of local rule, but also depended on local endowments with agricultural resources. With this, I simultaneously address a gap in the literature on the legacies of extractive colonialism. Here, after a wave of macro-research initiated by Acemoglu, Johnson and Robinson (2001, 2002), more recent studies have found mixed local effects of colonial resource extraction. While Peru's mining *mita* (Dell, 2010) and rubber concessions in the Congo Free State (Lowes and Montero, 2018) left behind impoverished communities, mining and cash crop production in Africa (Roessler et al., 2018) and the sugar economy in Java (Dell and Olken, 2018) came with more investments into colonial infrastructure and increased local development in the long run. Despite the diversity of these effects, we know little about why some regions profited from resource extraction while others did not. This chapter highlights the distribution of power in the colonies as an important determinant of the developmental effects of colonial resource extraction.

To test the argument that natural resource production, in particular cash crop agriculture, led to higher levels of public service provision under indirect than under direct rule, I draw on local colonial education outcomes. These are derived from contemporary DHS survey data (2018) of geocoded individuals born and schooled before decolonization. I combine these data with information on the precolonial political centralization of ethnic groups, which proxies for indirect rule in former British but not French colonies (see Chapter 4). To capture the level of local cash crop production in the absence of geographically disaggregated data on real production, I rely on soil's suitability for cash crop agriculture as an exogenous proxy.

The baseline empirical model tests the expectation that the marginal effect of cash crop suitability on primary education rates increases in the level of precolonial centralization in British, but not French colonies. Two identification strategies account for potentially endogenous sorting of ethnic groups and colonizers in space. First, I test whether the expected effects hold when I compare individuals across borders of neighboring ethnic groups with different levels of precolonial centralization, thereby balancing the natural environment in which respondents live. In a second specification, I compare individuals within the settlement regions of ethnic groups split by French-British colonial borders. This accounts for the potentially endogenous spatial sorting of colonial empires.

Together with a set of robustness checks, the analysis shows differing marginal effects of local cash crop suitability on education rates between ethnic groups and colonial empires. In British colonies, the effect of soils' suitability on primary education rates increases in the level of precolonial centralization. In the French colonial sample, this relationship is absent or even reversed. This contrast suggests that the patterns observed in the British colonies are indeed driven by local indirect rule that gave enough bargaining power to centralized ethnic groups to secure more public services in return for the taxation of cash crops. The effects of indirect rule in resource-rich areas in former British colonies still today account for variation in socio-economic outcomes, as evidenced by data on postcolonial education rates, current household-level wealth, and per capita nightlight emissions.

6.1 THEORETICAL ARGUMENT

Under what conditions does extractive governance lead to local public service provision and development? My main theoretical argument highlights that the bargaining power of rulers vis-à-vis the population determines the terms of the trade of revenue extraction for public service provision. At the same level of extraction, more bargaining power in the hands of local populations leads to more public service provision. This section builds on this logic and connects it to the colonial context empirically examined.

One important determinant of governments' bargaining power is the mode of local rule it employs in its territories. Indirect rule leaves more power in the hands of local elites than direct rule. Beyond the bargain between the central government and local elites, the relation between local elites and the people they govern determines the returns that the local population ultimately receives for providing the economic basis for state extraction.[1] Indirect rule through indigenous institutions leads to distributions of power in both relations that favor the provision of public services in return for taxing local resource production.

The first relationship that affects public service provision is the balance of power between the local ruler and the central government. In negotiating the amount of discretion over locally raised state income – whether taxed at the source or at the port – indigenous rulers under indirect rule could use their local networks of power to

1 This simplified two-level setup abstracts from the multiple tiers of governments, assuming a polity that consists of central and local rulers only. While being simplified, the general insights of indirect rule leading to more bargaining power for the local population travel to the empirically more accurate setting of three- or four-tiered administrative hierarchies.

mobilize the population against or in favor of the central government and its policies. This bargaining chip allowed indirectly ruled local governments to secure substantive influence over state revenues originating from the area they governed. In contrast, 'warrant chiefs,' local rulers lifted to power by the colonial administration in a scheme of direct rule lacked the capability to mobilize locally and did thus not possess much threat potential. In addition, they oftentimes lacked administrative efficacy, which was higher where centralized governance had existed prior to colonization. Both led to lesser budgetary discretion. Perham's (1937, 72) account of native treasuries in Northern Nigeria in 1911 reflects this logic. By virtue of the varying degrees of indirect rule, more institutionalized precolonial polities were able to secure a greater share of local state revenues and greater flexibility on how to use them:

> The highest proportion [of revenue] was retained by the Sultan of Sokoto; most of the Emirates kept a half; a quarter went to the smaller units, while in the pagan areas, where no foundation for treasuries existed, the Government took the not very considerable whole and paid small subsidies to the chiefs. The delegation of responsibility in administering the Treasury varied from Kano, where accounting was already well understood, down to the small central Emirates where it was hard to find literate officials.

With regard to the second relationship, that between local rulers and their population, indirect rule made local rulers dependent on their constituents and thereby created incentives to pass their revenue on to them. As traditional rulers, local elites under indirect rule based their customary and often inherited power on networks of patronage that extended, via the district- and village-level, down to the subjected population. These networks provided elites with the social ties needed to assemble information and enforce their rule. However, these ties also worked the other way around, transporting demands upwards and pressuring local elites to act on the preferences of their constituents. The combination of information on popular preferences and the incentives to act on them provided the grounds for responsive governance (Baldwin, 2016; Tiebout, 1956).

Empirically, this argument is well in line with evidence that decentralized governance typically lowers corruption (e.g. Fisman and Gatti, 2002) and increases the provision of public services where needed (e.g. Faguet, 2004; Iyer, 2010). Better governance might in turn have increased governments' legitimacy and tax compliance (Bodea and

Lebas, 2016; Levi, 1988; Timmons and Garfias, 2015), being one driver behind the higher local tax revenue observed under indirect colonial rule (see Chapter 4).

In contrast, newly installed rulers in directly ruled areas were largely independent and agnostic of the populations they governed (Cohen, 1971*b*,*a*; Crowder, 1968). Coming to power as agents of the colonial government, they lacked pre-established ties to the local population. They thus had only sparse information on their people's preferences and did not experience much pressure from below through formal or informal institutions. Corrupt and unresponsive governance oftentimes resulted, as illustrated by the colonial history of parts of South-Eastern Nigeria. Here, chiefs were appointed through warrants enacted by colonial officers. Afigbo (1972) recounts how local governance was ripe with corruption and fierce fights over offices, involving chiefs as well as British officials. In a similar vein, resistance against corrupt warrant chiefs and the taxes they collected were key drivers of the 'Women's War,' a tax rebellion that erupted in Southeastern Nigeria in 1929 (Martin, 1988; Perham, 1937).

Given the extractive aims of the colonial state and its inability to steer their warrant chiefs for a lack of personnel (Kirk-Greene, 1980), colonial authorities and institutions hardly neutralized this lack of incentives for good governance. This stands in stark contrast to settings in which a directly governing state has more control over its agents or where these are held accountable through elections. Here, indirect rule and persistent power of uncontested local autocrats lead to a lower quality of governance (Acemoglu, Reed and Robinson, 2014; Lange, 2009; Mamdani, 1996).[2]

With indirect colonial rule tilting the terms of trade in favor of the local population, its absolute benefits crucially depended on the value of resources it produced and paid taxes on. As highlighted in the previous chapter, the colonial state was financed mostly through taxing agricultural and mineral produce at the ports and, to much lesser degree, at the source (Havinden and Meredith, 1993; Gardner, 2012). For late colonial Nigeria, Helleiner (1966, 163) estimates that the production of major cash crops was taxed at rates between 21% and 32%. Estimating the overall level of extraction of rents from cash crop production in French colonies, Tadei (2014) arrives at a similar magnitude of 25% to 40%. Produced mostly by smallholders, the taxation of cash crops set in motion the bargaining between colonial governments, local elites, and the population described above. In comparison, mineral production occurred in few

2 Note that Acemoglu, Reed and Robinson (2014) study the effects of political competition among chiefs in Sierra Leone where these are elected by non-representative councils of local elites.

quickly developing towns through large, oftentimes foreign firms that employed wage labor (e.g. Hopkins, 1973, p. 210). Indirect rule with its pre-existing networks of power that connected local elites with the population is much less likely to have had a positive impact under these circumstances. Taken together, these arguments lead to the main hypothesis of this chapter:

H 6.1: *The effect of cash crop production on the provision of public services increased in the indirectness of local colonial rule.*

In empirical terms, I expect that the effect of cash crop production on public services increased in the level of precolonial political centralization of ethnic groups in British colonies, since British indirect rule was only implemented where pre-existing institutions allowed for doing so (Chapter 4). Because the French actively dismantled precolonial institutions and strove to rule directly across their territory, I do not expect these patterns in former French colonies.

6.2 DATA AND EMPIRICAL STRATEGY

To test the hypothesis that cash crop production led to more provision of public services under indirect than under direct rule, I analyze data on the level of education of individuals born at least 6 years before independence in 24 British and French colonies in Africa. Building on Chapter 5, I proxy the extent of local cash crop production with data on soils' suitability for cash crops agriculture. Following Chapter 4, I capture the degree of local indirect rule by relying on data on the precolonial institutions that formed the basis of indirect rule in British, but not French colonies where if at all direct rule was applied to politically centralize regions. The next section discusses the data and the empirical design of the analysis.

6.2.1 *Data*

To measure the extent of public service provision during the colonial period, I rely on the level of education of individuals raised before independence. Education was among the central public services provided by the colonial powers. Not only was education important to recruit capable colonial agents (Gifford and Weiskel, 1971), but it was also in high demand among local populations and elites (Cogneau, 2003;

Hicks, 1961; Mair, 1971). However, there were large differences between the education policies of the British and the French colonial governments, some of which can be connected to the differences in the model of colonial rule they applied (Cogneau, 2003; Cogneau and Moradi, 2014; Gifford and Weiskel, 1971). Schooling in French colonies was provided in French, free of charge, in a secular manner, and under the control of the colonial administration. Following their preference for decentralized institutions, British administrations relied heavily on local governments and missionaries to provide much more widespread education (Gifford and Weiskel, 1971).

To make up for the lack of detailed official statistics on local education provision from the colonial period, I rely on education data from contemporary surveys. In particular, the Demographic and Health Surveys (DHS, 2018) offer geo-referenced data on the educational achievements of about 250'000 individuals born at least six years before the independence of their respective French and British colonies. By taking these data to measure the main outcome of this chapter, individuals' primary education, I exploit the fact that educational outcomes are determined during childhood after age six (see e.g. Cogneau, 2003; Franck and Rainer, 2012). The DHS (2018) has been fielded since the late 1980s across most countries in Africa. The DHS includes standardized questions on respondents' and their household members' age and educational attainment, most importantly whether a household member has attended primary school. I multiply the respective dummy by 100 so that we can directly interpret marginal effects as percentage points. Crucially, many surveys provide the geographical coordinates of survey clusters. Assuming that individuals oftentimes live where they grew up and and that migration is unbiased,[3] I use this information to match the DHS data with geographical data on precolonial institutions, soils' suitability for cash crop production, and the colonial ruler of an area.[4]

Note that the main analysis draws on the data for all household members in the data. Only 10 percent of these were interviewed directly by the DHS, but as part of these interviews, respondents were asked to provide information on the remaining household members. As a robustness check shows (see Appendix F.3.5), baseline results between the full ('Household Member Recodes') and reduced samples ('Individual' and 'Men's'

3 Note that a robustness check in Appendix F.3.4 provides evidence that the results are not driven by migrants in the sample.

4 This is done using the Cshapes package (Weidmann and Gleditsch, 2010). For Nigeria and Cameroon, I reconstruct the border that separated British Nigeria from the French Cameroon using administrative boundaries from GAUL (FAO, 2014).

Recodes) are very similar. However, some of the specifications estimated below require the power of the full sample.

Figure 6.1a illustrates the education rates of georeferenced DHS respondents aged six and above at independence. The difference in overall education rates between French and British colonies is striking (Cogneau and Moradi, 2014). On the French side, only Cameroon and Madagascar exhibit levels as high as those in the British colonies.[5] As Figure 6.1b shows, this difference between the two empires has persisted for decades. While most former British colonies have come close to achieve universal primary education by the 1990s, this is not the case for former French colonies. In addition to the cross-colony variation, the map highlights substantial spatial variation within the colonies. This variation is the main explanandum of the empirical analysis.

To measure the main treatment, cash crop production, and its mediator, indirect rule, I recur to the arguments of the two previous chapters. As with official statistics on education, no data on the quantity and value of local colonial cash crop production is currently available.[6] Building on Chapter 5, I therefore use soils' suitability for cash crop production as an exogenous proxy for real production.[7] This approach has the advantage that soil suitability depends only on geographical and climatic features, all of which are exogenous to the observed outcomes. However, the use of cash crop suitabilities as a proxy for real production comes with two caveats. First, cash crops were not grown in every suitable region of the continent. Second, the functional form of the relationship between soils' suitability and real production is unknown. Given these shortcomings, the proxy thus captures nature's 'intention to treat' a location with cash crop production, with the actual treatment being unobserved.[8] As in Chapter 5, I calculate the local cash crop suitability (CCS) by taking the local maximum suitability across the eight main cash crops with values ranging between 0 (no suitability) and 1 (perfect suitability).[9]

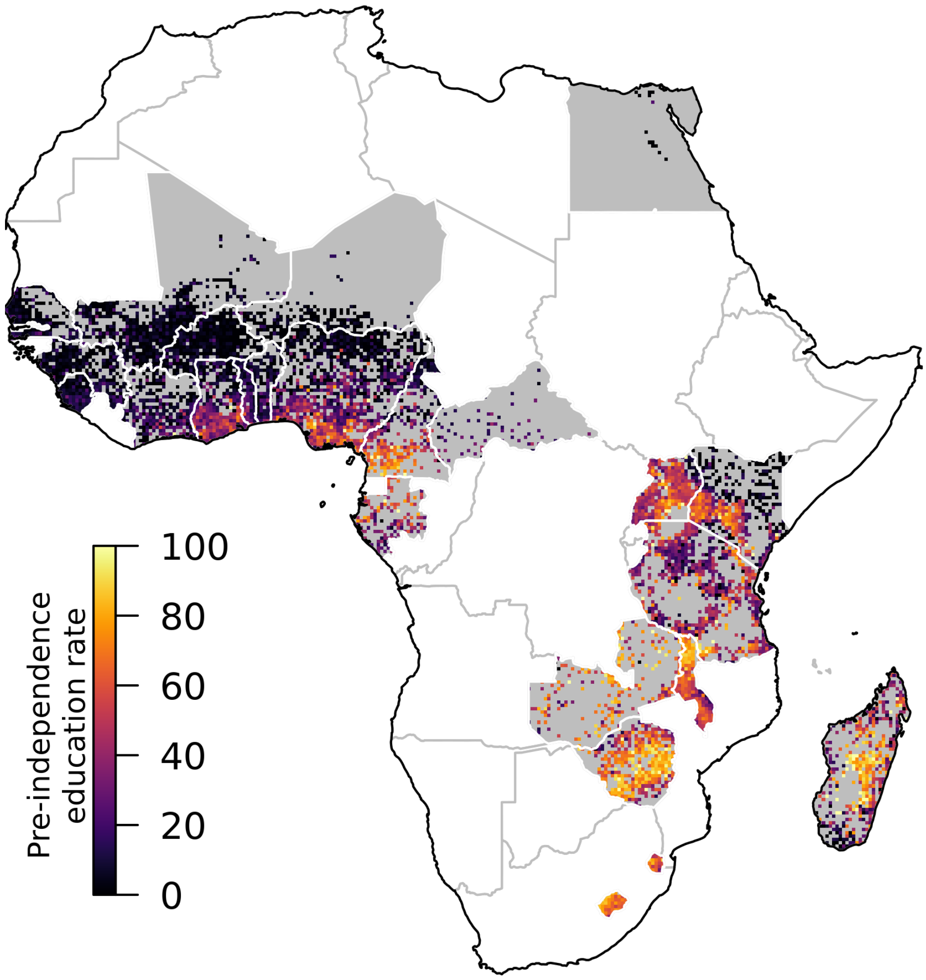
5 On the British side, the negative outlier is Sierra Leone, which has caught up only since the 1980s.

6 The extensive data digitized by Roessler et al. (2018) is not yet openly accessible. Since the goal of this analysis is to explain general patterns that exist across colonies of the same empire as well as differences between those patterns, single maps on cash crop production (as in Jedwab and Moradi, 2016) that are not standardized across colonies are ill-suited for this analysis.

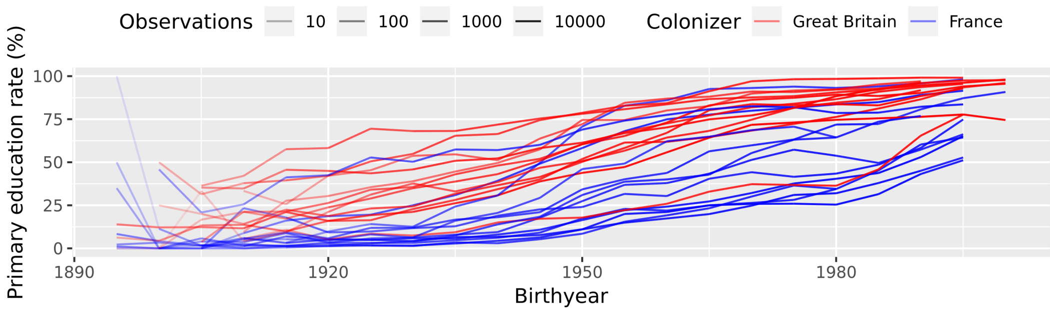
7 See also Nunn and Qian (2011) who, lacking data on potato production, use estimated maps of the suitability of local ecologies as a proxy.

8 Note, however, that Roessler et al. (2018) show that soils' suitability for cash crop production is a valid predictor for real production in the late-colonial era.

9 These are: cocoa, coffee, cotton, groundnut, oil palms, sugarcane, tea, and tobacco. For details on the measure, see Figure E.4 in Appendix E. For robustness checks that draw only on the five most important cash crops and take the mean instead of the max, see Appendix F.3.



(a) Primary education rates among pre-independence cohorts in French and British colonies. The map plots average education rates of individuals born at least six years before their country's independence per 25×25km grid cell. Grey grid cells do not contain any observations. White areas are not included in the analysis because their were not colonized by the British or French or lack geocoded DHS surveys.



(b) Primary education rates in (former) French and British colonies over time. Education rates are aggregated into five-year bins.

Figure 6.1: Primary education rates over space and time in colonial and postcolonial Africa. Data retrieved from the DHS Personal Recode data.

To measure local indirect rule, I draw directly on the finding from Chapter 4 that the British ruled indirectly where high levels of precolonial political centralization allowed them to do so. The French, in turn, ruled in a more direct manner throughout their colonies. I therefore take the interaction between the level of precolonial centralization from Murdock (1959, 1967) and a dummy for British rule as a proxy for indirect rule.[10] To facilitate the interpretation of the results, I will for the most part show the results of models estimated separately for the French and British samples. While I expect that the effect of cash crop suitability increases in the level of precolonial centralization in the British colonies, this should not be the case in the French territories. These mainly serve the purpose of illustrating the counterfactual situation of no variation in indirect rule that would affect the impact of soils' suitability on education rates.

6.2.2 *Empirical strategy*

With the data on individuals' primary education achievement, local soils' suitability for cash crop production (CCS) and precolonial centralization (PCC), I estimate the following baseline model on the French and British samples:

$$Y_i = \alpha_{c,t} + \gamma_s + \beta_1 \mathsf{PCC}_l + \beta_2 \mathsf{CCS}_l + \beta_3 \mathsf{PCC}_l \times \mathsf{CCS}_l + \boldsymbol{\delta X}_l + \epsilon_{i,l,c,t,s} \tag{6.1}$$

To isolate the joint impact of cash crop suitability and precolonial centralization on the education level of respondent i measured by the DHS across 24 colonies c and a total of 87 surveys s of varying composition, I include rigid fixed effects in all models. Since the main focus lies on cross-sectional effects, I include country×birth-year ($\alpha_{c,t}$) fixed effects. Survey fixed effects (γ_s) capture variation in the design of DHS surveys over time and across countries.[11] I cluster standard errors on the level of ethnic groups nested in colonies.

10 Note that one might use this interaction as an instrumental variable for observed indirect rule. Three reasons advocate against this approach. First and as I demonstrate in Chapter 4, there is not one catch-all measure on indirect rule to begin with. Second, using the measures from Chapter 4 would severely reduce the size of my sample. Third, using the interaction as an IV for any of those measures would require strong assumptions on the exclusion restriction, in particular that there is no effect of the centralization in British colonies on education rates that does not move through indirect rule measured in one particular manner. Given the multiple dimensions of indirect rule (i.e. the imperfect correlation between its different proxies) and potential difference in the treatment of centralized ethnic groups by the British and the French that have not much to do with indirect rule, this assumption would be implausible.

11 A survey is one round of the DHS conducted in one country.

Just as the main two independent variables, the composition of the vector of controls $\boldsymbol{X}$ follows the specifications used in the previous two chapters. As the minimalistic baseline, I control for respondents' age and its square,[12] as well as a dummy for female respondents. To account for the main omitted variables that influence the suitability score of soils for cash crop production, I control for local geographic and climatic conditions.[13] In order to distinguish the effect of precolonial centralization from that of other precolonial attributes of ethnic groups, I control for their dependence on agriculture and husbandry, the intensity of their agricultural activities (all from Murdock, 1959, 1967), as well as the local population estimate in 1880 (from Goldewijk, Beusen and Janssen, 2010).[14] Because the main variable of interest is the interaction between cash crop suitability and precolonial centralization, I include all control variables as constitutive terms as well as in interaction with either the measure for cash crop suitability (for the ethnic controls) or the measure for precolonial political centralization (for the geographic controls). This ensures that potential omitted variable bias does not sneak in through the backdoor of the interaction term.[15] I pool the samples from the British and French colonies where I assess the difference in the marginal effect of the interaction of cash crop suitability and precolonial centralization between the two empires. When doing so, I include the full set of interaction terms, interacting each term on the right hand side of Equation 6.1 with a dummy for British rule.[16]

12 Even though I include colony by cohort fixed effects, the age within each cohort varies as the cohort becomes older in later surveys. Because well-educated people may die later, this may introduce bias into the analysis.

13 The local mean annual temperature, precipitation, evaporation, and the ratio of precipitation and evaporation, as well as the mean altitude and slope of an area, all from FAO (2015). Also, I control for the local agricultural suitability score from Ramankutty et al. (2002), as well as a locations' logged distance to the coast, border, and closest navigable river.

14 As highlighted in the previous chapter, controlling for this population estimate is a conservative approach as it might introduce post-treatment bias that captures local development levels and might thus works against finding evidence in favor of the hypothesis.

15 In addition, I implement a robustness check that includes the square terms of the main constitutive terms to address the threat that they have non-linear effects that drive the results (see Appendix F.3.5).

16 This results in the following Equation:

$$Y_i = \alpha_{c,t} + \gamma_s + \beta_1 \mathsf{PCC}_l + \beta_2 \mathsf{CCS}_l + \beta_3 \mathsf{PCC}_l \times \mathsf{CCS}_l + \beta_4 \mathsf{PCC}_l \times \mathsf{British}_l + \beta_5 \mathsf{CCS}_l \times \mathsf{British}_l + \beta_6 \mathsf{PCC}_l \times \mathsf{CCS}_l \times \mathsf{British}_l + \boldsymbol{\delta X}_l + \boldsymbol{\delta X}_l \times \mathsf{British}_l + \epsilon_{i,l,c,t}$$

Due to this full set of interactions and except for the numerically negligible impact of non-nested survey fixed effects (surveys in Cameroon took place in former British and French areas), this specification yields results that are equivalent to those of the split sample regressions. To avoid further complexity, I therefore report the pooled results only in the Appendix.

With this baseline empirical strategy, biasing factors that might have influenced the spatial sorting of ethnic groups and colonial empires remain unobserved. To counter this threat, I employ two strategies below. First, I identify the effect of precolonial institutions in interaction with cash crop suitability on education rates by only comparing respondents from contiguous ethnic settlement areas with diverging levels of precolonial centralization (see also Michalopoulos and Papaioannou, 2013*b*). This limits the potential bias of unobserved geographical, climatic, and soil-related variables.[17] Second, I exploit differences within ethnic groups split by French-British borders (see e.g. Ali et al., 2018; Michalopoulos and Papaioannou, 2013*a*) to identify the effects of the differing application of indirect rule. This strategy minimizes the potential bias from colonial powers endogenous responses to precolonial ethnic institutions and environmental conditions.

6.3 RESULTS

The following empirical analyses support Hypothesis 6.1 that British indirect rule fostered the translation of resource rents into public services. The results show that the effect of cash crop suitability on primary education increases with precolonial political centralization in British, but not French colonies. Within colonies, these results hold when the analysis is restricted to variation across ethnic borders. I also find a consistent, yet less precisely estimated difference between the French and British patterns within ethnic groups split by French-British borders. Additional robustness checks show that the effects are applicable to higher educational outcomes, are not due to migration-induced biases in the data or caused by alternative mechanisms such as colonial infrastructure or missions. Finally, I also report that British indirect rule in resource rich-areas is associated with long-term effects on current education rates, household wealth, and economic activity. The presentation of the results follows the order of this summary. Throughout, Appendix F presents all further details and results.

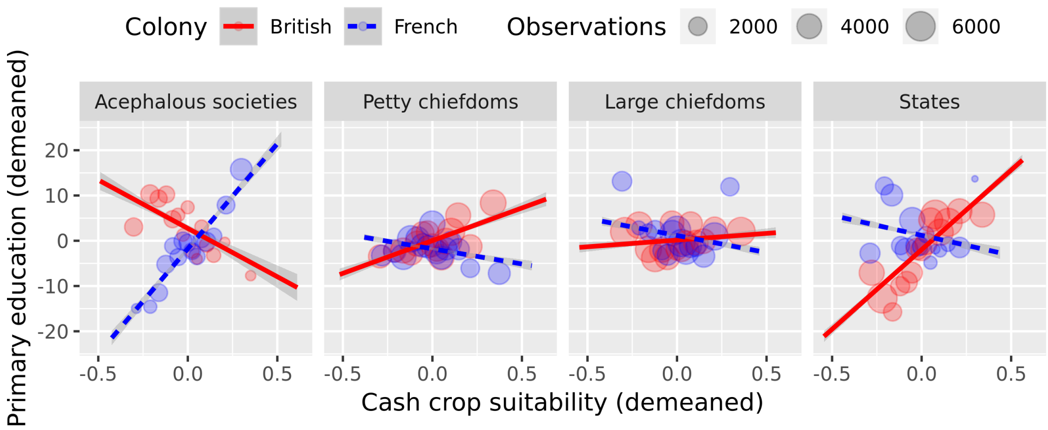


Figure 6.2: Correlation of cash crop suitability and primary education by level of precolonial centralization in French and British colonies.
Primary education and cash crop suitabilities are demeaned with the colony×birth-year and the survey fixed effects. The lines plot the results of a linear regression, the points show observed values after clustering DHS respondents' cash crop suitability into 15 bins of equal size.

6.3.1 *Baseline results*

Providing a first visual impression of the relationship of soils' suitability for cash crop production and primary education, Figure 6.2 plots the raw correlation of the two variables (demeaned through the main fixed effects) by level of precolonial centralization and identity of the colonizer. While the correlation between soils' suitability and finishing primary school is negative in acephalous societies under British rule, it turns positive in precolonial states. In French colonies, the change of the correlation over the levels of precolonial centralization is reversed. We see a positive correlation between suitabilities and education rates in acephalous societies, but a negative one in centralized ethnic groups. Note however, that the change in these correlations over the levels of precolonial centralization is not linear in both empires. In the British one, acephalous societies and those with a state most strongly exhibit the expected relationship. In the French empire, the difference between acephalous societies and more centralized ethnic groups drives the overall pattern.

Table 6.1 and Figure 6.3 present the results of the main specification estimated separately for respondents born at least 6 years before independence and interviewed in former British and French colonies. Primary education rates among respondents

17 Balance tests are reported in Table F.2.

from former British colonies exhibit the pattern described before: education rates increase in the level of cash crop suitability to a greater extent in precolonially centralized than non-centralized ethnic groups. This change in the marginal effect of cash crop suitability is statistically significant and large. In the 'British' Model 1, an increase in the local degree of cash crop suitability by one standard deviation (0.18) insignificantly decreases education rates in an acephalous society ($\mathsf{PCC} = 0$) by -1.7 [-6, 2.6] percentage points but increases the same by 5.1 [2.5, 7.6] percentage points in a precolonial state ($\mathsf{PCC} = 3$). In the sample of individuals from former French colonies, the results from the same specification do no show an increase in the marginal effect of cash crop suitability on the primary education rates. Here the sign of the interaction effect points towards a *negative* interaction effect. This implies that education provision increases less with local cash crop suitability in centralized than in non-centralized areas.

Table 6.1: Indirect rule, cash crops, and colonial education

	Primary Education (0/100)					
	British colonies			French colonies		
	(1)	(2)	(3)	(4)	(5)	(6)
Precol. centr. (PCC)	−7.600***	41.583*	61.247***	5.840***	72.733**	59.981**
	(2.847)	(23.726)	(21.284)	(2.154)	(35.700)	(26.466)
Cash crop suit. (CCS)	−9.262	−11.150*	−40.647*	20.182	10.527**	46.973*
	(12.184)	(6.045)	(23.271)	(13.054)	(4.749)	(25.999)
PCC × CCS	12.328**	13.160***	7.021***	−11.399*	−8.594***	−1.485
	(5.363)	(2.955)	(2.365)	(6.265)	(2.736)	(3.129)
Colony×Birthyear FE	yes	yes	yes	yes	yes	yes
Survey FE	yes	yes	yes	yes	yes	yes
Ind. controls:	yes	yes	yes	yes	yes	yes
Geo. controls:	no	yes	yes	no	yes	yes
Ethn. controls:	no	no	yes	no	no	yes
Mean DV:	49	49	49	18	17	17
Observations	192,650	184,872	184,872	150,072	147,539	147,539
Adjusted R^2	0.211	0.271	0.288	0.265	0.315	0.333

Notes: OLS linear probability models. Standard errors are clustered on ethnic groups within colonies. Individual controls consist of respondent's sex and age. Geographical controls consist of their distance to the coast, border, and the closest navigable river (all logged), the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and ethnic groups' dependence on agriculture and husbandry as well as the intensity of agriculture. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

To account for other factors that might drive this interaction effect, I add the vectors of geographic and ethnic control variables in columns 2-3 and 5-6 of Table 6.1. The interaction effect for the British sample remains stable in the second specification,

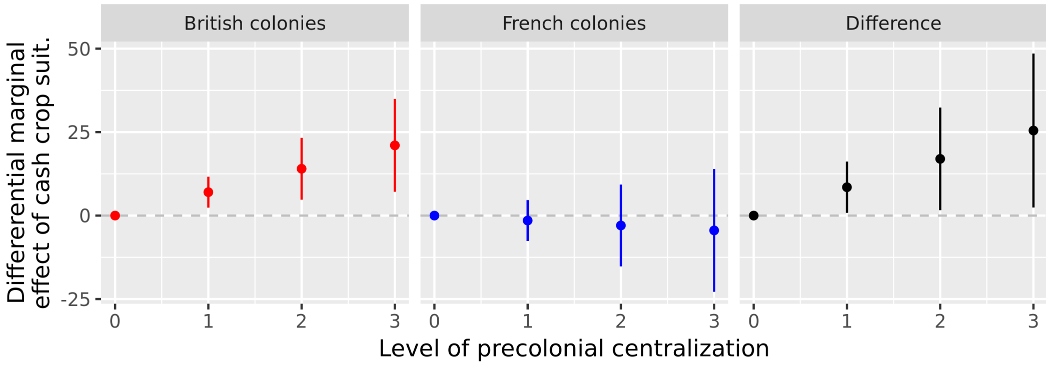


Figure 6.3: Differential marginal effect of cash crop suitability on primary education across observed values of precolonial centralization, with a value of 0 (acephalous societies) as the baseline for comparison.
Calculated on the basis of the full specification with all controls estimated on the pooled sample in Table F.3 in the Appendix. The pooled model adds an interaction term of a dummy for British rule with each variable in Equation 6.1.

but decreases significantly when I add the vector of ethnic control variables. In substantive terms, the respective result implies an increase in the effect of a one standard deviation increase of cash crop suitability by 4.2 percentage points as one moves from an acephalous society to a precolonial state. For the French respondents, the estimate of the interaction effect remains negative but becomes indistinguishable from zero in the full specification. Throughout, however, the interaction term remains significantly smaller in the French than in the British sample (Figure 6.3).[18] It should be noted that we cannot interpret the constitutive terms cash crop suitability and precolonial centralization as such when the control variables are added to the models. This is because they are interacted with the additional control variables. The constitutive terms therefore capture uninformative conditional marginal effects at an unrealistic value of zero for all controls.

6.3.2 *Comparing neighboring ethnic groups*

In order to reduce the potential for omitted variable bias due to unobserved geographical, or climatic factors, I restrict the exploited variation to respondents born in the same year that live across borders between ethnic groups with different levels of precolonial centralization.[19] In econometric terms, I thus add ethnic-pair×cohort

18 For the results of the fully specified model that is used to estimate the triple difference, see Table F.3 in the Appendix.
19 Ethnic groups are only paired within colonies.

fixed effects to the baseline model 6.1 and restrict the sample by setting a cutoff for the distance of respondents to their neighboring ethnic group.[20]

At a cutoff of 100km of respondents to their neighboring ethnic group, the design improves the balance on the vector of geographic controls in both samples.[21] However, imbalances remain in both and differ between them. In the British sample, 1880 population densities and ethnic groups' dependence on agriculture are imbalanced. In the French sample, imbalances relate to local altitudes, the evaporation to transpiration ratio, and the intensity of ethnic groups' agricultural activities. In addition to these patterns, the estimated effect of the interaction term $\mathsf{PCC} \times \mathsf{CCS}$ might still be driven by interactions of either PCC or CCS with the control variables. In order to preclude omitted variable bias originating from observed confounders to affect the results, the main specification includes all controls and their interaction terms.[22]

Figure 6.4 visualizes the main results for this specification, varying the cutoff of the distance of respondents to the ethnic border between 25 and 500km (the maximum value observed in the sample). As expected from the imprecise geographic data on ethnic borders, the main interaction term $\mathsf{PCC} \times \mathsf{CCS}$ is insignificant at the lowest distance to the border. However, as the cutoff is raised, its coefficient increases in the British, reaching a statistically significant value of 5 at a cutoff of 75km. This coefficient is slightly smaller than the value of 7 resulting from the full baseline specification. In contrast, it decreases and then increases again in the French sample. Already at a cutoff of 50km, the difference between the coefficients from the two empires is statistically significant and close to the baseline difference. Mirroring the results from the baseline analysis and assuming that there are no unobserved imbalances that affect the comparison of individuals across ethnic borders, these findings suggest that the potential spatial sorting of ethnic groups does not bias the baseline results.

20 This use of ethnic settlement borders coded by Murdock (1959) deviates from the ideal regression discontinuity design insofar as no trends towards the border are included in the model. This is due to the imprecision of Murdock's original map (Murdock, 1959) and the fact that the authority of precolonial polities faded towards the periphery of their territory (Blanton and Fargher, 2008; Wilfahrt, 2018). In addition, any polygon-based ethnic map that represents ethnic groups with sharp binary borders fails in most cases to reflect the mixed ethnic composition of local populations accurately (Müller-Crepon and Hunziker, 2018). The imprecision of the data biases the results towards zero at the border and prevents the implementation of a full regression-discontinuity design with geographic trends towards the border.

21 I test the balance on all 'geographical' and 'ethnic' controls by estimating the baseline specification without any controls and using each variable as an outcome. See Appendix F.

22 See Table F.4 for the full results with and without control. Including the full vector of controls indeed reduces the size of the coefficient of the main interaction term of interest.

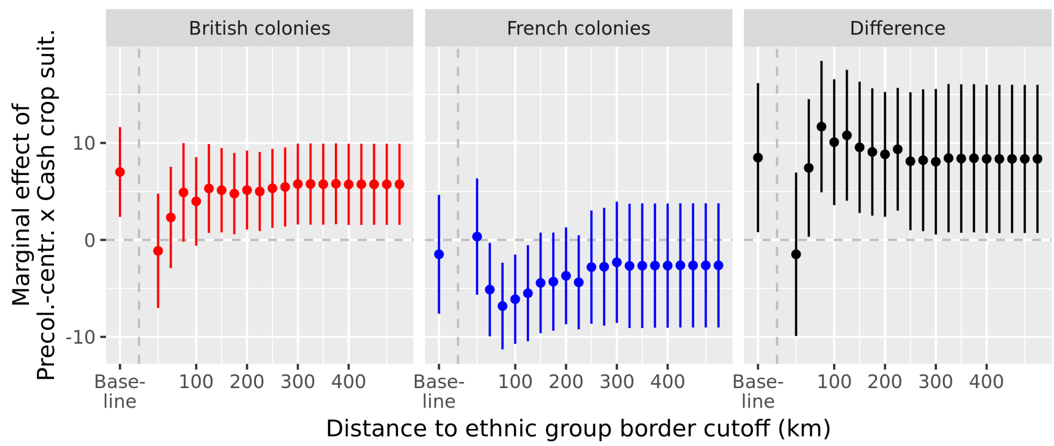


Figure 6.4: Results from the ethnic-pair design: Marginal effects of the interaction of cash crop suitability × precolonial centralization across varying maximum distances of respondents to their neighboring ethnic group.
Calculated across varying cutoffs on the basis of the full specification with ethnic-pair×birth-year fixed effects estimated on the British and French (see Table F.4 in the Appendix), and – for estimating the difference in the coefficients, the pooled samples. The pooled model adds an interaction term of a dummy for British rule with each variable in Equation 6.1. Baseline estimates plotted to the left of each panel result from the fully specified baseline specification.

6.3.3 *Using split ethnic groups to identify differential effects in British and French colonies*

A second caveat of the baseline results relates to the potentially endogenous spatial sorting of the colonial empires that might have responded to precolonial ethnic institutions or the natural enviroment. To mitigate this threat to inference, I follow Michalopoulos and Papaioannou (2013*a*) and Ali et al. (2018) and exploit the fact that the colonial borders drawn during the Scramble for Africa cut through a number of precolonial ethnic groups. Exploiting variation from within these ethnic groups, I can assess the double difference in the marginal effect of cash crop suitability on primary education rates within centralized and non-centralized groups governed by the British and the French. Econometrically, this research design requires (1) adding an ethnic group×birth-year fixed effect to the baseline specification used for the pooled sample and (2) restricting the sample to respondents who live in ethnic settlement areas split by French-British borders.[23]

23 I refrain from estimating two variants of this design. First, one could, in principle, estimate the discontinuity of the interaction term $\mathsf{PCC} \times \mathsf{CCS}$ at the French-British border. This seems inadvisable for two reasons. First, the pure and much simpler within-group setup achieves good balance on the covariates (see Table F.2). Second, estimating the discontinuity would make the model much more

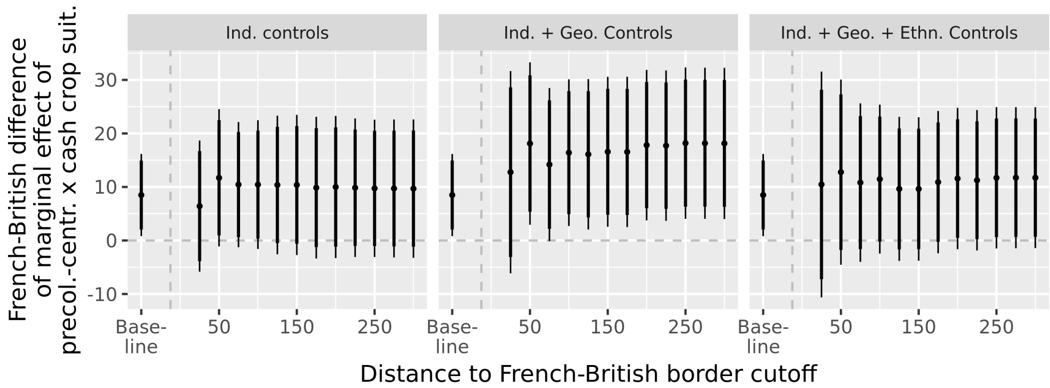


Figure 6.5: Results from the split-ethnic-group design: Marginal effects of the interaction of cash crop suitability × precolonial centralization across varying maximum distances of respondents to the closest French-British boundary.
Calculated across varying cutoffs and adding the main vectors of controls (with interaction with the British dummy) with ethnic group×birth-year fixed effects estimated on the pooled sample (see also Table F.5 in the Appendix). Baseline estimates plotted to the left of each panel result from the fully specified baseline specification.

With the resulting specification, balance tests show French-British differences in the marginal effect of the interaction of PCC × CCS on two pre-treatment co-variates – the population density estimate in 1880 and precipitation (both with $p < .1$, Table F.2 in the Appendix). This is not all too surprising given that I test balance on 11 covariates.[24] In addition, the first imbalance might well result from the post-treatment data used for estimating historical population data (Goldewijk, Beusen and Janssen, 2010). However and to prevent the possibility that it is not the interaction of British × PCC × CCS but a covarying interaction term with one of the covariates that is driving the results, I sequentially add the covariates and their respective interaction terms to the Models.

Figure 6.5 visualizes the results from this analysis. The plot shows the estimate difference between the interaction of PCC × CCS observed on the French and British sides of the split ethnic groups. To test for the robustness of the results near the colonial borders, I estimate the model with the sample of observations reduced by cutoffs of their distance to the border between 25 and 300km (the maximum distance observed).

complex, since it would require not only to let the interaction term, but also its constitutive terms vary at each side of each border. As a second alternative, I could restrict the ethnic groups split by colonial borders to those split by French-British borders perpendicular to the West African coast line (see Chapter 4). This strategy, while exploiting potentially more exogenous borders, would however lead to a further decrease in the number of observations in a setting where statistical power is already much lower than at the baseline.

24 The ethnic group fixed effects automatically balance the sample on the three variables that originate from Murdock's (1967) Ethnographic Atlas.

The results show three consistent patterns. First, across the three specifications the point estimates of the French-British difference in the coefficient of the interaction term $\mathsf{PCC} \times \mathsf{CCS}$ are close to the baseline difference, deviating upwards for the second specification only. Second, the estimates are consistent across different distance-to-the-border cutoffs. This suggests that there are no biasing geographic trends towards the border. Third, the estimates come with more uncertainty than at the baseline. Statistically significant only in the second specification, they hover around the $p < .1$ threshold for the first and third specification. This uncertainty is mainly driven by the smaller cross-border sample and the ethnic group×birth-year fixed effects, which soak up a lot of variation in the data. While the point estimates are consistent with the previous results, this uncertainty cautions against an overly optimistic interpretation of the results.

6.3.4 *Robustness checks*

Having addressed threats to inference arising from potentially endogenous spatial sorting of ethnic groups and colonizers, the following summarizes a number of additional robustness checks to the baseline model. Appendix F.3 discusses all robustness checks in detail.

A first set of additional analyses tests the robustness of the results across alternative measures of educational outcomes, of the suitability of soils for cash crop production, and of precolonial centralization. The results show that the effect of cash crop suitability on secondary and tertiary education rates increases in the level of precolonial centralization in British, but not in French colonies. The size of the estimated marginal effects is comparable to that estimated at the baseline. I obtain results very similar to those discussed above when I vary the indicator for the local cash crop suitability, aggregating across the five instead of eight most prominent cash crops, and taking their mean rather than the local maximum suitability. Lastly, I replace the indicator for precolonial centralization mapped to ethnic groups by Nunn and Wantchekon (2011) with (1) the slightly different coding from Michalopoulos and Papaioannou (2013*b*) and (2) indicators for either the presence of or minimal distance to a precolonial polities' capital in 1885 (see Chapter 4). The results from the respective models are consistent with the baseline results.

One risk of using contemporary survey data to 'travel back in time' to respondents' childhood and draw individual-level (rather than location-level) inferences relates to respondents who self-select into or out of their local colonial treatment through migration. I test for such bias by recurring to the more informative but much smaller sample of respondents born at least six year before independence that went through the full DHS interview – only 10% of all individuals used in the baseline analysis. Identifying those who have not always lived in their current place of residence as migrants, the results show that the effects reported above are not driven by differential migration rates. In the British sample, non-migrants exhibit somewhat stronger interaction effects of $\mathsf{PCC} \times \mathsf{CCS}$ as those presented in the baseline results. In the French cases, the marginal effect of cash crop suitability decreases more strongly in the level of precolonial centralization among migrants, and the interaction effect that is estimated around zero among non-migrants. [25]

Lastly, Section F.3.5 of the Appendix presents results from a number of additional specifications. In particular, I add further controls,[26] vary the rigidity of the fixed effects,[27] weight respondents so that either colonies or colony-cohorts receive equal weight in the regressions, and cluster standard errors in alternative ways.[28] None of these robustness tests leads to substantive changes in the results.

6.3.5 *Mechanisms*

The analysis has so far consistently demonstrated that precolonially centralized ethnic groups in British colonies profited more from soils suitable for cash crop production than non-centralized groups. This relationship is absent or even reversed in French colonies. However, the raw association of primary education rates with the main interaction term of interest, $\mathsf{PCC} \times \mathsf{CCS}$, provides little information about the underlying historical mechanism at work. In combination with the findings from Chapter 4 we can confidently point to indirect rule as the main driver of the difference

25 With the full set of controls, the coefficient of the interaction effect $\mathsf{PCC} \times \mathsf{CCS}$ turns to a positive value of 1.2. Its difference to the interaction effect in the British sample is with a value 11.8 slightly bigger than in the baseline specification, although estimated with more uncertainty ($p < .1$). This uncertainty might however well be due to the much smaller sample size.

26 In particular, the local disease environment and additional precolonial characteristics of ethnic groups.

27 Starting with simple colony and ending with full colony×survey×birth-year×sex fixed effects.

28 I implement a two-way clustering on the colony and ethnic group level as well as on the colony and cohort level.

in the patterns observed in the British and French colonies. However, we cannot conclude that the effects are solely due to bargaining processes that lead to more publicly provided education in fertile areas under indirect rule. Instead, they might have been alternatively caused by other factors, such as generally higher levels of economic activity or more missionary activities in these areas. Drawing on the data from Native Treasuries' budgets, mission locations, and transport infrastructure, this section provides inconclusive evidence on the first pathway, and suggests that the two alternative mechanisms do not explain the results.

NATIVE TREASURIES' REVENUES AND EXPENDITURES: In order to test whether indirectly ruled local governments indeed redistributed gains from cash crop taxation back to the local population, I estimate the baseline specification taking as the outcome the size of British Native Treasuries' revenues and expenditures, in particular those on education and social services (see Chapter 4). The results, reported in Section F.4 of the Appendix, at first sight show a consistent positive increase of the marginal effect of the local cash crop suitability in the level of precolonial centralization. However, further robustness checks show that these results are driven by a few outliers in the small sample of between 126 and 146 observed districts. Absent a larger sample of observations, this test is thus unable to provide strong evidence in favor or against the hypothesis.

AGRICULTURAL PRODUCTION: The first alternative explanation follows directly from one of the shortcomings of the data used here, namely that tax-generating cash crop production is only approximated by soils' suitability for planting cash crops – nature's 'intention to treat.' However farmers from different ethnic groups and under different types of colonial rule might have responded differently to this treatment. The results reported above might then just stem from higher levels of cash crop production, household wealth, and demand for education in indirectly ruled societies with fertile soils. Such higher production might have been possible because of higher levels of precolonial development in the respective areas. Note however, that more local cash crop production under indirect rule would not be inconsistent with the theoretical argument, if it occurred because of the greater benefits local citizens received in return for their produce being taxed by the colonial governments.

As argued in detail in Chapter 5 (see also Jedwab and Moradi, 2016) transport infrastructure was an important precondition for cash crop production. I build on this logic to gauge whether indirect rule had a direct effect on the use of good soils for cash crop production that increased education rates through private wealth. In particular, I reestimate the fully specified baseline model, adding post-treatment controls for the local presence of rail and road infrastructure as well as locations' travel times to the next port in the 1960s (see Chapter 5 for details). While transport infrastructure is positively associated with local education rates, the bad controls do not affect the estimated coefficient of the main interaction term in a substantive way. Hence, although there might be a direct effect of the interaction of cash crop suitability and precolonial institutions on agricultural production, the extent to which this pathway is captured by local transport infrastructure does not explain the main result.[29]

MISSION STATIONS: A second alternative mechanism may run through the substantial and continuous impact of missions on education rates, in particular in British colonies (Cogneau and Moradi, 2014; Gifford and Weiskel, 1971; Lankina and Getachew, 2012). Although missionary education was oftentimes co-financed by governments, missions settled in wealthier and healthier places first (Jedwab, zu Selhausen and Moradi, 2018) and might have been attracted by precolonial institutions or early trade (Gifford and Weiskel, 1971). To control for such historical path-dependency triggered by endogenous settlements of missionaries, I include post-treatment measures of the distance to and presence of missions as recorded in 1924 (Roome, 1924; Nunn, 2010).[30]

Doing so only slightly decreases the interactive effect of indirect rule and cash crop suitability in the British sample from a value of 7 to 5.5, which remains to be sizable and statistically significant. This indicates that of missionary education only drive a relatively small portion of the reported results. Similarly, no significant changes are discernible in the results for the French sample. Consistent with the literature (e.g. Cogneau and Moradi, 2014; Lankina and Getachew, 2012), the effect of missionary stations on education in itself is substantive and significant. Finally, a specification

29 Note that this result fits well with the previous finding that indirect rule had no consistent effect on infrastructure investments in general and in fertile areas in particular (see Chapter 5).

30 Note that these data are likely incomplete, as shown by current but unpublished work by Jedwab, zu Selhausen and Moradi (2018). However, no better data source on mission stations is currently available for open access.

that combines the two alternative explanations reaffirms that they account only for a small portion of the observed effects.

6.3.6 *Long-term effects on education, wealth, and economic activity*

What have been the long-term consequences of local populations' bargaining power under indirect colonial rule on development in Africa? If state revenues from resource production increased public service provision under indirect rule, this colonial advantage should have had long-term effects on local development. As highlighted before, long-term effects of precolonial and colonial institutions have been documented by a rapidly growing literature.[31] With this last set of analyses, I add to these latest efforts by focusing on the joint impact of precolonial institutions, colonial modes of governance, and resource endowments on current levels of education, household wealth, as well as economic activity.

EDUCATION: To analyze the longevity of the combined effect of indirect rule and cash crop suitability on education, I divide the sample of DHS respondents into respondents born by the decade between 1920 and 1999. Using primary, secondary, as well as tertiary education as outcomes, I estimate the fully specified baseline model on each subsample.[32]

For former British colonies, the results in Figure 6.6 show a remarkable persistence in the difference of educational outcomes between precolonially centralized and decentralized areas with soils suitable for cash crop agriculture. Formerly British regions with centralized precolonial institutions and good soils stayed comparatively advantaged up to this day. Only advantages in primary education are withering away with time, likely due to saturation effects (see Figure 6.1b above). In contrast, the early advantages lead to increasingly large inequalities in secondary and tertiary education rates.[33] Consistent with the baseline results for the French sample, the

31 See, among others, Acemoglu, Johnson and Robinson (2001, 2002), Dell (2010), (Dell and Olken, 2018), Gennaioli and Rainer (2007), Iyer (2010), Lowes and Montero (2018), Michalopoulos and Papaioannou (2013*b*), and Roessler et al. (2018).

32 Respondents used to assess primary education rates are older than 12, older than 18 for secondary education, and older than 25 for tertiary education rates.

33 The drop in the effect on secondary education rates in the 1990s can be explained by the relatively low age threshold of 18 set by the analysis. Many African students finish secondary school only after that age.

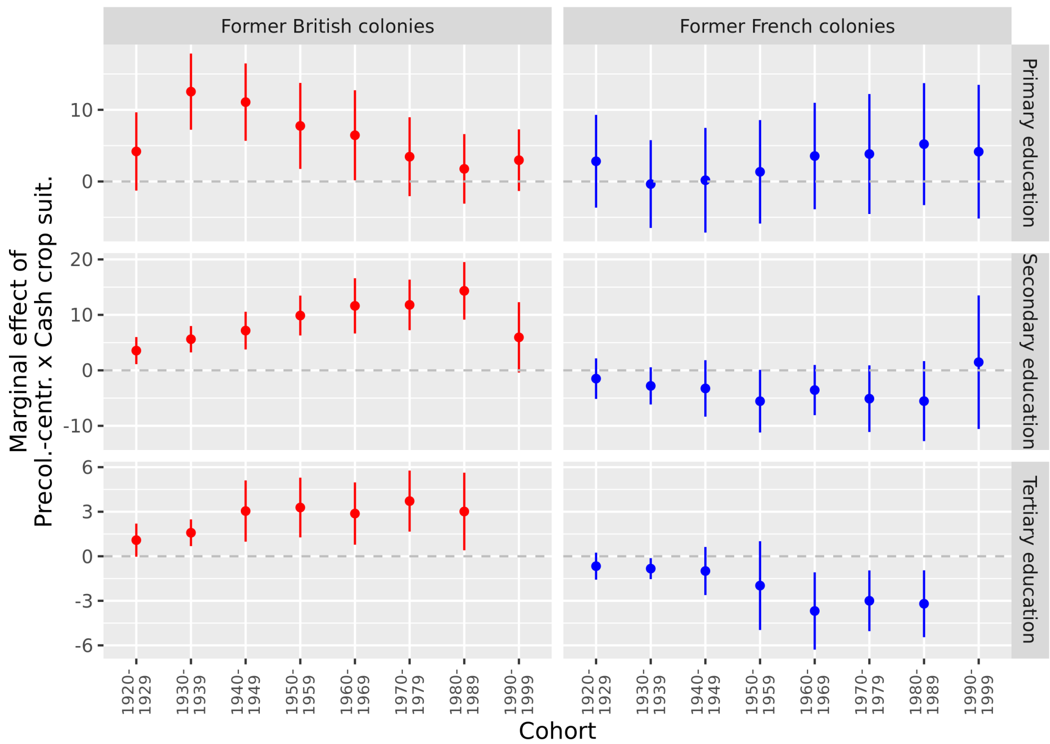


Figure 6.6: Interaction effect of cash crop suitability × precolonial centralization educational outcomes by 10-year cohorts in former British and French colonies.
Marginal effects are calculated on the basis of the full baseline specification estimated on the British and French samples split into 10-year cohorts.

estimated effect of the interaction term $\mathsf{PCC} \times \mathsf{CCS}$ are zero with large standard errors for primary education rates, and negative for secondary and tertiary education rates.

HOUSEHOLD-LEVEL WEALTH: Differences in the provision of public services, in particular education, likely translate into differences in local wealth. Using geocoded data on households from the DHS (2018), I re-estimate the baseline specifications using the DHS's wealth-index[34] as the dependent variable and the household as the unit of analysis (Table F.11).

Consistent with the previous finding of temporal persistence, the results suggest that indirect rule in the British empire led to long-lasting effects on local wealth levels. The estimated marginal effect of cash crop suitability on household wealth rises significantly in the level of precolonial centralization. Increases in the cash crop suitability by one standard deviation (0.2) in a precolonial state are associated

34 The wealth-index is a factor score of socio-economic assets held by surveyed households. It is normalized within DHS country-rounds to a mean of 0 and a standard deviation of 1.

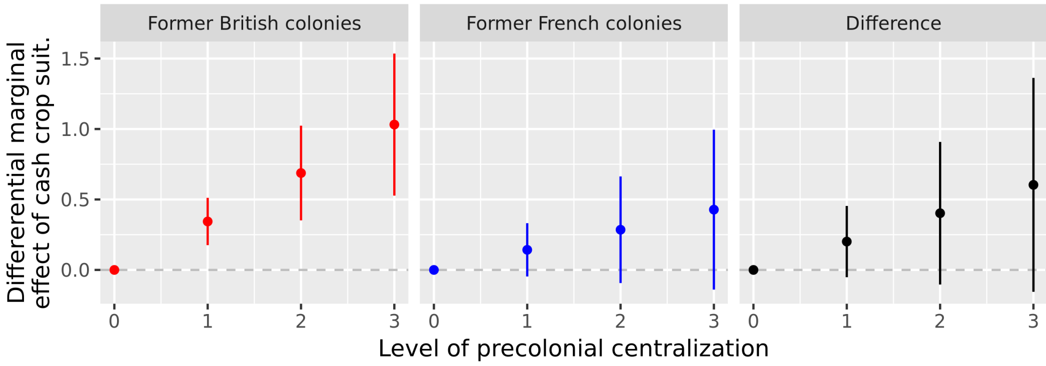


Figure 6.7: Differential marginal effect of cash crop suitability on household wealth across observed values of precolonial centralization, with a value of 0 (acephalous societies) as the baseline for comparison.
Calculated on the basis of the full specification estimated on the pooled sample in Table F.3 in the Appendix. The pooled model adds an interaction term of a dummy for British rule with each variable in Equation 6.1.

with a change in household wealth that is 0.2 [0.1, 0.3] points larger than in an acephalous society. Because the wealth index is normalized, this difference can be directly interpreted in terms of standard deviations. While the estimated effect is positive in the French sample as well, it is not statistically significant and half the size (Figure 6.7). The difference between the effects in both samples is positive but not statistically significant at conventional levels (p= .12).[35]

DISTRICT-LEVEL ECONOMIC ACTIVITY The last analysis captures the long-term effect of resource extraction under indirect rule going beyond survey measures of local development. To proxy for local economic wealth and activity, I use districts' average per-capita nightlight emissions between 1992 and 2013 (e.g. Henderson, Storeygard and Weil, 2012; Weidmann and Schutte, 2017).[36] All covariates are aggregated to the district level by taking their spatial mean. I furthermore control for the logged

35 Note that this uncertainty is driven by the inclusion of the vector of ethnic controls. In the less specified models, the interaction term PCC × CCS is negative in the French sample and its difference with the British pattern is statistically significant (see Table F.11).

36 $Y_d = ln(0.001 * mean(\mathsf{nightlights}_d^{1992,\ldots,2013})/\mathsf{pop}_d^{2000})$, where d is the district observed in 2013 (from FAO, 2014). Data on local population counts come from the GRUMP population count for the year 2000 (CIESIN et al., 2011). Data on nightlight emissions come from the National Geophysical Data Center (2014). While I am aware that the size of local populations and the degree of urbanization are post-treatment outcomes, the inclusion of these variables is warranted by the fact that I am interested in per-capita nightlights. This measure depends on local population densities due to the limited sensitivity of satellite sensors that do not detect lights at very low levels and are saturated at high levels of emissions. I use the natural logarithm of the average per capita nightlight emissions to account for different levels of population density and limit the influence of outliers (Cogneau and Dupraz, 2014). To the same intent, I drop districts with nightlight measures spoiled by gas flaring from oil fields (Lujala, Rød and Thieme, 2007).

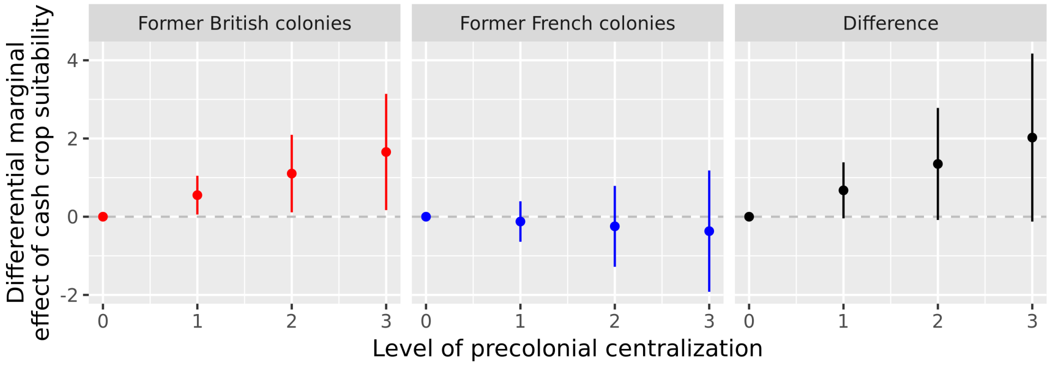


Figure 6.8: Differential marginal effect of cash crop suitability on per capita nightlight emissions (logged) across observed values of precolonial centralization, with a value of 0 (acephalous societies) as the baseline for comparison.
Calculated on the basis of the full specification estimated on the pooled sample. See Subsection F.5 in the Appendix. The pooled model adds an interaction term of a dummy for British rule with each variable in Equation 6.1.

size of the rural and urban population (CIESIN et al., 2011) as well as the area of each district.

The results indicate a continuous developmental effect of cash crop production in areas under indirect British rule (Figure 6.8). The model associates an increase of local soils' suitability by one standard deviation (0.17) in a precolonial state with a change in local nightlights per capita that is 33 [3.1, 72] percentage points larger than in an acephalous society. The difference in the marginal effects of cash crop suitability on nightlight emissions between the two types of precolonial societies is close to zero in former French colonies. The effects in the former British Empire are primarily driven by nightlight emissions from rural areas (see Table F.12). This suggests that most persistence stems from rural areas suitable for cash crop production rather than from higher levels of urban development.

6.4 CONCLUSION

The distribution of bargaining power between central governments, local rulers, and the people affects the terms of the trade of taxes for public services. During the colonial period in Africa, colonizers provided public services in return for state revenue raised from natural resources produced by rural populations. Building on the insights from the previous chapters on state building in colonial Africa, this chapter has argued that the bargaining power left in the hands of local elites and populations under indirect colonial rule increased their returns on taxation.

The results I find support this argument. As I have shown in Chapter 4, centralized precolonial institutions provided the basis for indirect rule in British colonies. Such indirect rule increases the marginal effect of the local suitability for cash crop agriculture, used as an exogenous proxy for real production, on colonial education rates. This result suggests that indirectly ruled areas received higher returns for revenue extraction than areas under more direct control of the British colonial governments. In French colonies, where direct rule crushed the bargaining power of centralized polities, this relationship is absent and partly reversed. These patterns remain stable when I account for the potentially endogenous spatial sorting of ethnic groups by comparing individuals from neighboring ethnic groups of different levels of centralization. With greater statistical uncertainty, the point estimates of specifications that compare individuals within ethnic groups split by a French-British border also bolster the baseline results.

The diverging effects of cash crop suitability under direct and indirect colonial rule have persistent impacts until today. In former British colonies, contemporary education rates, household wealth, and per capita nightlight emissions all increase to a greater extent with soils' quality in precolonially centralized ethnic groups than in non-centralized, acephalous societies. Consistent with the patterns of local colonial education rates, this is not the case in former French colonies. Taken together, these results not only strengthen the theoretical argument. They also emphasize the value of decompressing history and studying the interactions between precolonial and colonial factors that shaped the pathways on which local societies developed.

Moving beyond the impact of the distribution of bargaining powers for local development, the next two chapters examine how transaction costs, in particular those due to transport costs, affect interactions between states and their subject. Chapter 7 studies the effects of lowering state-society transaction costs on local development, and Chapter 8 analyzes their impact on local conflict between the state and its challengers.

7

STATE REACH AND DEVELOPMENT IN AFRICA 1965–2015

Adverse population distributions and suboptimally drawn national borders in Africa limit states' reach, increase the costs of transactions between states and their citizens, and thereby curb economic development. Not least since Herbst's seminal work on *States and Power in Africa*, this argument is invoked to explain misgovernance and low levels of development on the continent. However, its empirical underpinnings lag behind its practical importance. In particular, there is currently no continent-wide, geographically disaggregated, and time-varying measure of state reach in Africa. This has impeded the estimation of the effect of changes in state reach on local development. This chapter addresses this gap and builds on the argument that transaction costs between governments and their people are a crucial determinant of the trade of tax revenue for government services that foster local development.

Empirically, I follow the observation that physical distance impedes interactions between the state and society and proxy transaction costs through travel times towards states' administrative headquarters. The empirical results show strong cross-sectional correlations between low costs of state-society transactions and better developmental outcomes. Exploiting local temporal changes in travel times to national and regional capitals between independence and today, I document varying, but in sum positive effects of reductions in transaction costs on local education and infant survival rates, as well as nightlight emissions.

With this focus, the chapter fits squarely into the growing literature on the consequences of state capacity in Africa (e.g. Herbst, 2000). Most importantly, a number of studies demonstrate that historically accumulated state capacity positively affects local development (e.g. Gennaioli and Rainer, 2007; Michalopoulos and Papaioannou, 2013*b*; Dell, Lane and Querubin, 2018; Pierskalla, Schultz and Wibbels, 2017). However, evidence on the postcolonial evolution of local state capacity in Africa and its impacts on development remains incomplete.[1] Beyond Herbst's country-level statistics of road densities and aggregate data on tax ratios and administrative capacity (e.g. Hendrix, 2010), no consistent, time-varying, and spatially disaggregated measure of local state reach since the colonial era exists for the continent. The best evidence so far thus comes from cross-sectional studies (e.g. Henn, 2018) and research on the effects of administrative unit proliferation (Gottlieb et al., 2019; Grossman, Pierskalla and Dean, 2017), which is only one way to extend state reach. That said, the overall impact of changes in state reach on local development has remained empirically unexplored. Addressing this gap in the literature is the main contribution of this chapter.

To overcome the scarcity of spatio-temporal data on state reach in Africa, I draw on the data on national and regional administrative geographies and African road networks since independence introduced in Chapter 5. These data allow me to estimate, for every year, the travel time from each cell of a 5×5 kilometer grid of the continent to its regional and national capitals. These travel times constitute my proxy for state-society transaction costs and, by implication, local state reach. On average, changes in administrative geographies, road building, and urbanization have decreased travel times between national (regional) capitals and their citizens by about 20 (30) percent between 1966 and 2015. In line with the argument that the breadth of state-society interactions decreases in their costs, a brief cross-sectional analysis shows that citizens closer to administrative capitals more frequently pay taxes and receive government-provided security, public services, and jobs. These cross-sectional patterns repeat themselves in measures of local development, be they education and infant mortality rates, households' wealth, or per capita nightlight emissions.

To address problems of reverse causality and omitted variable bias, my main empirical strategy exploits local temporal variation in the travel times to national and regional capitals. To measure local development outcomes over time, I construct geocoded pseudo-panels of individual-level education rates and infants' mortality from the Demographic and Health Surveys (2018). I complement these with data on remote-sensed nightlight emissions between 1992 and 2013. Holding constant all variation between points and country-years, the analysis finds that getting closer to a national capital is associated with higher education and infant survival rates as well as, albeit associated with large standard errors, more light at night. Decreases in the

1 This is different for other continents, see for example Acemoglu, García-Jimeno and Robinson (2015) on the developmental effect of local state capacity and its network externalities in Colombia.

distance to regional capitals come with higher education rates and brighter nightlight emissions, but not lower infant mortality rates. These effects are robust to a series of permutations to the baseline model that aim to test whether the results are driven by non-parallel trends, migration, changes in access to economic markets, ethno-regional politics, or endogenous road-building and capital placements.

These results support the theoretical expectation that high costs of state-society transactions limit the developmental benefits of centralized governance. Exchanges between governments and citizens become more frequent as states improve their reach and make their populations accessible by redrawing the administrative map and building transport infrastructure. This leaves the population with substantive, but not universal, gains from increased trade of taxes for public services.

7.1 THE COSTS OF STATE-SOCIETY TRANSACTIONS AND DEVELOPMENT

7.1.1 *Restating the argument*

States trade with the societies they govern. They exchange goods and services at the production of which they have a comparative advantage for resources better and cheaper produced by the people. Thus, state governments provide security services, enforce legal institutions, and produce public goods and services in exchange for taxing the economic surplus generated by their citizens.

Because comparative production advantages set in motion the exchange between the state and its citizens, gains from trade produce profits for both. Through taxation, the state gains access to economic resources. In exchange, citizens receive security, law enforcement, and public services, at a higher quality and quantity than they could produce on their own (Bates, Greif and Singh, 2002; Levi, 1988; North, 1981). In the last chapter, we have seen that the bargaining power of citizens affects how the gains of trade are distributed between them and the state. Beyond this factor, the costs of trade determine the extent to which citizens and governments will exchange resources. At low transaction costs, trade between the government and its citizens will be extensive. This logic, I have argued in Chapter 5, is the main reason for states' biased investment in transport networks that make natural resources accessible and exploitable. As transaction costs rise, an ever-larger share of all possible exchanges becomes unprofitable for either the state or its citizens. Thus, the extent of state-society transactions and the gains from trade decrease.

As highlighted in Chapter 3, natural and administrative geography can be an important impediment to state-society transactions. For the state, its physical distance from the people determines its ability to observe citizens' actions, collect revenue, implement policies, and punish non-compliance. With their power degrading over distance,[2] state officials will have a hard time making far away populations comply with their part of the bargain. Just as states' power to enforce decreases with distance, their power to deliver security, justice, and public services does as well. Citizens far away from the state have therefore less to expect from the state. Lacking direct contact with the state, they are also less able to pressure the state to fulfil any promises once made. In sum, these arguments suggest that transactions between governments and their people become less frequent the farther they are apart.

As in exchanges on regular economic markets (e.g. Donaldson, 2015, 2018), transportation and communication costs lie at the root of transaction costs that increase with physical distance. While modern communication technologies have heavily contributed to bringing about the 'death of distance' (Cairncross, 1997), they have not dispensed with the need of transport in government-citizen transactions.[3] Many of the goods exchanged between governments and citizens are physical, ranging from the tax paid in cash to the road or hospital built. Most government services also entail the physical presence of its agents, be they teachers, judges, or police officers, who will be more compliant if their superiors oversee them in person (e.g. Olken, 2007).

There are four main channels through which lower transport costs between states' headquarters and the citizens they govern and, by implication, higher levels of state reach, can affect local development. The first channel consists in improvements of states' ability to collect taxes. This has, in and of itself, a negative effect on local development. These negative effects are however offset by the remaining three mechanisms that increase local development.

In this regard, the first channel consists in the ease with which states can enforce the 'rules of the game', its national institutions and policies. The enforcement of

2 In the conflict and counter-insurgency literature (e.g. Buhaug, 2010; Tollefsen and Buhaug, 2015), the 'loss of strength gradient' has been an influential concept ever since it was introduced by Boulding (1962).

3 Note that with the rapid development of digital information, communication, and surveillance technologies, the importance of physical access will further decrease but not vanish.

institutions – in particular of secure property rights – is a necessary condition for them to have the developmental effects stressed by the institutionalist literature (Acemoglu, Johnson and Robinson, 2002, 2001; North, 1981). Where the state is unable to enforce its rules for a lack of watching bureaucrats and patrolling police officers, the institutional void will likely be filled by non-state actors. This can give rise to local competition for power and possibly inconsistent institutions that curb development (Migdal, 1988).

The second channel through which lower transaction costs improve development runs through the provision of local public services, in particular schools, clinics, and public water, sanitation, and electricity infrastructure (Henn, 2018; Krishna and Schober, 2014). As argued above, providing these services becomes more expensive in inaccessible areas as material and agents have to be transported and supervised. Moreover, specialized services that are provided only in bigger towns, often administrative capitals, are more accessible to citizens that live nearby.

A third channel by which state reach increases citizens' welfare consists of private rents provided by the state. Citizens close to administrative headquarters typically have better access to rents from, for example, public sector jobs, direct subsidies, or personal handouts from state officials. Shorter distances between the state and its people will make interactions more frequent, allowing citizens' to know about opportunities for rent-seeking[4] and enabling government officials to observe and potentially enforce the behavior expected in return. This logic of greater accessibility of private rents, so Ades and Glaeser (1995), motivates migration towards capital cities. Combined with the argument that the trade between states and their citizens produces net benefits for both sides, these arguments lead the way to the main hypothesis of this chapter:

H 7.1: *Local development decreases with higher costs of physical interaction between the population and the state administration.*

7.1.2 *Transaction costs and development in Africa*

The argument that high transaction costs between states and their citizens impede local state rule and development resonates well with the literature on African development.

4 See, for example, Banerjee et al. (2018) who show that giving citizens information about their eligibility to rice subsidies improves their access to them.

It has, however, not been put to a comprehensive empirical test. The following brief review of the literature highlights that current evidence is limited by the endogeneity of administrative geographies and imprecise measurements that neglect the importance of transport infrastructure for reducing transaction costs. Following up on this discussion, I present new data and empirical analyses that address both points.

Most prominent in the literature is Herbst's (2000) reasoning about the long-term effects of African states' geographies. Already before colonization, there were few places on the continent fertile and rich enough to sustain dense populations and allow centralized governance to persist. The problem of governing spread-out populations got only worse after the colonial conquest and establishment of new, fixed rather than fluid, borders. As Herbst (2000) notes, "[in] the precolonial era, population distributions yielded boundaries. In the modern era, boundaries define a people." Many of the new states encompassed what Herbst calls 'difficult geographies,' in which several population clusters are separated by large, unpopulated areas.[5] In these countries, state-society interactions are too expensive to sustain uniform centralized governance.

As I have argued in Chapter 5, states can and do mitigate their initial geographic disadvantages where it pays off, for example by designing their administrative geographies and building infrastructure to improve the links between headquarters and subjects worth taxing (see also Hechter, 1975; Mann, 1984; Tilly, 1990; Weber, 1977). However, Herbst's data on roads show that the varying success of African states to do so only reinforced their geographic disadvantages. Those states with the worst geographies inherited the fewest colonial roads per square mile and invested the least to extend them until 1990. Notwithstanding its innovation at the time, Herbst's analysis of road densities is limited for three reasons. First, road densities fall short of capturing states' costs of interacting with their population. To do so, it is necessary to assess road networks in conjunction with the spatial population distribution. Second, simple country-level measures of state reach mask the variation within countries which is at the heart of Herbst's argument (see also Boone, 2003; Pierskalla, Schultz and Wibbels, 2017).[6] Third and lastly, Herbst's aggregate road data covers only the years

5 Examples include the DR Congo, Mozambique, Nigeria, Somalia, and Senegal.

6 As a remedy, Wig and Tollefsen (2016) draw on data on local state institutions from Afrobarometer data. These surveys do however not cover countries in their entirety and are collected in a sample of comparatively well-governed, capable states. Lee and Zhang (2017) examine age-heaping in local census data and attribute the phenomenon to the inability of states to make their population legible.

1963 and 1997. Without richer spatio-temporal data, we therefore do not know when and where African states extended their reach.

Following up on Herbst's work, a growing literature shows that the distance from administrative centers of the state curbs local development by decreasing the quality of governance and fostering conflict. There is substantial agreement that locations farther away from their capital experience higher levels of conflict (Buhaug and Rød, 2006; Cederman, Buhaug and Rød, 2009; Raleigh and Hegre, 2009; Tollefsen and Buhaug, 2015) and suffer from misgovernance and corruption (Campante and Do, 2014; Campante, 2017; Krishna and Schober, 2014). As a consequence, they exhibit lower levels of development, in particular where local traditional authorities do not substitute for the absence of the state (Henn, 2018). Pierskalla, Schultz and Wibbels (2017) suggest that these effects are persistent by showing that a location's distance to its historical capitals is correlated with its current level of development.

One way to shorten the distance between the state and its people consists in increasing the number of administrative units. In recent years, this strategy has been used extensively by African governments. Modifying the geographic board of the political game, governments can create opportunities for patronage (Green, 2010; Hassan, 2016; Gottlieb et al., 2019), reduce the power of local authorities (Grossman and Lewis, 2014), and harm the opposition (Resnick, 2017). Going beyond the political effects, Grossman, Pierskalla and Dean (2017) analyze the impact of administrative unit proliferation on public goods provision and report a generally positive effect that levels off in very fragmented countries. Their corresponding micro-level analysis of child and infant mortality rates in Malawi, Nigeria, and Uganda produces more mixed results. This suggests that the effects of administrative unit creation may vary across public services as well as geographic and political contexts.[7]

While greatly contributing to our knowledge about the developmental effects of administrative geographies in Africa, current evidence on the effect of costs of state-society transactions on local development are limited for three reasons.

First, while the association of the gradient of state power with good governance and development is generally well documented in cross-sectional studies, most of them do not account for the potential endogeneity of states' geography. As argued by the

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Age-heaping is, however, also related to low education rates (A'Hearn, Baten and Crayen, 2009), which are an outcome of rather than an input to local level state capacity.

7 See also Gottlieb et al. (2019) for an analysis of public spending and splits of communes in Senegal.

literature of administrative unit proliferation and highlighted in the first empirical part of this thesis, the location of capitals and borders are choices made by governments. These respond to geographic and socio-economic factors that may also have a direct effect on local wealth and well-being. In the most robust study, Campante and Do (2014) use distances from the geographical centers of US states as an instrument for a locations' distance from their capitals. While certainly an improvement, this approach might still be biased if the location of capitals determine where borders are drawn.[8] More recently, Henn (2018) exploits variation in the distance to subnational capitals in Africa across the boundaries of their units. However, this regression discontinuity design still relies on the assumption that subnational boundaries are drawn as if random, at least within a small geographic area.

Second, while panel analyses of the effects of administrative unit reforms could offer some remedy to the identification issues of cross-sectional studies, indicators of changes in administrative designs mask heterogeneous effects of administrative unit splits and mergers on local transaction costs. Most importantly, the creation of a new unit leads to decreases in transaction costs mostly around the new capital, but not necessarily in the periphery of the new unit. The solution followed here consists in assessing the effects of changes in distances to capitals over time.

Besides these identification issues, all studies use mere geodesic distances as a proxy for state reach. With that, they treat geographic space as essentially flat and fall short of capturing the fact that distances are distorted by transport infrastructure that might additionally change over time (see Chapter 5 and Herbst, 2000). This is not merely a problem of measurement error, which, if unbiased and small enough, poses no caveat to the analyses. Rather, empirical models that proxy state reach through geographically predetermined variables can only offer limited advice to policy makers who want to improve state reach and foster development.

The empirical test of the argument that local development decreases in the cost of state-society transactions responds to these challenges. After a mostly descriptive cross-sectional analysis, I improve upon previous identification strategies by exploiting temporal variation of locations' distance to their changing regional and national capi-

8 In their instrumental variable setup, the exclusion restriction assumes that there is no factor that affects the instrument (border-related state centroids) and the observed capitals at the same time. If however capitals are causally antecedent to borders, this assumption does not hold.

tals since independence. To realistically measure the costs of state society-transactions, all distances are measured as travel times on time-variant road networks.

7.2 LOCAL STATE REACH IN AFRICA: MEASUREMENT AND DESCRIPTIVE STATISTICS

7.2.1 *Measuring state-society transaction costs*

Because transport infrastructure is an important determinant of the transaction costs states' face, I follow the procedure laid out in Chapter 5 and proxy state-society transaction costs through the travel time between citizens and their capitals at the various levels of administrative hierarchy:

$$\mathsf{TAC}_{put} = ln(1 + \mathsf{travel\ time}_{put}) = ln(1 + d_t(p, C_{put})) \tag{7.1}$$

where transaction costs (TAC) between the state on a level of hierarchy u and a citizen who lives in point p at time t is calculated as the shortest travel time (in hours) between p and its unit's capital C_{ut} on the road network at time t. I assume that a capital's reach is decreasing convexly as distances increase and therefore log-transform all travel times.[9] Travel times correlate with but are not the same as geodesic distances. As argued before, state agents most often rely on earth-bound vehicles to reach citizens, making the road measure more precise.

DATA ON ADMINISTRATIVE GEOGRAPHIES: Data on national borders and capitals comes from the Cshapes data set (Weidmann and Gleditsch, 2010). To trace subnational administrative geographies, I collect the first comprehensive data set on the boundaries and capitals of first-level administrative since countries' independence. To do so, I rely on the qualitative account of unit changes from `statoids.com` and encode each administrative unit change geographically. The underlying geographic data comes from the GAUL database (FAO, 2014),[10] more than 100 digitized maps, mostly from the CIA Base Map series as well as other GIS data, such as the GADM database.

9 I add a constant of 1 hour to avoid missing values in capitals, where travel times amount to zero hours.

10 All unit-*splits* – this type of unit-change constitutes the vast majority of cases – that happened over time can be geographically encoded by simply merging the units observed after the split. This results in the original spatial unit.

Capitals are geocoded via the geonames.org gazeteer. The final data set, described in greater detail in Appendix C.2, covers 1763 unique region-periods, covering each African country from independence to 2016.

DATA ON ROAD NETWORKS: The second ingredient to Equation 7.1 consists in the time-varying data set on African road networks digitized from the Michelin map corpus collected at 23 points in time between 1966 and 2014 (see Chapter 5 and Appendix B.2). As before, I transform the digitized road maps into a planar, georeferenced graph that covers the entire continent in a continuous manner at a resolution of about 5km at the equator. Each edge on the network is associated with a travel time that results from the product of its length and the attainable speed on the respective road type. Footpaths are associated with a speed of 6km/h.[11]

With the data on roads and administrative units, I calculate the travel time to regional and national capitals for each centroid of a raster with a resolution of 5km for every year between each state's independence and today. Visualized in Figure 7.1, the result is a set of yearly grids that depict states' reach towards each grid cell.

Figure 7.1 highlights three important advantages of this measurement of state reach. First, the Michelin corpus allows us to measure travel times in a consistent manner across time and space, both within and across countries. This produces unique data on state reach in Africa over a long time span. Second, travel times are measurable with a high level of precision if compared to other variables on which most often no complete, consistent, and spatially disaggregated data exists, and even less so over time. Third and important for the empirical analysis, local changes in roads, capitals, and borders induce spatial spillover effects in state reach that are credibly exogenous to temporal variation within one point.

Notwithstanding the advantages of the road- and capital-based measure of transaction costs, the approach comes with limitations. First, the measure is based on road maps of a resolution of 1:4 million. The coarse resolution leads the measure to be noisy at the very local level. In addition, the digitization procedure leads to small (≈0-5km) differences in the location of roads observed in various versions of the Michelin maps.[12] Both factors lead to small (and random) measurement errors in

11 Note that I assume that improvements in vehicles' speed are negligible and do not favor one type of road over another.

12 Unfortunately, there is at the moment no automatic and robust algorithm to align spatial lines with each other without introducing inconsistencies into the networks.

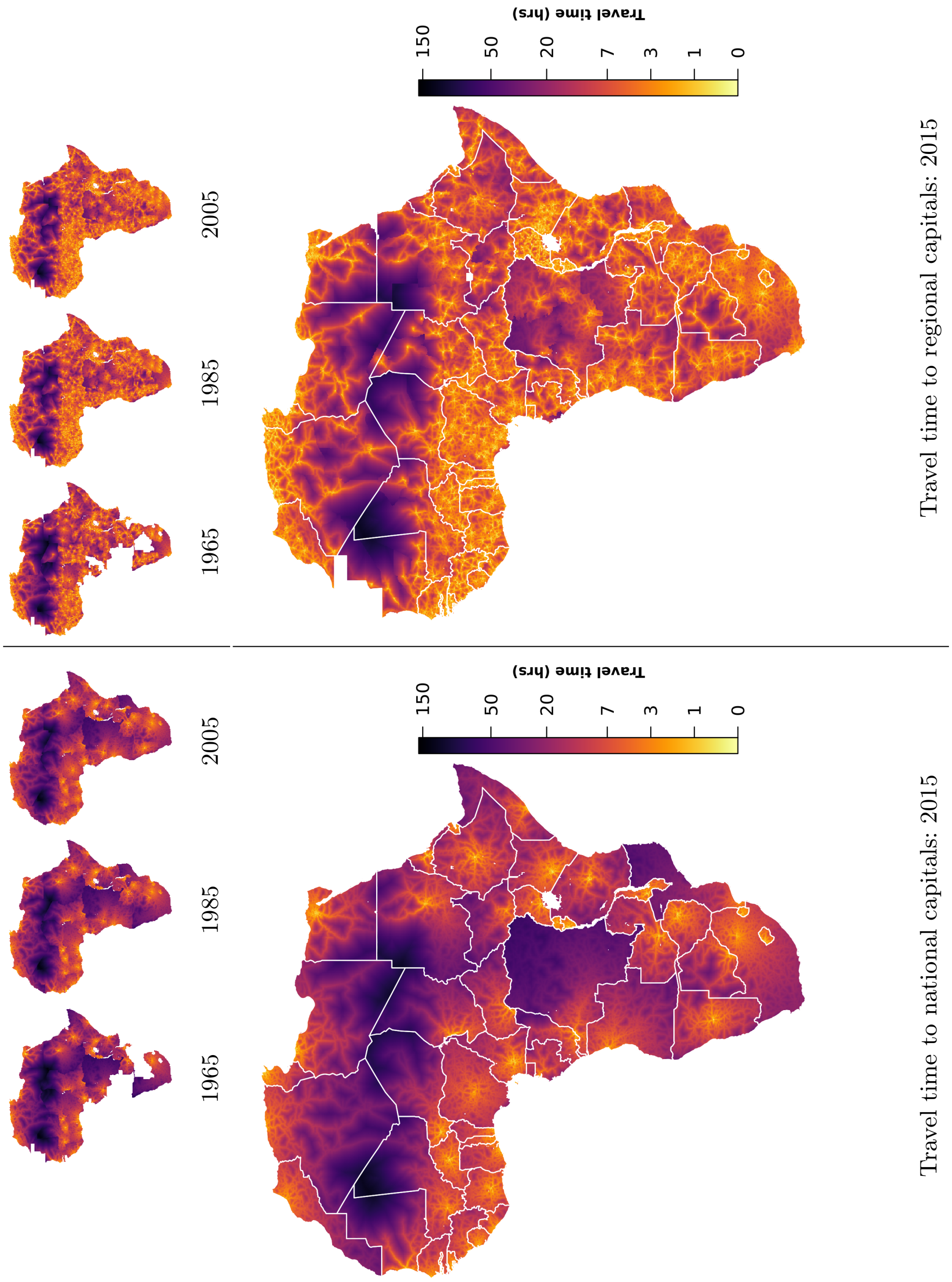


Figure 7.1: State reach in Africa, 1965–2015.

travel times. Second, I assume that all regional headquarters have the same amount of administrative resources at their disposal. In principle, Equation 7.1 could be extended with the resources (number and quality of personnel, technical equipment, etc.) of each capital. However, in particular on the regional level, no such data on the capacity of headquarters exists.[13] Third, I neglect determinants of transaction costs other than transport costs, in particular cultural and linguistic distances between states and their citizens.

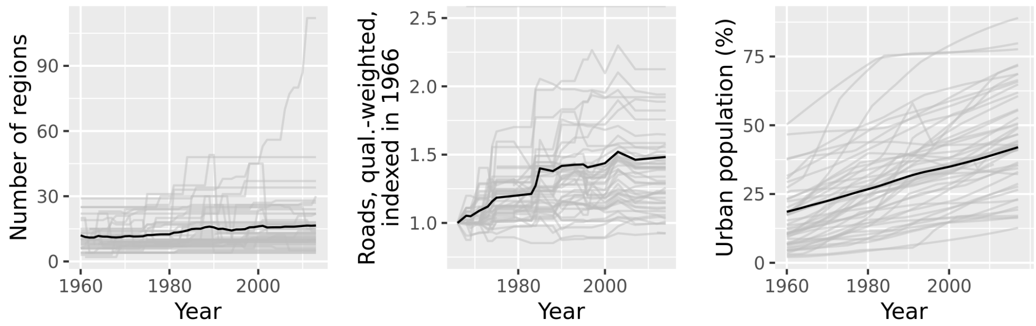
7.2.2 *State reach in Africa: weak, growing, and unequal*

Three main factors influence the difficulties of a state to reach out to its population: the location of administrative borders and capitals, and the structure of the transportation network that links the state to its subjects, and the geographic distribution of its population. By changing their geography along each of these three dimensions, states can increase their reach and lower transaction costs. First, states can optimize the location of its headquarters, open new branches of state agencies, and shift the boundaries of administrative units (Fesler, 1949). Since independence, the Côte d'Ivoire, Nigeria, and Tanzania have relocated their national capitals[14] and most have increased the number of administrative units (Figure 7.2a, see also Grossman and Lewis, 2014; Grossman, Pierskalla and Dean, 2017).[15] Similarly, the independence of Eritrea and South Sudan has reduced distances to their capitals.[16]

Second, states can improve transport networks to access certain areas. Although the main backbones of African road networks are of colonial origin, the networks' extent has increased by about 50 percent since independence (Figure 7.2b). While this figure is not necessarily impressive (Herbst, 2000), it has shortened the distance between states and their citizens.

Lastly, states can incentivize their population to concentrate and urbanize and thus increase governments' economies of scale of reaching out to a particular populated place (e.g. Scott, 2017). While in 1960 only about 20% of Africans have lived in

13 Cross-country differences are later accounted for by country fixed effects.
14 Côte d'Ivoire (Abidjan to Yamoussoukro in 1983), Nigeria (Lagos to Abuja in 1991), and Tanzania (Dar es Salaam to Dodoma in 1974). The change in Tanzania was less de facto than de jure. Until today, all ministries are located in Dar es Salaam.
15 Note the case of Uganda being the outlier with the steepest increase in Figure 7.2a.
16 Their independence of course also affected other dimensions of the distance between the state and citizens, in particular the ethnic distance.



(a) Number of administrative regions

(b) Quality-weighted road mileage in Africa

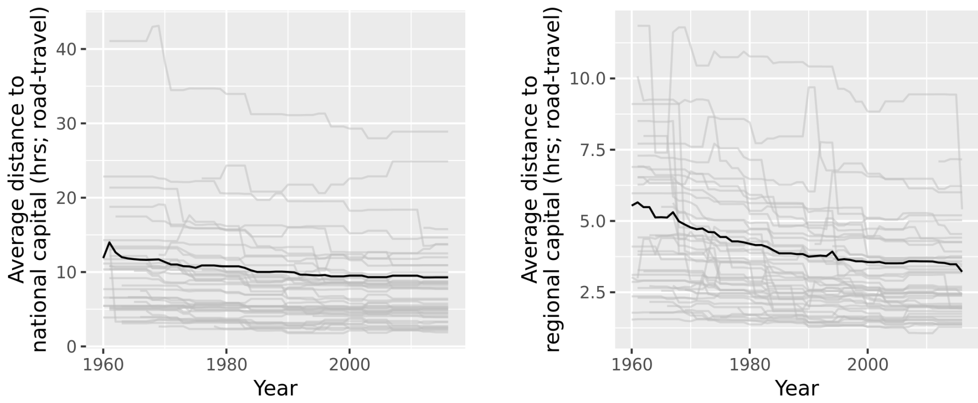
(c) Urban population

Figure 7.2: Drivers of expanding state reach in Africa 1960–2015.
Sources: (a) own data (see above); (b) Michelin road map corpus; (c) World Development Indicators (World Bank, 2018).

cities, the proportion of urban residents in 2016 has risen above 40% (Figure 7.2c). Among other social changes brought about by this development, rural-urban migrants experience a steep decrease in transaction costs with the state since administrations and state institutions are typically based in cities. Equivalent state-led population concentration also occurred in the countryside. In particular villagization programs, such as the resettlement of millions of Tanzanians into so-called 'Ujamaa-Villages' in the 1970s (e.g. Miguel, 2004), have made rural populations accessible to the state.

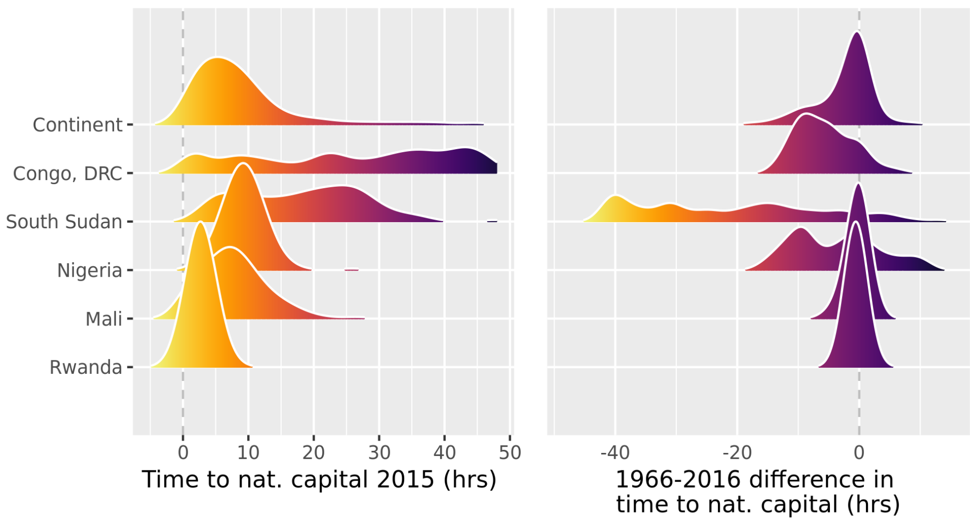
Together, administrative unit changes, road building, and population concentration since the 1960s have extended states' reach and lowered their transaction costs with their citizens. As seen in Figure 7.3, the average travel time between African national capitals and citizens has decreased from 11.7 hours in 1965 to 9.3 hours in 2015, a change of about 20.8 percent. Moving to the level of regional administrations where changes in the design of units are more common, we observe a steeper trend. While citizens in 1965 had an average travel time of 5.1 hours to their regional capital, they had to travel 'only' 3.5 hours in 2015 – a decrease of 32.2 percent.

As expected from the large diversity of countries and their geographies, these continent-wide aggregates mask substantial heterogeneity across and within states. Figure 7.4 visualizes this variation and plots population-weighted densities of travel times to national capitals for five countries in 2015 and their change since 1965. From a cross-sectional perspective, Subfigure 7.4a shows how states differ in their reach towards their population. Capitals of countries with difficult geographies and poor infrastructure such as the DR Congo are farthest away from their median inhabitant



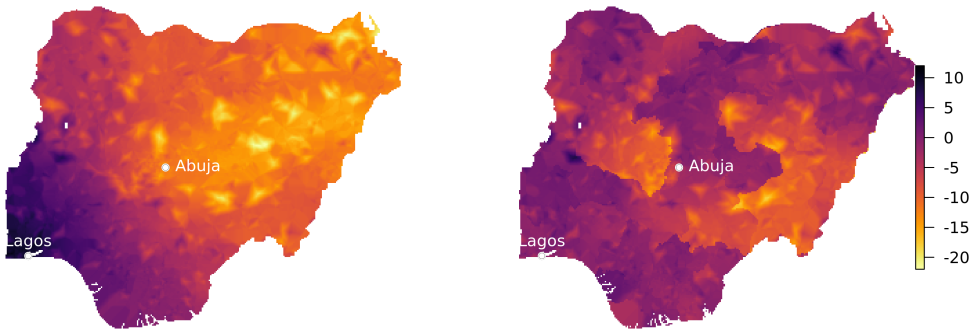
(a) Average time to national capital (b) Average time to regional capital

Figure 7.3: Decreasing travel times to regional and national capitals in Africa 1960–2015. All averages are population weighted. Sources: Own calculations based on Hyde population estimates (Goldewijk, Beusen and Janssen, 2010), Michelin-based road networks, Cshapes (Weidmann and Gleditsch, 2010), and own data on administrative regions and their capitals (see above).



(a) Travel time to national capital 2015 (b) Change in travel time to national capital 1965–2015

Figure 7.4: Population-weighted distribution of state reach across the continent and in five selected countries in 2015 and its change since 1965.
Note: Brighter colors identify better outcomes. To calculate population-weighted distributions for single countries, I fix the spatial distribution of the population and the borders of countries to their status in 2015. Sources: Own calculations based on 2015 WorldPop population estimates, Michelin-based road networks, and country-borders from 2015 from Cshapes (Weidmann and Gleditsch, 2010). For the full set of African countries, see Figure G.2 in the Appendix.



(a) Change in travel time to the national capital

(b) ... to regional capitals

Figure 7.5: Change of travel times to capitals in Nigeria 1965–2015.

(34.3 hours). In the mid-range, we find Mali where one travels 7.2 hours from Bamako to the median citizen. Lastly, capitals of small countries such as Rwanda naturally are closest to their median citizen (2.6 hours). Similar variation marks changes in the accessibility of the population since 1965 (Subfigure 7.4b). Here, the populations of states that seceded (Eritrea, Namibia, and South Sudan) and of those that relocated their capital (Nigeria, Côte d'Ivoire, Tanzania) profited the most. Other states, such as the DR Congo, significantly improved their reach in absolute term. However, in relative terms, these improvements look less impressive.

Even more striking than the variation across countries is the variation observed within countries. The density plots in Subfigure 7.4a visualize high levels of inequality in state reach in some countries. In particular states that Herbst (2000) associates with 'difficult geographies'[17] exhibit large variation in travel times – to the point where the distribution of travel times in the DR Congo is heavier in its right than left tail. Similarly, new roads, borders, and capitals do not have a geographically uniform effect. For example, relocating the Nigerian capital from Lagos to Abuja in 1991 increased state reach towards the Northern areas of the country, while decreasing it around Lagos in the South-West of the country (see Figure 7.5).

17 Angola, DR Congo, Ethiopia, Mozambique, Namibia, Nigeria, Senegal, Somalia, Sudan (borders of 2000), Tanzania. Cf. Herbst (2000, p. 161).

7.3 DATA ON LOCAL DEVELOPMENT

The brief description of state reach in Africa over the past 50 years has highlighted substantial variation within and across countries as well as over time. How does this variation affect development? To evaluate this question, the empirical analysis of this chapter uses survey data on local education, infant mortality, and household wealth, as well as remotely sensed data on nightlight emissions. The following section introduces the main outcome data. I then turn to a brief cross-sectional analysis to finally focus on the main analysis that assesses the developmental effects associated with changes in travel times towards national and regional capitals within the same point in space.

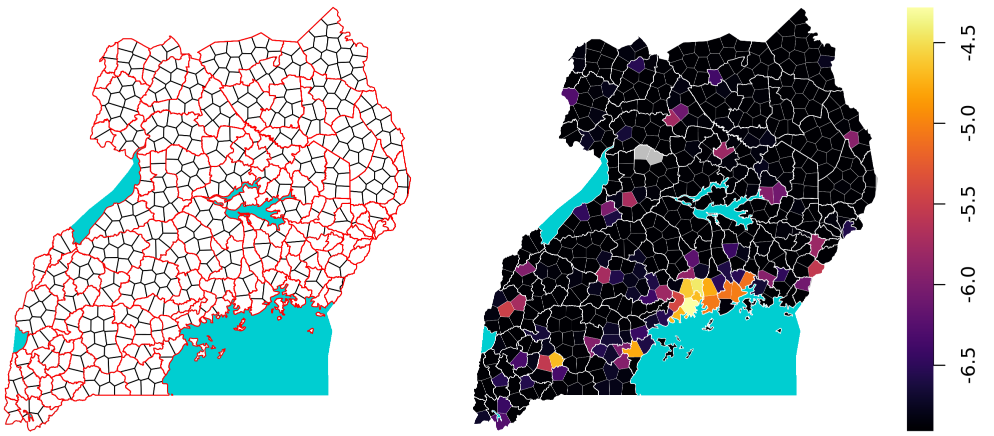
Table 7.1: Data on local developmental outcomes

Outcome	Unit of Analysis	Time-span	Local time-variance
Primary education	Individual at age 6	Indep.–2008	yes
Infant mortality	Individual at birth	Indep.–2016	yes
Household wealth	Household at interview	1997–2016	no
Nightlights per capita	Voronoi cell	1992–2013	yes

Table 7.1 provides an overview over the four different measures of local development that I use in the analysis, three of which allow me to capture changes in local development over time.[18] To measure the first three outcomes, primary education, infant mortality, and household wealth, I rely on data from the Demographic and Health Survey (DHS, 2018). As already presented in the previous Chapter 6, the DHS is the largest, individual-level, and geocoded survey conducted in African countries. As before, I make use of the information on the educational achievement of the members of households sampled by the DHS. I assume that these individuals were interviewed where they were raised and model the level of education of 1.9 million DHS respondents older than 15 as having been influenced by the travel time towards their capitals at age 6. A dummy of whether an individual completed primary school or not serves as the main outcome.

The DHS also asks women aged 15–49 about the complete history of the children they have given birth to, in particular about their survival up to age 1. Using this information on 2.5 million infants, I model the death of an infant in its first year of

18 For maps and summary statistics on the outcomes, see Appendix G.1.



(a) Regions and Voronoi cells (b) Logged nightlights per capita, avg. 1992–2013

Figure 7.6: Voronoi cells, regions, and nightlights in Uganda 1992–2013.
Note: Panel (a) plots all regional borders between 1992 and 2013 in red and Voronoi polygons in black. Grey Voronoi cells in Panel (b) are unpopulated and therefore dropped from the analysis.

life as depending on the travel time to the mother's regional and national capital at the time of the infant's birth. Infant mortality as well as education rates vary locally over time because children and household members sampled in the same place are born and schooled at different points in time.

As in the previous chapter, I assume that individuals' decisions to migrate do not respond to changes in state reach in a manner that correlates with the observed outcomes. To mitigate the caveat of biased migration into and out of the treatment, the analysis includes robustness checks that investigate whether effects differ between migrants and non-migrants.

As a third and more direct measure of local development, I also draw on the DHS's wealth index that combines information on households' material assets. Unfortunately, the geographic density of DHS samples is not high enough to exhibit meaningful variation within the same location over successive rounds of the survey. I therefore only use the household-level data on wealth in the more descriptive cross-sectional analysis.

To mitigate the uneven covrage of the DHS data and counter concerns about migration biases, the fourth measure of local development consists in local nightlight emissions per capita. As noted in Chapter 6, nightlight emissions are a robust indicator of economic development (e.g. Henderson, Storeygard and Weil, 2012; Weidmann and

Schutte, 2017). The data comes from satellite images made available by the National Geophysical Data Center (2014) for the years 1992–2013[19] at a resolution of .0083 decimal degrees (≈1km). As in Chapter 5, I choose Voronoi cells as the unit of analysis. The cells are constructed with an average size of 400km^2 and are nested within all regions observed over the sample period (see Figure 7.6).[20] In contrast to regular quadratic grid cells that frequently overlap with (changing) administrative borders, the Voronoi cells allow me to precisely assess the impact of local changes in travel times to regional capitals. Nightlights are aggregated up to Voronoi cells, divided by the population count in the cell (from CIESIN et al., 2011), and log-transformed after adding a constant of .001.[21]

7.4 CROSS-SECTIONAL ANALYSIS

Before delving into the main analysis that studies the effects of changes in local state reach on local development, this section documents the cross-sectional correlates of travel times to capitals in Africa. To assess the argument that state-society transactions decrease with transport costs between the state and its citizens, I first present correlations between travel times and state-society transactions enumerated by the Afrobarometer (2018). I then turn to cross-sectional associations between local state reach and development. I intend these results to be illustrative only, with no claim at causal identification. As argued before, cross-sectional analyses of the impact of state reach are prone to bias from endogenous locations of capitals, borders, and roads. The main panel analysis addresses these challenges with a more rigorous research design.

19 There is additional data for the years 2015 and 2016. These are however generated with a different set of sensors that are more sensitive towards low levels of emissions, which makes them incomparable with the earlier data.

20 See Appendix E.1 for details on the algorithm that produces the tesselations. The relatively small unit size is chosen to be able to pick up variation within regions over time. As a robustness check, I re-estimate the baseline specification for Voronoi cells of varying size between 100 and 6400km^2. Effect sizes slightly increase with the size of units. See Appendix G.3.6.

21 I therefore drop cells without any observed population from the data. I also drop cells with oil wells (data from Lujala, Rød and Thieme, 2007) which spoil nightlight measures due to the bright flares brought about by the burning of gas. Cells with oil production are clear outliers in light intensity and feature much higher variation in nightlights over time.

7.4.1 *Travel times to capitals and state-society transactions in the Afrobarometer data*

I here report results from assessing the argument that the extent of transactions between states and their citizens depends on their transaction costs, measured through travel times to and from administrative headquarters. To that intent, I turn to data from the Afrobarometer (2018), the most comprehensive survey on political outcomes in Africa. The survey includes in particular information on individuals' tax payments, the presence of police and military forces, the provisions of local public services such as schools, clinics, and piped water, and government employment.[22] These four dimensions map onto the four main mechanisms by which low transaction costs with the government affect local development: taxation, the enforcement of 'the rules of the game,' public service provision, and the availability of private rents.

Table 7.2 presents the results of a short descriptive analysis of these four dimensions of state-society transactions. To capture the revenue citizens provide to the state, I construct (1) an index of individuals' payments of fees and taxes.[23] To mirror the three main channels of government goods provided to the population, I rely on indices for the presence of (2) law enforcement and security agents[24] and (3) public services[25] in an enumeration area. (4) A dummy for whether a respondent is a government employee captures private access to the coffers of the state.[26] I use these four indicators of state-society transactions as outcomes in simple linear models, in which I control for the baseline covariates used in the earlier chapters and the current local population count (logged, from CIESIN et al., 2011).[27]

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22 To map travel times to regional and national capitals onto respondents and enumeration areas, I draw on the geocodes of survey clusters coded by Ben Yishay, Ariel Rotberg et al. (2017). Respondents and enumeration areas are associated with travel times observed in the year of the respective survey.

23 The respective data comes from round four of the survey. The index is the first principal component of four dummies that capture whether respondents have paid income and property taxes and whether the have paid fees for government services and utilities. The index accounts for 49 percent of the variance in these variables.

24 This measure is the first principal component of dummies for the presence of police and military forces as well as of a police station in an enumeration area (rounds 1–6 of the survey). The index accounts for 39 percent of the variance in the variables.

25 This is the first principal component of dummies for enumeration areas' access to the electricity grid, piped water, a sewage system, as well as the local presence of a post office, tarred roads, schools, and clinics (rounds 1–6 of the survey). The index captures 42 percent of the variance in these variables.

26 This question was asked in round 6 to all employed respondents.

27 For a full enumeration of the control variables, see the notes of Table 7.2.

Table 7.2: Time to national/regional capital and state-society transactions

	Tax & fee idx.	Law & order idx.	Gvt. services idx.	Gvt. employee (0/1)
	(1)	(2)	(3)	(4)
Time to nat. capital (log)	−0.112*	−0.028	−0.278***	0.0004
	(0.066)	(0.045)	(0.051)	(0.006)
Time to reg. capital (log)	−0.131***	−0.220***	−0.476***	−0.026***
	(0.041)	(0.039)	(0.041)	(0.008)
Unit:	Individual	Enum. area	Enum. area	Individual
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	-0.055	-0.029	-0.027	0.097
Std.-dev. DV:	1.4	1.3	1.7	0.3
Observations	21,204	14,296	13,007	31,146
Adjusted R^2	0.134	0.160	0.415	0.057

Notes: OLS models. Control variables are: the local population count, local cash crop suitability, mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of an area, as well as the local agricultural suitability score and a mineral deposit dummy. In addition, I add a locations' logged distance to the coast, border, and closest navigable river. Precolonial ethnic attributes are captured through their political centralization, dependence on agriculture and husbandry, and the intensity of their agricultural activities. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

The resulting cross-sectional patterns partly conform with the argument that governments and their citizens transact less often where they are far appart. The respective associations are particularly stable with regards to travel times to regional capitals, and weaker for travel times to national capitals. Model 1 on individuals' tax and fee payments shows a negative correlation between payments and travel times to regional and, less precisely estimated, national capitals ($p < .1$). The presence of police and military forces inversely correlates with the distance to regional but not national capitals. Government services best align with the argument of that state-society interactions decrease with higher transaction costs: we observe less services farther away from national and regional capitals, although the 'regional' coefficient is two times larger than the 'national' one. Lastly, more respondents work for the government close to regional but not national capitals. It is important to note that about 50 percent of the size of the reported correlations is driven by differences between capitals and other places, while continuous travel times account for the other half (see Appendix G.2.1).

Taken together, these patterns suggest that distances to regional capitals are more associated with the extent of state-society transactions than national capitals, with the notable exceptions of tax payments and public service provision. This is not all too surprising, given that the hierarchical structure of administration decentralizes interactions between the state and its population.

7.4.2 *Travel times to capitals and local development in the cross-section*

More transactions between the state and the population close to administrative headquarters are the theoretical precursor to higher levels of local development. Table 7.3 underscores this logic, documenting the cross-sectional associations of travel times to national and regional capitals with the four local development indicators.

Table 7.3: Time to national/regional capital and local development: Cross-sectional OLS

	Primary educ. (0/100)	Infant mort. (0/100)	Wealth Idx.	Light/capita (log)
	(1)	(2)	(3)	(4)
Time to nat. capital (log)	−2.079***	0.665***	−0.229***	−0.073***
	(0.468)	(0.095)	(0.032)	(0.019)
Time to reg. capital (log)	−6.651***	0.320***	−0.324***	−0.333***
	(0.436)	(0.084)	(0.027)	(0.021)
Country-year FE:	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	70	9.9	-0.022	-6.5
Observations	1,705,113	2,377,578	729,927	1,385,023
Adjusted R^2	0.348	0.039	0.376	0.280

Notes: OLS models. Control variables consist of the local population count (log), local cash crop suitability, mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, and the mean altitude and slope of an area, as well as the local agricultural suitability score and a mineral deposit dummy. In addition, I add a locations' logged distance to the coast, border, and closest navigable river. Precolonial ethnic attributes are captured through their political centralization, dependence on agriculture and husbandry, and the intensity of their agricultural activities. Control variables for models of primary education consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. In models of househols' wealth, I control for the size and the number of children (both linear and squared). Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Across all outcomes, travel times to both types of capitals negatively correlate with local levels of development. On average, a traveler can observe education and infant survival rates drop, households getting poorer and darker at night as she drives away from a regional or national capital. In substantive terms, Table 7.4 shows that these changes are large. For example, driving five hours from a national capital to its periphery, a traveler can observe primary education rates deteriorate by 3.7 percentage points, infant mortality rise by 1.2 percentage points, household wealth decrease by almost half a standard deviation and nightlights per capita decline by 13 percent. For a traveler who leaves a regional capital, changes in education rates and nightlight emissions are three to four times as large. The correlation of times to regional capitals and household wealth is of similar size, and infant mortality rates increase less with travel times to regional than with travel times to national capitals.

Table 7.4: Travelling away from administrative capitals and changes in development

Outcome	Change in...	$0 \rightarrow 5$ hrs. to nat. capital	$0 \rightarrow 5$ hrs. to reg. capital
Primary education rate	Perc. points	-3.7 [-5.4, -2.1]	-12 [-13, -10]
Infant mortality rate	Perc. points	1.2 [0.86, 1.5]	0.57 [0.28, 0.87]
Household wealth	Std. dev.	-0.41 [-0.52, -0.3]	-0.58 [-0.67, -0.49]
Nightlights per capita	Percent	-13 [-20, -6.5]	-60 [-67, -53]

Note: 95 % CIs in parentheses.

I conduct a variety of robustness checks to address some of the caveats of these cross-sectional results, except for the potential endogeneity of administrative borders and capitals, which has been highlighted by the analyses in Chapters 4 and 5. I consider this endogeneity a vital threat to the validity of these cross-sectional estimates, which is best addressed by the panel analysis in the next section. I therefore provide only the brief following summary and direct the interested reader to Appendix G.2 for all additional specifications and results.

In particular, these specifications account for endogenous road building by relying on an instrumental variable strategy that is based on simulated road networks and introduced in the next chapter,[28] and for biased postcolonial changes in administrative geographies by drawing on travel times to countries' first postcolonial capitals. Furthermore, I implement the robustness checks that are discussed and motivated in the main panel-analysis below. These address potential biases arising from, inter alia, access to national and international markets, the power status of ethnic groups, and local road densities. In sum, the results show patterns largely consistent with those reported above. Importantly, the association of travel times to regional capitals with local infant mortality rates is unstable and turns statistically indistinguishable from 0 in some specifications. This coincides with the results from the panel analysis. Highlighting the need for further research in this area, it is theoretically and empiri-

28 In Chapter 8, distances to national capitals rather than regional capitals are the main predictor. Given that national capitals and borders rarely change and were set up early on by colonizers, I expect them to be much less endogenous than regional administrative geographies.

cally unclear why infant mortality rates do not exhibit the same patterns as other indicators of local development.

7.5 CHANGES IN TRAVEL TIMES TO CAPITALS AND LOCAL DEVELOPMENT

Moving beyond the description of cross-sectional patterns, the main analysis of this chapter assesses the impact of changes in travel times to national and regional capitals on local development. I first discuss the respective empirical strategy to then present the results. These vary across outcomes and show that local education rates rise in parallel to reductions in travel times to national and regional capitals. Lower infant mortality rates, in comparison, are only associated with reductions in travel times to national, but not regional capitals. Lastly, nightlight emissions per capita significantly decrease with lower travel times to regional capitals. The estimated effect of reductions in travel times to national capitals is of the same size, but statistically insignificant.

7.5.1 *Empirical strategy*

As highlighted already, the main challenge of the empirical analysis is to identify the effect of state reach on development despite the many contemporary and historical processes that affect both variables. For example, rich and populous towns are often capitals of their immediate surroundings, with larger and more populous areas governed by bigger cities. Roads are built to connect resource-rich areas and major cities that are often regional or national capitals. Finally, wealthy people may more readily migrate to places where the state is present. Such dynamics simultaneously affect individual's economic well-being and travel times to their capitals.

Because the cross-sectional component of the resulting endogeneity is likely the most influential one, the empirical strategy exploits only *temporal* variation within the same location to identify the effect of changes in state reach on development. At the baseline, I estimate the following ordinary least square regression:

$$Y_{ipct} = \alpha_p + \lambda_{ct} + \beta_1 \, \textsf{time to nat. cap.}_{pt} + \beta_2 \, \textsf{time to reg. cap.}_{pt} + \delta X_{ipt} + \epsilon_{ipct}, \quad (7.2)$$

where β_1 and β_2 capture the effects of the travel times to point p's regional and national capitals at time t. For outcomes retrieved from the DHS, individuals i

are assigned to the 5×5 km grid-cell p in which they were interviewed.[29] For the nightlight measures p is synonymous with i, being the Voronoi cells within which I aggregate nightlights and travel times. The model controls for all constant attributes of points/units and country-years through fixed effects α_p and λ_{ct}. A vector of control variables X_{ipt} controls for attributes of respondents to increase precision. Individual-level controls for modeling education consist in respondents' age and its square as well as a female dummy. Models of infant mortality include the mother's age at birth and its square, the infant's birth-order and its square, as well as female and twin dummies. In the baseline specification, I add no time-varying attributes of Voronoi cells. I cluster standard errors on the point p and country-year levels.

This baseline model assumes at its core that changes in the distance to regional and national capitals are exogenous to local development observed the years thereafter. Unfortunately, the data do not lend themselves to a straightforward binary difference-in-difference design. This is because the treatments – changes in travel times – are continuous variables with positive and negative values (see e.g. Figure 7.4b). Changes in administrative geographies and road networks also 'treat' each location multiple times since independence. To account for the potential endogeneity of these treatments, I present a series of robustness checks below.

7.5.2 *Results*

The results from the empirical analysis show three patterns. First, reductions in travel times towards regional and national capitals are robustly associated with higher primary education rates. Second, infant mortality rates are improving as travel times to national, but not regional capitals decrease. Lastly, nightlight emissions significantly increase with declining travel times to regional capitals. Their relation to changes in the time to national capitals is of the same size but statistically insignificant. Throughout, the estimated effects are smaller than those found in the cross-sectional analyses.

Table 7.5 presents the results from estimating the baseline specification. Interpreting coefficients in substantive terms, Table 7.6 presents the estimated changes in develop-

29 Grid-cells are defined by the raster on which travel times are measured, so that all respondents in the same cell and year are attributed the same travel times towards their capitals. Note that the DHS randomly displaces clusters by up to 10 (2) kilometers in rural (areas) to protect the privacy of respondents. This introduces random noise into the measurement of local state reach.

Table 7.5: Changes in time to national/regional capital and local development

	Primary educ. (0/100)	Infant mort. (0/100)	Light/capita (log)
	(1)	(2)	(3)
Time to nat. capital (log)	−2.667***	0.890***	−0.044
	(0.420)	(0.239)	(0.028)
Time to reg. capital (log)	−1.102***	0.067	−0.045**
	(0.313)	(0.156)	(0.018)
Point FE:	yes	yes	yes
Country-year FE:	yes	yes	yes
Survey FE:	yes	yes	yes
Controls:	yes	yes	–
Mean DV:	70	9.9	-6.5
Observations	1,893,067	2,634,666	1,506,991
Adjusted R^2	0.445	0.051	0.836

Notes: OLS linear models. Control variables for models with primary education as the dependent variable consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

mental outcomes as a location experiences a reduction of travel times to its national and regional capital from two to one hours. Such a decrease in transaction costs with the national (regional) capital is associated with an increase in primary education rates by 1.1 (.45) percentage points. While smaller than in the cross-sectional analysis, both effects are precisely estimated.

Similarly, infant mortality rates decrease by .36 percentage points when the national capital moves an hour closer to town. In contrast, I do not find evidence for an impact of changes in the time to regional capitals on infant mortality rates. The respective coefficient is close to zero and statistically insignificant. This is in line with the mixed cross-sectional results reported above and the somewhat inconclusive evidence on the effects of district splintering in Malawi, Nigeria, and Uganda found by Grossman, Pierskalla and Dean (2017).

It is a unclear what causes this deviation. Beyond the possibility of a true non-effect, one driver might consist in potential effects of travel times to capitals on mothers' reporting behavior. In particular, it might be that, if education rates and access to health care increase with reductions in the distance to regional capitals, better educated mothers that received health care might more accurately report the births and deaths of their children. However, because this mechanism would also affect the estimated effect of travel times to national capitals on reported infant mortality, such dynamics are unlikely to fully explain this result. In additional, measurement error in the changes of travel times that originate from the slight non-alignment of consecutive Michelin maps discussed above adds attenuation bias.[30]

Lastly, per-capita nightlight emissions are significantly affected by changes in travel times to regional capitals. As the average distance to the regional capital of a Voronoi cell decreases from two to one hours, nightlight emissions increase by 1.8 percent. The same change in the distance to its national capital is associated with an increase in nightlights of the same size, but this estimate is statistically insignificant at conventional levels. This imprecision might be due to the fact that the nightlight data covers a shorter period of time (1992-2013) which includes two changes in the locations of national capitals in newly independent South Sudan (2013) and Eritrea (1992), each with only one pre-/post-treatment year. Thus, the estimate is mainly driven by changes in road connections to national capitals across the continent which are more gradual and have, as the robustness checks below show, only second order impacts on local development.

Table 7.6: Getting closer to administrative capitals and changes in development

Outcome	Change in...	2 → 1 hrs. to nat. capital	2 → 1 hrs. to reg. capital
Primary education rate	Perc. points	1.1 [1.4, 0.75]	0.45 [0.69, 0.2]
Infant mortality rate	Perc. points	-0.36 [-0.17, -0.55]	-0.027 [0.096, -0.15]
Nightlights per capita	Percent	1.8 [4, -0.45]	1.8 [3.3, 0.39]

Note: 95 % CIs in parentheses.

One important property of the baseline analysis is that it combines cases of changes in the travel time to national capitals that originate from re-locations of capitals with those that stem from changes in road networks.[31] Splitting the sample into these two groups reveals that the effects of travel times to national capitals are primarily driven by re-locations of capitals (see Appendix G.3.1). Changes due to road networks have a smaller, but statistically significant effect on primary education rates and no effect on infants' mortality. I find no effect of decreases in travel times to the national capitals on nightlights in Eritrea and South Sudan. This is not too surprising given the

30 To assess the importance of this bias, a robustness check in Appendix G.3.4 fixes road network information while only varying the location of capitals. As a result, the point estimate of the effect of travel times to regional capitals increases but remains statistically insignificant.

31 In the sample of DHS respondents, capital re-locations have occurred in Côte d'Ivoire, Nigeria, and Tanzania. The data on nightlights include the secessionist cases of Eritrea and South Sudan.

one-year pre-/post-treatment periods and the violence that came with the countries' secession.

7.5.3 *Robustness checks*

TESTING FOR NON-PARALLEL TRENDS AND BIASED MIGRATION: The most serious threat to inference consists in non-parallel trends in locations that do and do not experience changes in their distance to capitals. Such trends would indicate that a location that, for example, becomes a capital in 2000 had already before experienced increases in local development. These increases or any of their proximate causes may lead to the locations' upgrade to a capital. If that was the case, either reverse causality or omitted variables would bias the estimates. In order to test for non-parallel trends, I add lead indicators of the future time to national and regional capitals to the baseline specification. Specifically and because primary education rates can be affected by state reach until teenage years of a respondent, I add the travel times to the capitals of respondents at age 12 to 16 to the models and take the average travel time to capitals between age 6 and 11 as the main predictor. For infant mortality rates and nightlight emissions, such a complication is not needed. I thus add the travel times in the years $t+1, ..., t+5$ to the respective models.

At the very left of each panel, Figure 7.7 plots the resulting main effects of contemporaneous travel times to capitals together with the sum of the coefficients of the leads (see also Appendix G.3.2). The main estimates are similar to the baseline results and the effect of leads are close to zero. The one exception to this pattern consists in the estimated negative effect of future travel times to national capitals on education rates ($p < .1$) which indicates non-parallel trends. However, these lead effects may be due to biased migration patterns of, for example, adults who migrated into a new national capital such as Abuja after having gone to school.

The second and third sets of estimates in the panels on education and infant mortality rates explore this possibility, using the smaller sample of DHS respondents on which I have migration-related information.[32] The results suggest that migration slightly biases pre-treatment trends and the baseline estimate more generally. The

32 I therefore draw on data from the much smaller DHS Individual and Men's Recodes that were fully interviewed and code a migrant dummy capturing whether a respondent has not 'always' lived in her current place of residence. Since observations are contemporaneous in the case of nightlight emissions, ex-post selection into treatment is not an issue here.

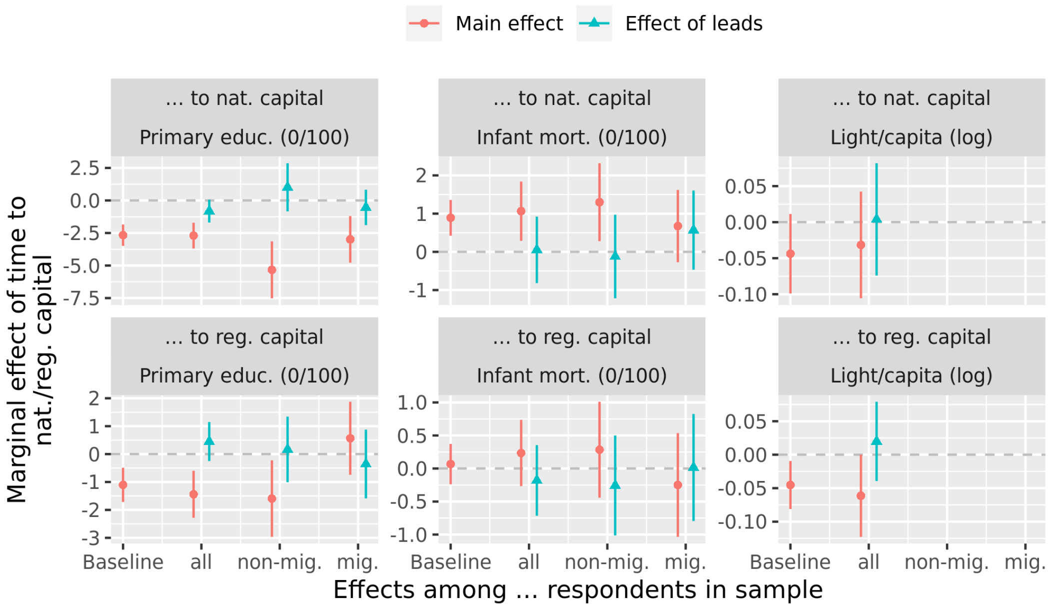


Figure 7.7: Testing for parallel trends: estimated effect of contemporaneous and future travel times to regional and national capitals.
Note: Based on models in Tables G.10 and G.11. The coefficients of leads, that is future travel times to capitals, are based on the sum of coefficients of travel times in $t+1, ..., t+5$, where t is age 11 for education rates, the birth-year of infants, and the year of observation for the nightlight analysis. The effects of current and future travel times to capital on primary education are based on the smaller sample of respondents from the DHS Individual Recodes which provide information on migration.

main estimate of the travel time to national capitals on primary education is bigger for non-migrants than for migrants.[33] In turn, the lead effects are only negative (but insignificant) among the migrant sample, but not among non-migrants. Similarly, education rates of the non-migrant sample are more responsive to travel times to regional capitals, which have no discernible effect on primary education among migrants. In sum, these patterns suggest that trends before changes in distances to capitals are parallel among non-migrants in the sample. Migrants bias the estimates of effects on education downwards. Their bias in the analysis of infant mortality goes in the same direction but is much weaker.

SIMULTANEOUS OMITTED VARIABLES: Beyond non-parallel development trends at the local level, time-variant omitted variables that simultaneously cause travel times to capitals to decrease and development outcomes to improve might drive

33 Estimates of the interaction of migrant × travel times without the lead are similar to the ones reported in Figure 7.7 (see Table G.12).

the patterns observed in the data. I address four threats in particular, with details presented in Appendix G.3.4:

- First, investments in road infrastructure may anticipate future development opportunities such as the planned opening of a mine. Two strategies address this threat of reverse causality. First, I control for the time-varying density of the local road network. Second, I re-estimate the baseline model with travel times to time-varying national and regional capitals computed separately with each of the 23 road networks. This results in 46 travel time measures each of which covers the entire postcolonial period and is based on only one of the 23 road networks. The results from both checks show that changes in the road network do not drive the observed patterns. They instead suggest that the main effects stem from changes in administrative geographies and their geographically uneven impacts. In addition, the point estimate of the effect of travel times to regional capitals on infant mortality rates increases to .2 but remains statistically insignificant. This indicates that the estimates that are based on the time-variant road networks are affected by attenuation bias that is caused by the slight misalignment of roads observed in consecutive Michelin maps.

- Second, the baseline results might be spurious to changes in the market access induced by changes of roads and population distributions (Faber, 2014; Donaldson, 2018; Jedwab and Storeygard, 2018; Storeygard, 2016). As a response, I construct time-variant national and international market access measures for each point towards the population of the 1243 most important cities in Africa.[34] Adding these as control variables does not change the results. This is in line with the 'stable-road-networks' test, since changes in the network account for most of the variation in locations' market access.

- Third, the results may be biased by political processes that affect changes in administrative geographies and development at the same time. New administrative units might be systematically created to reward government allies (Green, 2010; Hassan, 2016; Gottlieb et al., 2019) or harm opponents (Resnick, 2017). Such processes may be accompanied by governments' ethnic or regional favoritism that has a direct effect on local development (Burgess et al., 2015; Franck and

34 The construction of these measures follows the relevant literature, in particular Donaldson (2018) and Eaton and Kortum (2002). For all details, see Appendix G.3.4.

Rainer, 2012; Hodler and Raschky, 2014). To capture such dynamics, I use the EPR data set (Vogt et al., 2015) to control for the political status of ethnic groups and their involvement civil wars. This extension does not significantly affect the results.

- Fourth, the baseline results might be driven by very local dynamics that increase locations' economic development and affect their chances to become a capital. I thus control for simple dummies that capture whether, for a given year, a respondent's place of residence is in or within an hours' travel of a regional and national capital, and whether a Voronoi cell contains a capital. The results show that the baseline results are not caused by dynamics within places that change their status of being a capital.

ALTERNATIVE OUTCOMES: Alternative indicators for educational achievements, birth-related health care, and nightlight emissions conform with the baseline results (see Appendix G.3.5). Decreases in travel times to national and regional capitals are associated with better chances of respondents to have been at school at all, and more years of education. Secondary education rates respond to travel times to regional but not national capitals. The DHS data also provide evidence that travel times to national capitals affect infant mortality through better public health care. Decreases in the time to national (but again not regional) capitals substantially increase mothers' chances of receiving professional prenatal care, and giving birth in a public hospital with professional rather than traditional assistance.[35] Going beyond measures of nightlights per capita, decreases in the travel time to regional but not national capitals come with an increased probability of any and more absolute nightlight emissions.

ADDITIONAL ROBUSTNESS CHECKS: Appendix G.3.6 reports the results of a number of additional robustness checks. In particular, the results show that over-weighting countries sampled more often by the DHS does not drive the results. When weighting Voronoi cells according to their population to produce results representative of the average inhabitant of the continent, the effects of travel times to regional capitals double. Lastly, clustering standard errors on the administrative unit, the country-year alone, or both does not affect the statistical significance of the baseline results.

35 Because this question is also asked with regard to births after which an infant has not survived, the above outlined reasoning about potential reporting biases in the data may also be relevant here.

In general but with two deviations, the results suggest positive effects of decreases in state-society transaction costs on local development. Where national and regional administrations move closer to the citizens they govern, education rates improve. Infant mortality rates increase with travel times to national but not regional capitals. Lastly, nightlights become brighter when regional capitals move closer. They also do so when an area gets better connected to its national capital, but this effect is statistically insignificant. One remaining open question outside the scope of this analysis concerns the difference between the cross-sectional patterns and the results from the panel analysis. Beyond endogeneity issues affecting the former, a part of the divergence between the results may originate in sticky spatial development equilibria created by early (colonial) state building that successive efforts at extending state reach are not able to revert. Similarly, changes in state-society transaction costs probably have long-term effects that may not be adequately captured in the panel analysis, in particular in the case of more recent changes in administrative capitals.

7.6 CONCLUSION

One of the most important determinants of states' impact on development is their ability to reach out and govern their citizens, exchanging with them taxes for public services. Analyzing African states, Herbst (2000) argues that their capacity to rule is curbed by 'difficult geographies' that exhibit populated regions far away and isolated from national capitals. Acknowledging and measuring African states' efforts to overcome these natural impediments to centralized governance, this chapter has analyzed the effects of changes in the costs of state-society transactions on local development. Following the theoretical framework of this dissertation, I have argued that physical constraints to transactions between the government and its population impede local development by reducing citizens' access to state-run law enforcement, public services such as education and health care, and private rents handed out by administrations.

Measuring state-society transaction costs through travel times between citizens and administrative capitals since independence, the chapter has documented spatio-temporal development patterns that support the argument that cheaper transactions with governments generally increase the wealth and well-being of citizens. Shortening the time it takes to reach one's national capital improves education and infant mortality rates, and has a positive, albeit insignificant effect on local nightlight emissions. Increases the accessibility of regional capitals similarly increases primary education rates, has no discernible effect on infant mortality, and is associated with brighter nightlight emissions.

With these results, this chapter opens up an array of further questions. The first question concerns the origins of the variation in the effect of state reach on different outcomes, and might be answered by more precise theorizing about the nature of goods or heterogeneous effects of state reach between or within countries. Such heterogeneity might in particular relate to the importance of state institutions and the bargaining power they give to citizens, which may affect the developmental effects of state reach. Second and addressed in the next chapter, the focus on development masks potentially important but not necessarily unambiguous effects of state reach on political violence. Lastly, and being the focus of a short excursus thereafter, the estimates of the developmental effects of state reach open up the way to investigate the effects of 'inefficient,' arbitrary national borders cut across the African continent during the colonial Scramble after 1885. Through simulations of counterfactual national designs, the excursus shows that in particular densely populated areas far away from their capitals would have profited from 'better' administrative geographies.

8

ROADS TO RULE, ROADS TO REBELLION: RELATIONAL STATE CAPACITY AND CONFLICT IN AFRICA

Based on co-authored work with Philipp Hunziker and Lars-Erik Cederman[1]

I told you not to build any road. [...]
Building roads never did any good.
I've been in power in Zaire for thirty years
and I never built one road.
Now they are driving down them to get you.
Mobutu Sese Seko to Juvénal Habyiarama
(quoted in Englebert, 2000, p. 135)

Weak state capacity is a leading explanation of civil war. Scholars and policy-makers frequently argue that rebels fight where governments are too poor, incompetent, and distant to defend their monopoly of violence. Yet, despite its relevance in the literature, the empirical link between state capacity and conflict remains elusive. For one, existing studies often argue that conflict occurs where states are absent. This perspective neglects that conflict is inherently relational. Whether the state is capable of defending its monopoly of violence also depends on whether local conditions are favorable to non-state mobilization. Second, it is challenging to measure subnational state capacity consistently across countries. Finally, investments in state capacity may be endogenous to (anticipated) conflict, complicating causal inference.

This chapter addresses these points. My co-authors and I develop a relational concept of state capacity determined by the costs of transactions between local

1 This chapter is based on the joint Working Paper "Roads to Rule, Roads to Rebellion: Relational State Capacity and Conflict in Africa." I have slightly adapted the manuscript, to avoid redundancies and make the link between social control, transaction costs, and physical accessibility more explicit. I have furthermore added a short discussion on how high levels of internal connectedness of ethnic groups far away from the state can produce *incentives* for revenue-seeking challengers to the state to seize upon low transaction costs with the local population and expand their power. These incentives add to the opportunities for rebellion in the respective ethnic groups.

populations and the state as well as potential rebels. We operationalize this concept with road network data that captures how well states can reach their subjects, and how well subjects are interconnected among themselves. Focusing on ethnic groups in Africa, we show that state access reduces the risk of conflict, while groups' internal connectedness increases it. This result holds when we exploit exogenous variation from simulated road networks.

Our theoretical argument focuses on violent competition among states and non-state actors over controlling local populations. We argue that conflicts arise where states are weak compared to potential challengers. Because high transaction costs impede political control, we operationalize this concept of "relational state capacity" with accessibility metrics computed with road network data. In parallel to the theoretical argument of this thesis, we argue that states can exercise control only in areas that are accessible from their capitals. However, roads also benefit local non-state elites. Areas that are internally well interconnected will be considerably easier to control, which gives potential rebels the power to mobilize the local population and challenge the state. Combining these two arguments, relational state capacity is low in areas that are poorly accessible from the capital, but internally well connected. We test this argument with data on ethnic groups in Africa, where challengers that mobilize along ethnic lines frequently threaten state rule by violent means.

In parallel to Chapters 5 and 7, we compute a proxy of relational state capacity using the road data retrieved from the Michelin maps. These allow measuring ethnic groups' accessibility from the capital and their internal connectedness via traveling times. We define relational state capacity as the difference between both measures and analyze its effects on local conflict, captured by event-level conflict data from the post-1997 period (Raleigh et al., 2010).

However, and as noted earlier in the thesis, state building affects the presence of roads and their network structure. Where road builders react to threats of violence or secondary factors such as natural resources that affect conflict risks, road networks will be endogenous to conflict patterns. An instrumental variable strategy based on artificial road networks addresses this caveat. Specifically, we simulate road networks that maximize the connectivity between any two inhabitants of a country using historical population data. These simulated networks are credibly exogenous to politics by design and exhibit strong similarities to the observed networks. We recompute our accessibility metrics on these simulated networks and employ the resulting variables as instruments.

In line with our expectations, we observe more armed groups, more violence among these armed groups, and more violence between armed groups and the state in ethnic settlement areas with low relational state capacity. The results from our instrumental variable analyses imply that decreasing states' access to ethnic groups by a moderate 10 percent or increasing groups' internal accessibility by the same amount, leads to an increase in the number of armed groups and battles between them by 3.7 and 2.8 percent, respectively. The same change in relational state capacity is associated with an increase in the number of battles between government forces and armed groups by 1.2 percent, a result that is stable in the baseline analyses but unstable in some sensitivity checks.

With this exception, the data support our claim that challenges to state rule arise not just where states are weak, but where they are at risk of being outgoverned. Our findings also highlight the dual role of road networks as a tool for political mobilization and control. As Mobutu knew well, building roads is literally a two-way street. It extends states' ability to govern, but also provides potential challengers with an infrastructure for mobilization.

8.1 LITERATURE

Much of the contemporary discussion on state capacity and conflict is inspired by Fearon and Laitin (2003), who assert that civil wars are a consequence of weak statehood. Following their seminal contribution, many studies have refined the concept and measurement of state capacity. A first wave of research argues that conflict erupts where states lack the military and financial resources to uphold their monopoly of violence. While intuitive, country-level studies show mixed evidence for this resource-centric mechanism (Buhaug, 2010; Walter, 2006; Fjelde and de Soysa, 2009; Hendrix, 2010). A second argument focuses on institutions rather than resources. It holds that low-capacity states are those with an 'incoherent' set of institutions that feature both autocratic and democratic decision-making elements (Hegre et al., 2001). Again, however, there is only mixed evidence that incoherent institutions increase the risk of conflict (Vreeland, 2008).

Finally and at the center of this chapter, a third approach focuses on the role of social control, which refers to the ability of states to control the behavior of their subjects through targeted incentives (Mann, 1984; Migdal, 1988). As highlighted by the theoretical discussion in Chapter 3, capable states are those that can administer selective incentives through, for example, a professionalized police force, bureaucracy, and a judicial system. Social control is central to the prevention of rebellion because it undermines mobilization. Individuals who believe that the state scrutinizes their behavior will be less likely to join, and more likely to denounce organizations planning to challenge the state's monopoly of violence.

Perhaps the most widely used proxy of social control is the fraction of national economic output that states capture through taxation (see Thies, 2007; Fjelde and de Soysa, 2009; Thies, 2010; Hendrix, 2010; Buhaug, 2010). Because levying taxes requires effective and penetrative institutions, so the argument, high tax ratios should be indicative of high social control. Empirically, however, the relationship between states' tax ratios and the occurrence of rebellion is inconclusive (Thies, 2010; Fjelde and de Soysa, 2009; Buhaug, 2010). One likely reason is endogeneity (Thies, 2010). The fear of rebellion may deter states from levying taxes, which would veil any pacifying effect of taxation. Another drawback is that most data on taxation is only available at the country-level, while social control is best conceptualized and measured at a more local level.[2] Because the effectiveness of institutions of social control deteriorates drastically over distance, states may be strong in some parts of their territory, but not in others (Buhaug and Rød, 2006; Buhaug, 2010; Tollefsen and Buhaug, 2015).[3]

Adopting this spatially explicit logic, a number of studies use accessibility as a proxy for social control. These studies find that areas that are difficult for states to reach – i.e. areas far removed from their country's capital, and covered by mountainous terrain – are particularly prone to civil conflict (Buhaug and Rød, 2006; Raleigh and Hegre, 2009; Tollefsen and Buhaug, 2015). These results coincide with long-standing arguments by Herbst (2000) and Tilly (1990) that favorable geography is a key determinant for the emergence of strong social control. As I have already argued, these 'geographic' findings have an important drawback: They do not take

2 But see Harbers (2015) who presents subnational tax data to measure local state capacity in Ecuador.
3 Three related and recent contributions introduce a census-based measure of local 'legibility' (Lee and Zhang, 2017), a measure of local institutional quality computed from survey data (Wig and Tollefsen, 2016). These approaches rely on census or survey data, the collection of which is inherently affected by social control. Third, Koren and Sarbahi (2018) use nightlight luminosity as a proxy for local state capacity. This comes with difficulties to distinguish local state capacity from local development.

into account that states may overcome their geographic constraints by investing in transport infrastructure.

As a remedy, some authors study the effects of roads, in particular the density of road networks, on social control and conflict. In a pioneering effort, Herbst (2000) collects aggregate data on African road network densities and finds that weak social control and sparse road networks tend to go hand in hand. Buhaug and Rød (2006) and Raleigh and Hegre (2009) analyze the relationship between local road densities and African conflict at the sub-national level, but obtain mixed results.[4] Most recently and studying one-sided violence, Rogall (2017) finds that rainfall-induced reductions in the accessibility of Rwandan villages curtailed Hutu militia's efforts to mobilize their inhabitants to participate in the genocide of their neighbors.[5]

Despite considerable progress over the past decade, the measurement of social control and its effect on political violence remains elusive for three main reasons. First and with the exception of Rogall (2017), current measurements of accessibility – and, by extension, social control – do not appropriately factor in transport networks. While country-level aggregates obfuscate local variation, local road density metrics disregard that roads form a network. Therefore, they cannot capture the costs associated with actually *reaching* an area. To measure these transaction costs, we approximate accessibility of a given location via realistic travel times on observed road networks.

Second, past uses of road data to estimate the effect of social control on conflict have suffered from an inherent endogeneity problem: Low social control and anticipated or actual violence may deter road-building, thus complicating causal inference. Rogall (2017) relies on short-term variation in roads quality, a strategy that is econometrically sound but insufficient to capture the long-term effects of accessibility. As a solution, we leverage exogenous variation in the structure of road networks to identify the causal effect of accessibility on conflict.

Finally, with few exceptions, the recent empirical state capacity literature tends to ignore the relational nature of social control and accessibility.[6] Many scholars take a state-centric view, argue that rebellion erupts where the state is absent, and test whether inaccessibility is associated with conflict. In contrast, we advance a relational

4 While Buhaug and Rød (2006) find local road densities to be associated with less conflict events, Raleigh and Hegre (2009) report a positive association.

5 In a similar vein, Zhukov (2016) finds that local road networks are associated with one-sided violence perpetrated by governments and rebels.

6 Dargent, Feldmann and Luna (2017) provide a theory of relational state capacity similar to our own, but do not study its relationship with organized violence.

perspective: Conflict is likely where actors that compete with the state over local power have more social control than the state. As a corollary, we argue that conflict is likely in areas that are more accessible to such challengers than they are to the central government.

8.2 THEORETICAL ARGUMENT

In this section, we advance a theoretical framework of relational state capacity (RSC) and conflict in Africa that highlights the role of the costs of transactions between local populations and the state as well as its challengers. As in the earlier chapters, this motivates the accessibility-based measurement strategy for testing our claims.

8.2.1 *Social control and the state's monopoly of violence*

Low transaction costs between the state and society increase the state's social control over its population. Physical accessibility is key for rulers to monitor the behavior of their subjects, punish defectors, and reward those who stick to the rules of the state-society bargain. As Chapter 7 shows, transaction costs are therefore an important determinant of the extent of material state-society transactions. Where citizens are far away from the state, they pay less taxes and get less public services in return.

In the same vein, low costs of interacting with society and the resulting social control allow the state to monopolize violence and keep the peace. The underlying mechanisms can be split into long- and short-term effects of low transaction costs. In the long run, states that face low transaction costs can monitor and punish individual behavior, and thereby deter subjects from founding, joining, or financing rebel organizations. States can do so in accessible territory through a number of means, including the sticks swung by their own agents on the ground, the carrots provided with public services (see Chapter 7), and generally higher levels of integration into the national community through increased state-society interactions. Few civilians, including those who share the grievances of undeterrable ideologues, will risk their livelihoods for some greater cause with uncertain outcome (Lichbach, 1994). As a result, it is difficult for prospective rebels to challenge state rule in areas cheaply accessed by the government.

Notably, counterinsurgency strategists have long highlighted this logic (Galula, 1967; Kilcullen, 2010).

In the short run, accessibility has an impact on social control, in that it is necessary for upholding the government's monopoly of violence in times of crisis. If state rule in a given area is contested, maintaining control requires the ability to quickly punish defectors and curb violent threats. In practice, this means employing coercive resources for repressive, punitive, and mobilizational purposes, responses that requires physical accessibility.

Scholars have argued at length that African states' lack of accessibility and control constitutes the main reason for their "security predicament" (Ayoob 1995, see also Migdal 1988; Herbst 2000). As noted earlier, colonial rule hardly ever resulted in administrative structures that yielded high social control. Backed by imperial armies that could be called in on demand, colonial administrations were not designed to maximize control over their subjects, but to maximize profits from extraction. Consequently, many post-independence leaders inherited states that controlled capitals and resource-rich areas, but not the 'empty panorama' formed by peripheries (Fanon 2004, see also Herbst 2000, Roessler et al. 2018). As shown in Chapters 5 and 7, governments' success in overcoming these limitations has been modest. Still today, many African states have little control over remote areas, leaving much room for rebels to mobilize and fight them.

8.2.2 *Fragmented social control and mobilization*

This state-centric view predicts conflict to erupt where states have little access to, and control over their subjects. This logic rests on the assumption that rebellions emerge from a vacuum of social control caused by the state's absence. Migdal (1988, p. 28) points however out that this view is incomplete, as the absence of the state rarely implies a vacuum of social control:

> [T]hrough most of human history, legions of strategies have been at work in areas today claimed by single states. Territories have hosted a diversity of rules of the game – one set for this tribe and another for a neighboring tribe, one for this region and another for that. Social control

> has not been of a piece, but it has frequently been highly fragmented through a territory.

Thus broadening the state-centric perspective, Migdal (1988) argues that social control often resides with societal institutions that emerge among communities that speak the same language, trade at the same markets, and follow the same customs. In other words, where states are absent or weak, social control is typically organized along ethnic lines.

An important difference between these ethnic governance institutions and modern states lies in the way social control is organized. Today's ethnic power holders rarely preside over "proto-states" that rule their subjects directly. Rather, non-state governance institutions typically rely on patronage networks. Hence, hierarchical networks of personal allegiance and protection, rather than professionalized bureaucracies, ensure the surveillance of, and control over subjects. Acknowledging that ethnic governance structures often compete with, or sometimes even replace the state has important implications for our understanding of political violence. For one, the emergence of armed groups is much more likely where ethnic governance institutions trump state control and allow prospective rebels to govern their realm (e.g. Weinstein, 2007; Mampilly, 2011; Arjona, 2014). If local ethnic leaders decide to defend or expand their power by force, dense and hierarchical social networks will allow them to mobilize more funds and fighters than otherwise possible (e.g. Humphreys and Weinstein, 2008; Parkinson, 2013). Similarly, existing governance structures allow ethnic elites to rely on civilian cooperation, a key requirement for successful insurgencies. Civilians will tend to cooperate with whatever party is able to punish defection (Kalyvas, 2006). Tried and tested patronage networks likely provide higher levels of surveillance, control, and thus threat potential than a weak and distant state bureaucracy (Weinstein, 2007).

Despite these differences between the structures of state and non-state governance, non-state rulers or those who attempt to rise to that status are as constrained by transaction costs with the population as the state is. Local ethnic elites will be better able to monitor, punish, and reward 'their' subjects in areas with high levels of internal accessibility. As with state-society interactions, the underlying logic has a long- and short-term component. Over the long run, infrastructure that permits accessing members of an ethnic group at low cost facilitates the maintenance of effective non-state governance structures. In the short run, accessibility facilitates upholding control over subjects in times of conflict by coercive means, i.e. when competing over civilian cooperation against the state or other challengers (Zhukov, 2012).

The similarity of the role of transaction costs for the state and non-state actors has not only implications for the latter's ability to mobilize, but also for their incentives to do so. Just as state rulers, they can profit from cheaper interactions with local populations by being able to trade security and public services for taxation. As a result, revenue-maximizing non-state actors have greater incentives to challenge state rule in areas where transactions with society are cheap.[7] The promise of greater rents for rebels in ethnic groups that can be cheaply ruled adds to the increased opportunities for rebellion they offer. As Schouten and Bachmann (2017) note in a recent report, armed groups "aim to use [infrastructure] for the same purposes as the state, namely, to expand their control and authority." They go on to cite an exemplary case from the Eastern DR Congo where the rebel group 'Raya Mutumboki' did not interfere with the construction of a road "because they used it to extend their political grip".

The emergence of armed actors that can easily access the group of people they aim to rule does not only raise the prospect of conflict against the state, but also that of conflict against other, competing non-state actors. Unlike state bureaucracies that rest on centralized and mutually exclusive hierarchies, ethnic governance networks are decentralized and often overlap. Consequently, multiple leaders can mobilize subsets of 'their' ethnic group for their personal gain. Absent a state capable of enforcing a peaceful resolution of disputes, competition among local power-holders may quickly lead to violence between the armed groups they command (Skaperdas, 2002). Indeed, violence among rebel organizations that are fighting the state is a common characteristic of civil wars (Fjelde and Nilsson, 2012).

A cursory analysis of recent African history confirms that fragmented social control is a more fitting description of the political topography of African states than the state-centric perspective of the previous section. As analyzed at length in Chapter 4, European colonizers encountered precolonial societies with a large variety of political

7 It is unlikely that potential rebels actively choose the most profitable ethnic group to mobilize. However, in a logic of the survival of the fittest, the rise of successful rebels becomes more likely when the geography of an ethnic group is such that rebellion pays off.

structures, ranging from complex empires to acephalous ethnic groups (Murdock, 1959). Through their varying strategies of direct and indirect rule, colonial rulers invented or co-opted local notables as intermediaries to govern oftentimes ethnically delimited constituencies. Many of these (pre)colonial governance institutions survived well into the postcolonial period (Mamdani 1996; Crowder 1964; Herbst 2000; Reno 1995). Left with fragile central administrations and armies, many postcolonial governments had to accept that actors weakly tied to or even outside the state's formal institutions ruled over much of their states' territory (Migdal, 1988). This has been an important driver of conflict in Africa: civil war is often the result of ethnic strongmen defending or expanding their power against local rivals and the state (Reno, 1999).

The acknowledgment that social control is often fragmented adds a second dimension to the link between social control and conflict. Going beyond the focus on the presence or absence of the state, it accounts for the social control exerted by those who might challenge their nominal government. What determines the feasibility of rebellion is *relational state capacity* (RSC): the degree to which the state's control over its subjects outweighs that of non-state ethnic governance institutions. Where RSC is low, local elites can mobilize against the state and each other, frequently taking up arms in the process. Because transaction costs determine the level of local control attained by the state and its challengers, RSC is low in areas that are difficult to access for the state, but easy to access for local power holders.

8.2.3 *Observable implications*

In line with the argument, we approximate RSC with two accessibility-based metrics, both defined on the level of ethnic groups. The first is state access, which is the degree to which the state is able to access all members of a given ethnic group. Because in Africa, state power and resources are typically concentrated in the capital (Herbst, 2000), we define state access as the ease of travel from a country's capital to any given member of an ethnic group.[8] The second metric is ethnic groups' internal connectedness,

8 While we acknowledge the existence of less important outposts of the state, in particular military bases but also regional capitals, we focus on national capitals for three reasons. First, we lack systematic data on the location of states' military outposts outside of national capitals. Regional capitals are arguably less important for maintaining the monopoly of violence and not every regional capital hosts military units. Second, even if such information was available, employing it may not be advisable. Peripheral state agents may become challengers to the state themselves, as exemplified by frequent mutinies. Third, using the location of regional or military headquarters will amplify

the degree to which transport networks interconnect the members of a group. Because ethnic governance institutions are often internally fragmented and overlapping, we remain agnostic about the location(s) of political leadership within an ethnic group and do not define a single geographical 'ethnic power center.' We operationalize RSC as the difference between state access and internal connectedness. Groups that are difficult to reach by the state but internally highly interconnected exhibit low RSC. RSC is high in groups that are easily accessible from the capital but lack internal connectedness.

The theoretical argument and our empirical proxy give rise to three hypotheses on the effects of RSC on armed conflict in African states. The first concerns the presence of armed non-state actors, which we refer to as *challengers.* As discussed earlier, if the state exercises little control over an ethnic group that is internally well connected, challengers of state rule have an incentive and the opportunity to mobilize recruits and resources. We therefore expect that:

H 8.1: *There are more challengers in ethnic groups with low RSC.*

As outlined above, ethnic groups that are internally well connected rarely feature a single, all-powerful node of social control. Instead, patronage structures will often allow multiple power centers to exert social control in parallel. Aiming to build, defend, and extend their realm, competition between these peripheral actors often takes a violent turn if no superior central power, like the state, is capable of enforcing the peace. Hence, we expect that:

H 8.2: *Ethnic groups with low RSC experience more conflict* among *challengers.*

Most challengers will thus be found in ethnic groups over which the state has little control to begin with, cannot count on developing it anytime soon, and faces logistical difficulties to fight. Furthermore, lacking good access to the national capital, such peripheral challengers might not pose a vital risk to governments that are more concerned about rebellions in or near their capital (Roessler, 2011; Roessler and Ohls, 2018). For these reasons, governments might choose not to intervene, letting rebels and militias fight among themselves. However, in practice, this strategy of non-intervention might prove intolerably risky in the long run. As Kabila's march from the Eastern

endogeneity concerns, since these are (re-)located according to strategic concerns (see Chapter 5). In contrast, most capital cities in Africa have been established during the early colonial period and have not, with few exceptions, been moved since independence.

DR Congo to overthrow Mobutu shows, rulers must fear that if challengers grow too powerful, they threaten the state's authority in other parts of the country and may take over the state as a whole. This fear may motivate governments to eventually intervene and militarily weaken their peripheral challengers in areas with low RSC:

H 8.3: *Ethnic groups with low RSC experience more conflict between challengers and the state.*

8.3 DATA AND EMPIRICAL STRATEGY

8.3.1 *Measuring relational state capacity on road networks*

As outlined above, we measure RSC as the difference between ethnic groups' accessibility from the state's capital and their internal connectedness. We operationalize state access as the inverse average travel time from the state capital to the members of a given ethnic group:

$$\textsf{state access}_g = \left(1 + \frac{1}{I_g}\sum_{i=1}^{I_g} d(p_i, C)\right)^{-1}, \tag{8.1}$$

where I_g enumerates the members of group g and $d(p_i, C)$ is the shortest-path travel time from the national capital C to the location p of individual i. We add a constant of one hour to avoid division-by-zero errors for groups with small territories.[9] Groups' internal connectedness is computed as the inverse average travel time between all group members:

$$\textsf{internal connectedness}_g = \left(\frac{1}{I_g^2}\sum_{i=1}^{I_g}\sum_{j=1}^{I_g} 1 + d(p_i, p_j)\right)^{-1}. \tag{8.2}$$

Finally, we approximate RSC by computing the ratio of state access to internal connectedness,

$$\textsf{RSC}_g = \frac{\textsf{state access}_g}{\textsf{internal connectedness}_g}. \tag{8.3}$$

9 Note that, because we log all measures for the empirical analyses, they are equivalent to $-1*$ transaction costs as defined in Chapter 7. For example, $log(\textsf{state access}_g) = -1 * log(1 + \frac{1}{I_g}\sum_{i=1}^{I_g} d(p_i, C))$.

To compute expressions 8.1 to 8.3 we require three types of data: Ethnic settlement patterns, spatial population distributions, and information on road networks, the primary type of transport infrastructure in Africa (Herbst, 2000, ch. 5). We obtain data on ethno-linguistic settlement areas from the Ethnologue data set (Global Mapping International and SIL International, 2015). We prefer the Ethnologue data over alternative sources like the Atlas Narodov Mira/GREG (Weidmann, Rød and Cederman, 2010) and Murdock's (1967) Ethnographic Atlas because of its extensive coverage. Still, we show robustness checks based on alternative data sets in Appendix H.2.6. For demographic information we rely on the data set provided by Goldewijk, Beusen and Janssen (2010), who estimate historical and contemporary gridded population data at high spatial resolution.

Finally, we measure all travel times on the road networks digitized from the Michelin map corpus (see Chapter 5 and Appendix B.2).[10] In our baseline analysis, we rely on the 1966 road data to maximize the temporal distance between the road-network "treatments" and conflict outcomes. This limits the potential for reverse causality bias.[11] Our instrumental variable analyses draws on road data from 1990 since endogeneity is addressed through the research design. Because the analysis requires computing the full connectivity matrix in each country, including large ones as the DR Congo, we must draw on road networks with a relatively coarse resolution of $.1667 \times .1667$ decimal degrees ($\approx$18.5 km at the equator).[12]

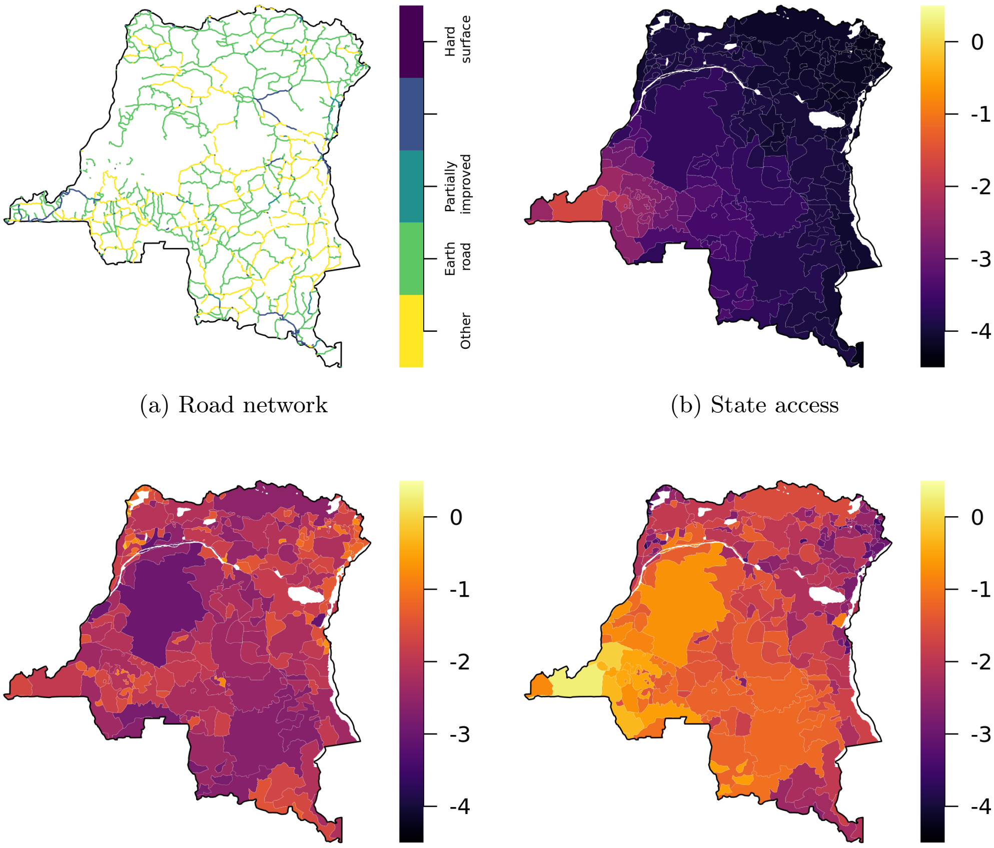
With these data, we compute all groups' state access, internal connectivity, and relational state capacity in 1966 and 1990.[13] Figure 8.1 illustrates the resulting estimates for the Democratic Republic of the Congo in 1966. Map 8.1a shows the road network. It is densest in the country's East and West. However, the sparsity of roads in the center curbs state access from the capital Kinshasa towards ethnic groups in the East and the North (see Map 8.1b). In contrast, the local road networks in the

10 Note that Philipp Hunziker and I have digitized an initial set of road maps for this research project, in particular those for the years 1966 and 1990. I have later digitized the remainder of the road maps used in this dissertation, using the fully convolutional neural networks Philipp Hunziker designed and the post-processing pipeline we have jointly developed.

11 The main danger of using contemporaneous road data is that roads in conflict areas decay. This creates reverse causality bias if conflicts that have begun before 1990 have destroyed the road network and caused conflict after 1990. Note however that the results barely change in a robustness check in which we deploy the 1990 road network data (Appendix H.2).

12 Because we rely on aggregates within ethnic groups, we gauge the loss in precision as negligible. Using a higher resolution results in an exponential increase of distances to compute, an increase that is not feasible with our computational infrastructure. Already in our current setup, a country such as the DR Congo requires computing and processing 6800^2 shortest paths.

13 We use population data from either 1960 or 1990, depending on the timing of the road network data.



(a) Road network (b) State access

(c) Internal connectedness (d) Relational state capacity

Figure 8.1: Measuring state access, internal connectedness, and relational state capacity in the Democratic Republic of the Congo 1966.
Figures (b)–(d) are based on Equations 8.1–8.3; plotted values are logged.

East result in high levels of internal connectedness of the respective ethnic groups. (see Map 8.1c). RSC, in turn, is high in the West, intermediate in the North, and very low in the East (Map 8.1d). These patterns highlight that, in contrast to local road densities, RSC accounts for the structure of the road network.

Throughout our empirical analyses, we employ a panel setup with group-*years* as units of analysis to account for moving borders and capitals.[14] To measure the three main outcomes of interest in Hypotheses 1–3, we employ the ACLED data set (1997-2016; Raleigh et al., 2010), which provides the most extensive collection of geo-coded event data for Africa. For each group-year we encode the number of:

1. *challengers*, defined as active rebel groups and ethnic or political militias,

14 Note the cross-sectional analysis in Appendix H.2.8.

2. *challenger events*, conflict events in which challengers fight against other challengers, and

3. *state events*, which denote conflict events in which the state forces fight with challengers

in a groups' settlement area. We analyze logged versions ($ln(y+1)$) of these count variables. In a series of robustness checks, we alter the coding of the outcome variables and use data from alternative sources (Appendix H.2).

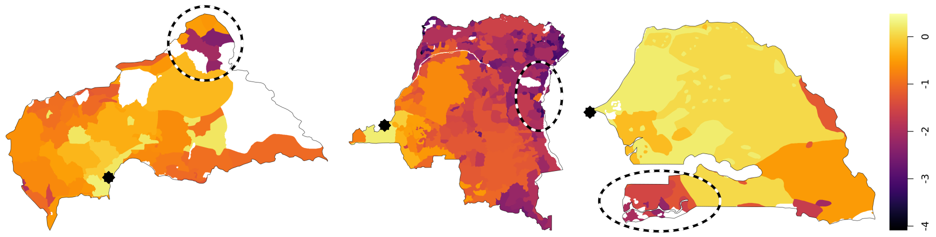
8.3.2 *Empirical Strategy*

In a first set of analyses, we test Hypotheses 8.1 to 8.3 by estimating the impact of RSC measured in 1966 on conflict-related outcomes in the 1997-2016 period. We choose this long temporal lag between treatment and outcome to minimize the risk of reverse causality, i.e, the risk of road networks reflecting anticipated future conflict. Note that we use the 1990 road-network data for our IV analysis and in a robustness check (Appendix H.2.3).

We estimate linear models with country-year fixed effects. This setup effectively controls for all phenomena that are constant within country-years. To mitigate the risk of omitted variables at the group-level, we add two sets of controls. First, we control for $\text{state access}_g^{foot}$ and $\text{internal connectedness}_g^{foot}$. These variables are analogous to the ones defined in Equations 8.1–8.2, but calculated exclusively on the 'footpath' network. Hence, they do not contain any road network information. These measures capture the impact of non-road related determinants of accessibility, such as population distributions, the relative location of the national capital, and the placement of borders. These controls allow us to empirically separate the impact of road networks from that of geographic and demographic features.

Second, we include a vector of geographic controls that may plausibly affect road construction and conflict risk: groups' distance to the closest border and the nearest coastline and navigable river,[15] a dummy for whether a group's settlement area includes the national capital, local climate (mean temperature, precipitation, evapotranspiration), and the altitude and roughness of a group's settlement area (from FAO, 2015). In light of Chapter's 5 result that local resources increased road investments,

15 Data on navigable rivers comes from Jedwab and Moradi (2016).



(a) Central African Republic (b) DR Congo (c) Senegal

Figure 8.2: RSC (logged) in three African countries, based on expression 8.3. We discuss the highlighted regions in the text.

I add groups' suitability for cash crop and general agriculture as well as a mineral deposit dummy.[16] We furthermore control for groups' contemporary total population and urban population (CIESIN et al., 2011) as well as the size of their settlement area (all logged, see e.g. Buhaug and Gates, 2002; Buhaug and Rød, 2006).[17] We add further control variables in a series of robustness checks summarized in Appendix H.2. To account for error dependence, we cluster standard errors at the ethnic group and country-year levels.

8.4 RESULTS

Before turning to the quantitative results, we discuss three cases that underline the face validity of our argument: the Central African Republic, the Democratic Republic of the Congo, and Senegal. Figure 8.2 depicts the distribution of RSC in these countries. Dark shades indicate ethnic groups with low levels of RSC, many of which were home to challengers to state power for years.

In the Central African Republic, RSC is lowest in the Northern region Vakaga from where Seleka rebels toppled the government in 2013. Cut off from the capital during the rainy season but internally well connected, Vakaga's ethnic groups are outside the reach of the government, and their inhabitants rarely use the national language and currency (International Crisis Group, 2007, 2015). In the DR Congo, we find that RSC is up to 350% higher among groups located in the West of the country compared

16 Note that this is a (empirically warranted) deviation from the original working paper. The inclusion of these two factors does not significantly alter the point estimates but slightly increases standard errors in some specifications. Data on general agricultural suitability comes from Ramankutty et al. (2002), on mineral deposits from Schulz and Briskey (2005), and the cash crop suitability score is constructed as in the previous chapters.

17 See Appendix H.2 for results from specifications without any control variables.

to those in the Eastern Kivu region. In line with our argument, civil wars have raged in the Kivus for years. Many areas are ruled and competed over by militias and rebel groups rather than the state (e.g. Sanchez de la Sierra, 2018). In nominally much stronger Senegal, the Casamance constitutes another area that features low levels of RSC and long-lasting conflict. As in the Eastern DR Congo, rebel groups in the Casamance have established governance structures that compete with the state and tax the local population – activities facilitated by their comparatively high internal accessibility and social control (Humphreys and ag Mohamed, 2005; Evans, 2004).

Turning to the statistical analysis, Table 8.1 presents estimates of the effect of ethnic groups' state access and internal connectedness on the three main outcomes.[18] The results align with our expectations. Higher levels of state access are associated with lower numbers of challengers (Model 1), less violence among them (Model 2), and less fighting between these challengers and government forces (Model 3). Conversely, internal connectedness is associated with more challengers and conflict events. Across the three models, the two coefficients are precisely estimated and have opposite effects. Indeed, the difference between the two estimates' absolute values is not significantly different from zero. This implies that we can substitute these two regressors with logged RSC (as defined in expression 8.3) without much loss of information.[19]

Table 8.2 summarizes the results obtained when using the combined RSC metric. Ethnic groups in areas with high levels of RSC feature fewer challengers, less conflict among them, and less fighting between challengers and the state. In substantive terms, the coefficients of RSC are sizeable, precisely estimated, and similar across the three outcomes. The models associate a 10% decrease of RSC with an increase in violent challengers and events by between 1.4 and 1.8 percent.[20] Returning to DR Congo, our estimates suggest that the 350% increase in RSC when moving from ethnic groups in the Kivus in the East to groups around Kinshasa in the West translates into a decrease in the number of challengers by 60 percent. Along the same route, the number of violent events is predicted to decrease by about 50 percent.

In Appendix H.2 we check whether these findings are robust to changes in the baseline model specification. Using dummies instead of counts as outcomes, using

18 Note that we log all RSC-related variables to account for decreasing marginal effects for high values of accessibility and connectedness.

19 Note that Equation 8.3 implies: $ln(RSC_g) = ln(\text{state access}_g) - ln(\text{internal connectedness}_g)$.

20 These percentage estimates are approximate due to the unit constant added to the outcome prior to taking the log. For low baseline values of the outcome, the actual effect is larger.

Table 8.1: Effect of the components of RSC, OLS

	Dependent variable (logged)		
	Challengers	Challenger Events	State Events
	(1)	(2)	(3)
β_1: State access '66 (log)	-0.157^{***}	-0.111^{**}	-0.138^{***}
	(0.038)	(0.044)	(0.046)
β_2: Internal connectedness '66 (log)	0.186^{***}	0.161^{***}	0.133^{***}
	(0.036)	(0.041)	(0.035)
State access; foot '66 (log)	0.036	−0.006	0.011
	(0.029)	(0.035)	(0.033)
Internal connectedness '66; foot (log)	-0.130^{***}	-0.117^{***}	-0.103^{***}
	(0.030)	(0.034)	(0.030)
$\beta_1 + \beta_2$	0.03	0.05	0
	(0.04)	(0.05)	(0.04)
Country-year FE:	yes	yes	yes
Controls:	yes	yes	yes
Mean DV	0.21	0.17	0.15
F-Stat:	22.83	20.33	15.99
Observations	31,760	31,760	31,760
Adjusted R^2	0.399	0.370	0.313

Notes: OLS models. Control variables consist of the total and urban population (log), groups' area (log), the mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of a group's settlement area, its cash crop and agricultural suitability, a mineral deposit dummy, as well as groups' logged distance to the coast, navigable river, and border. Two-way clustered standard errors in parentheses (ethnic group and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01.

fatalities instead of events, and sourcing the outcome variables from alternative data sets (UCDP GED, Sundberg and Melander, 2013, and SCAD, Salehyan et al., 2012) yields substantively equivalent results (H.2.1). The same applies when using logistic or negative binomial models instead of linear specifications (H.2.2). As already mentioned, the results hold when we employ the 1990 road network data (H.2.3). They are also robust to either dropping all control variables or expanding them, including precolonial and geographic characteristics of ethnic settlement areas (H.2.4). Adding fixed effects for 10'000 'bins' of ethnic groups of the same country and a similar geographic distance to the capital, distance between inhabitants, and population size produces equally consistent results (H.2.4). Dropping very small ethnic groups (H.2.5), using different ethnic settlement data as units of analysis (H.2.6), or conducting a country-by-country jackknife does not significantly change the results either (H.2.7).

Table 8.2: Relational state capacity and violence in Africa 1997–2016: Main Results, OLS

	Dependent variable (logged)		
	Challengers	Challenger Events	State Events
	(1)	(2)	(3)
RSC 1966 (log)	−0.174***	−0.141***	−0.135***
	(0.032)	(0.036)	(0.033)
State access 1966; foot (log)	0.048*	0.015	0.009
	(0.026)	(0.031)	(0.027)
Internal connectedness 1966; foot (log)	−0.122***	−0.103***	−0.104***
	(0.029)	(0.032)	(0.030)
Country-year FE:	yes	yes	yes
Controls:	yes	yes	yes
Mean DV	0.21	0.17	0.15
F-Stat:	22.84	20.33	16.01
Observations	31,760	31,760	31,760
Adjusted R^2	0.398	0.370	0.313

Notes: OLS models. Control variables consist of the total and urban population (log), groups' area (log), the mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of a group's settlement area, its cash crop and agricultural suitability, a mineral deposit dummy, as well as groups' logged distance to the coast, navigable river, and border. Two-way clustered standard errors in parentheses (ethnic group and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01.

Finally, cross-sectional analyses show that the results are not due to potentially endogenous movements of borders and capitals since independence (H.2.8).

Beyond the caveats addressed above, a causal mechanism other than states' and challengers' social control may explain the relationship between our road-based proxy of RSC and conflict. We test two such alternative explanations in Appendix H.2.9. The first alternative is that roads connect markets, foster growth and economic development, which can bring peace and stability. The second explanation is that roads to the capital capture generally higher levels of connectivity of an ethnic group with the entirety of its country. The resulting strength of inter-ethnic ties might curb the risk of rebellion. We find that controlling for these two alternative mechanisms barely reduces the size of the estimated effect of RSC.

8.5 AN INSTRUMENTAL VARIABLE APPROACH: SIMULATED ROAD NETWORKS

The potential endogeneity of road networks is the most important caveat of the above analysis. In particular, it is plausible that there are unmeasured group-level characteristics that have affected colonial road building and recent conflict. Indeed and as highlighted in Chapter 5, African road builders frequently followed their extractive preferences and administrative concerns.[21] If, for example, unobserved resource endowments caused ethnic groups to have better connections to capitals but increase the risk of conflict (e.g. Lujala, Gleditsch and Gilmore, 2005; Berman et al., 2017), we would underestimate the effect of RSC. In contrast, ethnic favoritism can lead governments to build roads towards their ethnic kin (Burgess et al., 2015) but not powerless ethnic groups, which are ex ante more likely to rebel (e.g. Cederman, Gleditsch and Buhaug, 2013). This biases the estimated effect of RSC downwards. To address such endogeneity, we implement an instrumental variable (IV) approach based on simulated road networks.

8.5.1 *An instrument for road networks*

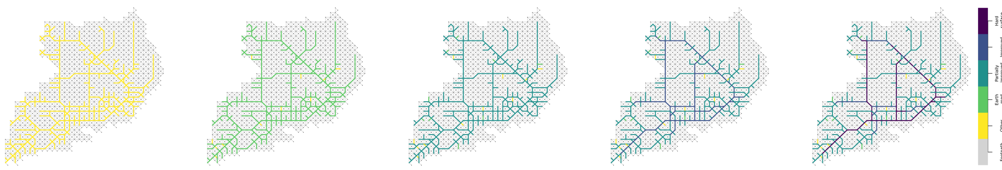
We construct an instrument for our observed RSC metric in two steps. First, for each country in our sample, we construct an "optimal" road network using only information that is plausibly exogenous to contemporary conflict. Building on the literature on optimal transport networks (e.g. Los and Lardinois, 1982) our road-building algorithm only takes a country's population distribution in 1880 (from Goldewijk, Beusen and Janssen, 2010) and its total type-specific (surfaced roads, ..., tracks) road mileage in 1966 as inputs. It then constructs a road network that minimizes the average travel time between any two inhabitants of the country. For details on this procedure, see Box 2 and Appendix H.3. In a second step, we use these simulated networks to recompute the state access and internal connectedness metrics introduced above. These new variables serve as instruments for our observed measure of RSC.

21 Another potential source of endogeneity is systematic bias in the Michelin maps' accuracy.

Box 2: Simulating road networks

We simulate simplified, yet realistic road networks with a minimal amount of information on (1) the shape of countries, (2) their population distribution in 1880 (Goldewijk, Beusen and Janssen, 2010), and (3) the total budget of roads per road-type to be built. With this, the algorithm seeks to 'build' a road network that minimizes the average distance between any two inhabitants of the country. Intuitively, the solution requires roads of good quality between densely inhabited areas, and roads of worse quality towards sparsely inhabited peripheries. Yet, despite this simple intuition, the problem of where to build which road is NP-hard. With the high number of possible configurations of roads, we rely on a heuristic approach.

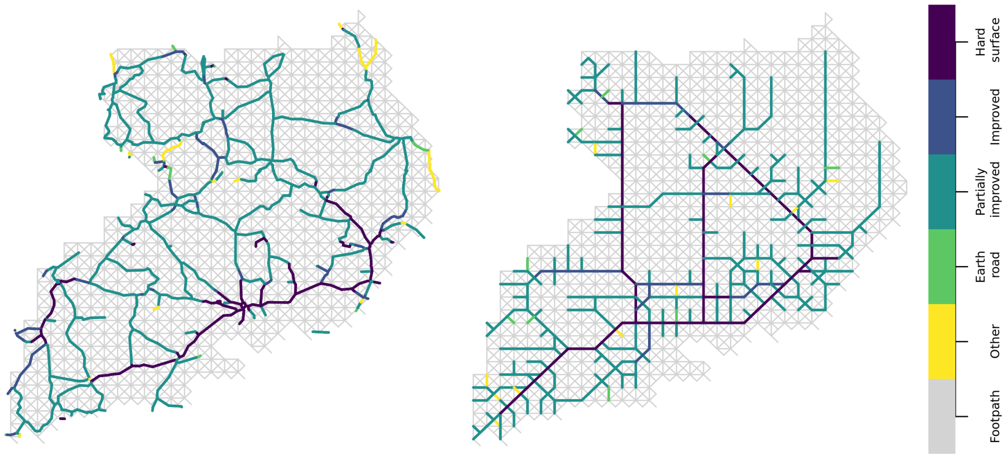
The heuristic we follow is relatively straightforward and explained in more technical terms in Appendix H.3. We initiate road building at 10 places drawn from the regular network of a country's footpaths (see Chapter 5) with a probability relative to their population. From these places, the algorithm starts upgrading footpaths to roads of the worst type ('marked tracks'), always building the road that most increases the overall connectedness in the country. Once it has built the total budget of tracks and all superior roads (see Figure B1.1), it proceeds to improving the network of tracks, with the next better type of roads, earth roads. It again builds one earth road after the other, always building the one that most improves the overall connectedness of the country (B1.2). We continue in the same manner until we have exhausted the budget (B1.5).



(1) (2) (3) (4) (5)

Figure B1: Simulation of the Ugandan road network in 1966

This approach varies from other strategies to simulate transport networks as part of an IV-strategy, which start with a fixed and low number of nodes in a network. These nodes are either start and end points of single (rail)roads as in Jedwab, Kerby and Moradi (2017), or nodes of a complete network (Faber, 2014). The edges of the transport network are then simulated by taking the least-cost path over the physical terrain. Faber then trims the resulting network to its minimum spanning tree. These techniques result in spatially highly disaggregated predictions. However, their disadvantage consists in the necessary assumption that the nodes of the networks are exogenous to what lies in between them. Given that roads are not only built to connect their start- and end-points, but also to connect points on the way, this assumption may be problematic. In addition, the Michelin road maps feature a large number of nodes, many of which might be endogenous to conflict processes. We therefore aim to simulate the entire network structure and not only the mere presence of roads, without fixing the set of nodes in the network.



(a) Observed network

(b) Simulated network

Figure 8.3: Observed and simulated road networks in Uganda, 1966. For further examples, see Appendix H.3.

For this IV strategy to be effective, two conditions need to be met. First, the instruments must be predictive of the endogenous treatment, which requires the simulated road networks to be sufficiently similar to their observed counterparts. Indeed, we find that this is the case. While real-world road building is obviously more complex than minimizing inter-citizen travel times, our simulated networks are fairly realistic, the reason being that some network configurations are more effective at interconnecting a country's population than others. Taking Uganda as an example, Figure 8.3 shows that the algorithm places the highest-quality roads between the most populated areas in the Southwest, center, and Southeast of the country. As in the observed network, the algorithm also produces trunk- and feeder-roads that facilitate travel towards the adjoining areas of a main road. This coincides with the evidence reported below, showing that our simulated RSC instruments are predictive of observed RSC.

Second, our instruments have to satisfy the exclusion restriction, i.e. they need to be exogenous to contemporary conflict, and may not affect conflict through any other channel than observed state access and internal connectedness. Exogeneity requires that the inputs to the simulation algorithm – population distributions, country borders, and total road mileage – are themselves exogenous. We ensure this is the case by (i) using historical data, i.e. 1880 population and 1966 road mileages, and (ii) by including country-fixed effects in the IV regression. The latter measure eliminates any potential endogeneity of population counts or road mileage on the country-level, and ensures that all identifying variance originates from groups' relative location vis-à-vis the country's borders and other inhabitants. Another threat to the exclusion restriction arises if our instruments simply pick up group-level population counts or other demographic/geographic information. This is particularly important since the estimated 1880 population data used for simulating the road networks is based on postcolonial population data that might be biased by conflict dynamics. As a solution, we include three sets of controls in the IV regression. The first set comprises 'footpath' versions of the state access and internal connectedness variables, now based on the 1880 population distribution and the simulated networks. The second set consists of the inputs to the simulation: ethnic groups' population count in 1880, and their distance to the country-border. The third set is the vector of geographic controls from our baseline analysis.

In sum, our IV design exploits the fact that some groups feature low RSC because building straight roads to access them from the capital is inefficient, but building roads that make them internally connected pays off. The design thus builds directly on Herbst's (2000, ch. 5) observation that some African countries' geographies are ill-suited for constructing road networks that benefit local state rule.

8.5.2 *Results*

We implement our IV strategy using 2SLS models. Note that, unlike in the baseline analyses above, we now measure observed RSC using our 1990 roads data. Assuming that our instrument is valid, we no longer need to rely on historical road network data. Since road networks and population distributions are highly persistent, our instruments (simulated with 1966 road mileages)[22] are still highly predictive of the 1990 state access and internal connectedness measures. The IV results strongly support the predictions of Hypotheses 8.1 and 8.2 that low levels of RSC facilitate the emergence of challengers to state power and leads to violent competition between them. The results show more mixed support for Hypothesis 8.3, stating that these dynamics also lead to increased violence between challengers and state forces. While such fighting decreases with instrumented RSC, this result is not stable across the robustness checks

22 Note that we conduct a robustness check with road neworks simulated on the basis of countries road stock in 1990, see Appendix H.2.3.

and mainly driven by the state access component of RSC. We discuss the implications of this finding after presenting the results.

Table 8.3: Instrumental Variable Approach: Reduced Form

	Dependent variable (logged)		
	Challengers	Challenger Events	State Events
	(1)	(2)	(3)
State access 1880 (sim; log)	−0.221***	−0.155***	−0.090*
	(0.046)	(0.052)	(0.047)
Internal connectedness 1880 (sim; log)	0.123***	0.098***	0.023
	(0.028)	(0.030)	(0.028)
State access 1880; foot (log)	0.062**	0.012	−0.029
	(0.030)	(0.036)	(0.032)
Internal connectedness 1880; foot (log)	−0.054**	−0.032	−0.002
	(0.027)	(0.030)	(0.026)
Country-year FE:	yes	yes	yes
Controls:	yes	yes	yes
Mean DV	0.21	0.17	0.15
F-Stat:	21.87	19.63	15.5
Observations	31,280	31,280	31,280
Adjusted R^2	0.391	0.364	0.309

Notes: OLS models. Control variables consist of the total and urban population (log), groups' area (log), the mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of a group's settlement area, its cash crop and agricultural suitability, a mineral deposit dummy, as well as groups' logged distance to the coast, navigable river, and border. Two-way clustered standard errors in parentheses (ethnic group and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01.

Table 8.3 presents the reduced form estimates. As expected, states' access to ethnic groups on the simulated network relates negatively and statistically significantly to the number of challengers, fighting between them, and battles between challengers and state forces ($p = .057$). Conversely, the simulated internal connectedness of ethnic groups has a positive effect on the number of challengers and fighting between them. However, the measure does not have a significant or large effect on battles between challengers and the state. With the exception of violence between the state and its challengers, these effects are precisely estimated and coincide with our baseline results. In particular the consistent negative effect of state access underlines the importance of Herbst's (2000) work for understanding contemporary political development and conflict in Africa: Violent competition over local power is more prevalent in ethnic groups in peripheries that cannot be easily penetrated by the state.

Table 8.4 presents the main 2SLS models. The explanatory power of our two instruments in the first stage is high, yielding a F-statistic of 62.[23] As expected, state access^{sim} has a positive effect on RSC, while internal connectedness^{sim} impacts the measure negatively. Both coefficients are statistically significant.

Table 8.4: Relational state capacity and violence in Africa 1997–2016: Main Results, 2SLS

	Dependent variable (logged)			
	Stage 1	Stage 2		
	RSC 1990	Challengers	Challenger events	State events
	(1)	(2)	(3)	(4)
State access 1880 (sim; log)	0.645***			
	(0.063)			
Internal connectedness 1880 (sim; log)	−0.262***			
	(0.032)			
RSC 1990 (log)		−0.370***	−0.269***	−0.128*
		(0.077)	(0.081)	(0.070)
State access 1880; foot (log)	0.261***	0.168***	0.092*	0.0002
	(0.040)	(0.051)	(0.056)	(0.048)
Internal connectedness 1880; foot (log)	−0.210***	−0.113***	−0.069*	−0.037
	(0.030)	(0.036)	(0.039)	(0.032)
Country-year FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV	-1.11	0.21	0.17	0.15
F-Stat:	267.09	21.18	19.38	15.49
F-Stat Stage 1:		61.99	61.99	61.99
Observations	31,280	31,280	31,280	31,280
Adjusted R^2	0.891	0.371	0.356	0.307

Notes: 2SLS-IV models. Control variables consist of the total population in 1880 (log), groups' area (log), the mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, and the mean altitude and slope of a group's settlement area, its cash crop and agricultural suitability, a mineral deposit dummy, as well as groups' logged distance to the coast, navigable river, and border. Two-way clustered standard errors in parentheses (ethnic group and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01.

The second stage shows substantive effects of RSC on the number of challengers, violence among them, and violence between them and the state. The coefficients for instrumented RSC in 1990 are statistically significant and of greater absolute size

23 Specifically, we can reject the weak instrument null under a maximum size of 10% of a 5% Wald test of $\beta_{\mathsf{RSC}} = 0$ (Stock and Yogo, 2002, p. 101). The first stage holds across different size-bins of ethnic groups and countries (see Appendix H.4.1). Because our simulations are imprecise in small areas, the instruments are weak for small ethnic groups and have no predictive power in the smallest countries in the sample.

than the baseline estimates for the number of challengers and violence between them. In substantive terms, the IV-estimates suggest that a decreasing RSC in 1990 by 10 percent increases the number of local challengers by 3.7 and violent events between them by 2.7 percent. The same decrease in RSC raises the number of battles between challengers and the state by 1.3 percent, an estimate that is similar in size than the baseline result but associated with greater uncertainty ($p = .066$).[24] As highlighted by the reduced form estimates in Table 8.3 and confirmed by estimating the effects of state access and internal connectedness instrumented separately (Table H.6 in the Appendix), the effect of RSC on state-challenger battles is mainly driven by states' access to ethnic groups. The estimated effect of groups' internal connectedness is half the size and associated with a large standard error.

To assess the robustness of the IV analysis, we implement the same robustness checks as for the uninstrumented analysis and discuss the results in detail in Appendix H.2. The estimated effect of RSC on the number of challengers and violence between them is robust across all alternative specifications, thus providing further support to Hypotheses 8.1 and 8.2. The more imprecisely estimated effect of the instrumented measure of RSC on fighting between states and their challengers generally coincides with the effect reported in Table 8.4, although with a few exceptions. These exceptions pertain to (1) the effect of RSC on the number of fatalities in battles between the state and its challengers, (2) the number of such battles as measured by UCDP GED, and (3) the number of state-challenger battled in ethnic settlement areas from GREG (Weidmann, Rød and Cederman, 2010). In these three specifications, the estimated effect of RSC is smaller than in the baseline models and statistically insignificant. They turn furthermore insignificant when we analyze the data in a cross-sectional manner, although here, the point estimates are indistinguishable from the baseline analysis. Furthermore, the effect of RSC on state-challenger fighting is mainly driven by observations from the DR Congo. Albeit being the proto-typical case in which we expect Hypothesis 8.3 to hold, this casts further doubt on its validity. The results are consistent with those reported above in all other specifications.

The results thus show consistent and strong support for Hypotheses 8.1 and 8.2 that low levels of RSC allow rebels and militias to mobilize and compete violently over local power. We find more mixed support for Hypothesis 8.3 that states fight

24 Again, these percentage estimates are approximate due to the unit constant added to the outcome prior to taking the log. For low baseline values of the outcome, the actual effect is larger.

their challengers more often in ethnic groups with low levels of RSC. In light of the consistent findings on the number of and fighting between challengers, this raises two questions in particular. The first concerns whether our geographic counting of state-challenger battles accurately captures conflict dynamics between the center and the periphery. The second relates to potentially heterogeneous incentives of governments to confront peripheral rebellions.

First, while fighting among challengers to the state is most likely to occur in the ethnic settlement areas they compete over, this is not necessarily the case for fighting between rebels and the state. As the example of Kabila's march on Kinshasa illustrates, peripheral challengers that grow strong and attempt to conquer the state will battle with its forces not only in the settlement area of 'their' ethnic group, but also elsewhere, in particular on their way to the capital. Such spatial patterns are not captured by our measurement. Instead, we attribute the respective battle events to ethnic groups from which the challenge did not originate.

Second, there might be substantial heterogeneity in the circumstances under which states invest the resources to fight their peripheral enemies. As noted above, some rulers might choose to not fight them at all, due to their inability to control the respective ethnic groups in the long run, logistical difficulties to stage a successful military campaign, or presuming that the rebels do not threaten their survival. In other cases, the threat of peripheral challengers will pose too much of a risk to governments' survival, thus motivating them to take up arms and defend their rule. These heterogeneous incentives to repress peripheral rebellions highlight important questions for future research on the effects of relational state capacity on the geography of civil wars.

8.6 CONCLUSION

The civil war literature correctly highlights state weakness as a central conflict determinant. Yet, past research on the effect of state capacity on conflict has relied on a set of incomplete theoretical arguments and empirical measurements. While shifting the theoretical focus from the rebel side to structural properties of the state, Fearon and Laitin's (2003) influential formulation leaves the interaction between governments and non-state actors mostly implicit. Subsequent work has either proposed better country-level proxies for state capacity or turned to the local level, without fully capturing the state-society nexus that is at the heart of conflict processes.

In order to address these issues, this chapter advocates a relational view on state capacity and conflict that highlights the importance of transaction costs between political actors and society. Building on Mann (1984, 1993) and Migdal (1988), we argue that state weakness cannot be understood without considering it directly in relation to the social control maintained by non-state actors that compete for power both among themselves and at the expense of the state. The crucial realization is that road networks do not merely serve to increase the capacity of central governments, but also constitute the backbone of challengers' attempts to exert control. For each kilometer of roads paved, and by extension, for each radio station, cell phone tower, or internet cable built, the state may provide the tools with which its competitors can outgovern it.

We have found empirical support for this dual perspective on governance. Focusing on ethnic groups in Africa, we have measured relational state capacity (RSC) as the difference between groups' accessibility from the capital and their internal connectedness. Our baseline analysis suggests that RSC decreases the number of challengers to state power, the risk of political violence among them, and between them and agents of the state. We address potential endogeneity biases through an instrumental variable approach that exploits credibly exogenous variation in road networks, which we capture with a novel network simulation strategy. The results bolster the baseline estimates, with the exception of the effect of RSC on the number of battles fought between the state and its challengers, which is positive in the main specification but not robust in a number of sensitivity analyses.

There are good reasons to believe that our results generalize beyond Africa. For example, Scott (2009) highlights similar dynamics in his work on how state penetration in Southeast Asia forced peripheral ethnic groups to flee into inaccessible terrain. This process turned into conflict when states tentacles impeded future escape and ethnic groups mobilized against the state. Historically, Hechter's (1975) account of "internal colonialism" and Weber's (1977) analysis of French nation building illustrate the pivotal role of road building in extending the state's social control into its entire territory. The problem of dual technologies of social control is also not limited to road networks. In recent work on civilian targeting in Syria, Gohdes (2019) highlights that the Internet presents rulers with a very similar dilemma than we have emphasized here: it provides information that improves their ability to target rebels, but at the same time allows them to organize and mobilize.

Moving forward, our theory of conflict needs to be extended to encompass the motives of the state and its challengers. Under what conditions will states choose to fight their peripheral challengers? And under what conditions does extending RSC, necessarily at the expense of local actors, affects their readiness to take up arms in defense of their realm? This question is of particular importance, since extending states' social control does not necessarily imply inclusive rule and good governance, but may instead lead to repression and 'internal colonialism' (Hechter, 1975). In that regard, the different strands of the literature on the motivations for armed conflict can offer a good starting point to explore how RSC can be increased without causing violent backlash.

9

EXCURSUS: WHAT IF COLONIAL BORDERS HAD BEEN DRAWN DIFFERENTLY?

The fact that the colonial partition of Africa along arbitrary borders has left some countries with territories that are difficult to govern has become prominent through the work of Herbst (2000) and has left its bearings on the theory and empirical chapters of this dissertation. While I have presented evidence that states' geographies and their efforts to reduce transaction costs with their citizens affect development and conflict, questions about the effects of colonial borders remain unanswered in my within-country analyses.

Herbst (2000) and Thies (2009) convincingly argue that countries with densely populated areas far away from their capital are associated with more violence and lower levels of development. However, absent a realistic counterfactual we cannot know by how much human welfare and peace would have improved under a set of 'better' borders. How much of the effect of "difficult geographies" origins in population distributions that make parts of the African continent inherently difficult to govern? And how much of their effect can be attributed to colonial borders that made things worse for the states built within them?

To answer these questions, I simulate a series of counterfactual borders and capitals that would have improved states' reach. Combined with the results from the previous two Chapters 7 and 8, I then estimate differences in the expected levels of development and conflict between the simulated and observed national geographies. This counterfactual exercise contributes to the literature on the effects of African borders that has so far mainly concentrated on the impact of borders that split ethnic groups or are drawn as straight lines on a green table (Alesina, Easterly and Matuszeski, 2011; Englebert, 2000; Englebert, Tarango and Carter, 2002; Michalopoulos and Papaioannou, 2016).

My objective here is strictly limited to evaluating the claim that the distance colonial borders and capitals put between governments and their people curbed development and peace. In order to enable a valid comparison, I neglect factors other than geographic distances and population distributions. It is important to note that I do not assess the impacts of changing borders now and do not intend to inform respective policies. States and societies in Africa have adapted to the current set of borders. Changing them haphazardly would likely have catastrophic repercussions far beyond the scope of the following analysis.

SIMULATING 'OPTIMAL' ADMINISTRATIVE GEOGRAPHIES

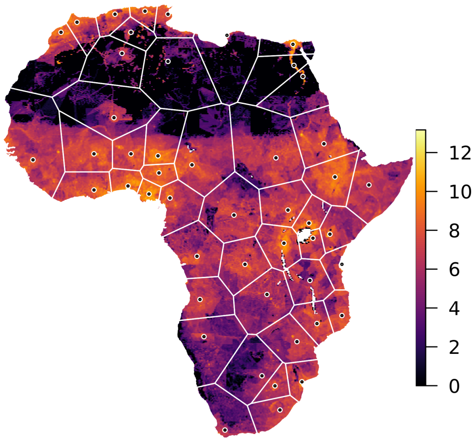
The distribution of government authority is "one of the oldest and most abiding problems of society" (Fesler, 1949, p.1). It is, in essence, a problem of clustering individuals distributed in space to achieve a set of objectives. Here, I aim to maximize the geographic reach of counterfactual states. Given the population distribution in Africa and a fix number of 48 countries, I search for the set of boundaries and capitals that minimizes the average distance between rulers and citizens.

This formulation captures the same objective function as a weighted k-means algorithm (Lloyd, 1982), which clusters observations by minimizing the distance between observations and the center of the cluster they belong to.[1] Given this equivalence, I adapt the k-means algorithm to cluster grid-cells on a $.1667 \times .1667$ decimal degree ($\approx$18.9 km) raster into governance units, taking into account cells' population estimate in 1880.[2] The population estimate from 1880 (Goldewijk, Beusen and Janssen, 2010) mitigates the effects of real capitals and borders on local demographic changes after the Scramble for Africa as far as possible. Note however, that post-1880 population data still affects the 1880 estimates since these are influenced by postcolonial census counts. Given that populations move towards capitals (Ades and Glaeser, 1995), this bias makes the simulations more conservative, that is closer to the observed distribution of capitals and borders. Because the weighted k-means algorithm is initiated at random points and therefore produces varying results, I simulate 100 sets of simulated national geographies for the continent.

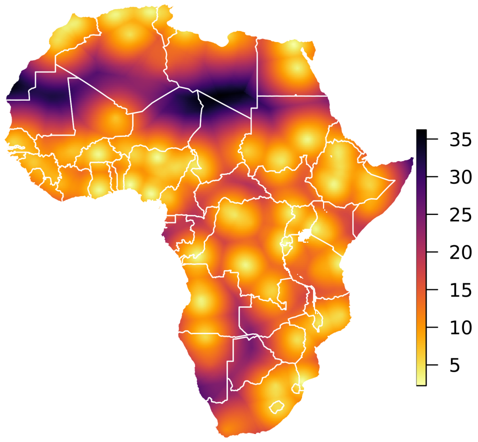
The results of the 100 simulations are plotted in Figure 9.1. First, Subfigure 9.1a maps the African population distribution in 1880 and one set of simulated capitals and

1 The problem is also closely related to a Hotelling-Downs problem (Hotelling, 1929; Downs, 1957; Davis, Hinich and Ordeshook, 1970) of locating businesses or parties in a two dimensional, irregularly populated space they compete over.

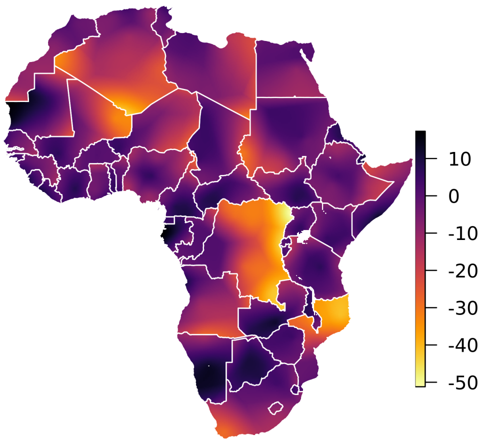
2 For details on the algorithmic procedure, see Appendix I.1. The algorithm is the weighted version of the procedure used to generate the Voronoi cells used as units of observation in Chapters 5 and 7.



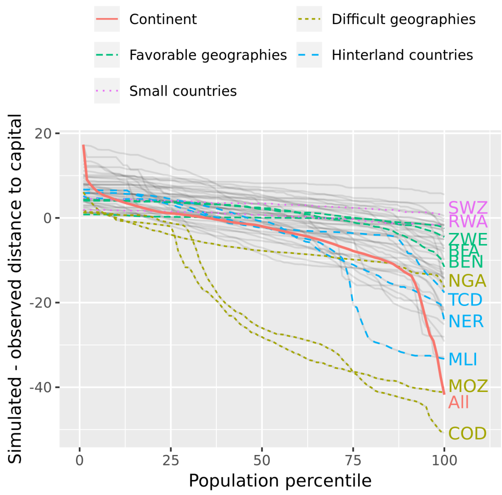
(a) Set of 48 counterfactual states and population in 1880 (log)



(b) Average travel times to capitals across 100 simulations



(c) Difference between observed travel times and average travel times across 100 simulations



(d) Difference between observed travel times and average travel times across 100 simulations, by country and population quantiles

Figure 9.1: Counterfactual simulations of national geographies and simulated difference in travel times.
Note: Lighter colors identify better outcomes. Simulations create 48 counterfactual countries based on the continents' population distribution in 1880.

borders. As one would intuitively expect, we see more simulated capitals in densely populated areas such as the Great Lakes region in East Africa. Just as in the real world, unpopulated areas such as the Sahara are at the periphery of large states (Englebert, Tarango and Carter, 2002). Next, I average the geographic distance of each point towards its 100 simulated capitals. On the basis of this average geodesic distance, I estimate each point's average travel time to its simulated capitals. This estimation draws on the results of a non-parametric LOESS estimate of the association between as-the-crow-fly distances and observed travel times to the actual set of capitals.[3] The resulting Map 9.1b shows that the simulations put capitals farther away from areas with low population densities than from densely populated regions.

The resulting differences between simulate and observed travel times to their capitals is mapped in Subfigure 9.1c. Just as Herbst (2000) argues, populated but peripheral areas such as the Eastern DR Congo or Northern Mozambique are much closer to their simulated than their observed capitals. It is in these places where suboptimal colonial borders had the largest effects. However, colonial national geographies put some regions closer to their observed than their simulated capitals. Subfigure 9.1d highlights such variation. It plots the difference between travel times to observed and simulated capitals in each population percentile. The main red line shows that the median inhabitant of the continent is only 2 hours closer to its simulated than observed capital city. Under the simulations, about 30 percent of the African population ends up farther away from their capital. However, on the right side of the distribution, we see substantial improvements. The 75$^{\text{th}}$ percentile would have gained 8 hours, the 90$^{\text{th}}$ percentile 13 hours. Once we zoom into single countries, we see that countries with difficult geographies would have profited the most from 'better' borders, while small states of the likes of Rwanda and Swaziland would have fared worse. In between, we find states with favorable geographies and the Sahel states that feature large hinterlands.

COUNTERFACTUAL RESULTS

We can now combine the results from the counterfactual simulations with the estimates of the effects of travel times to national capitals on local development and conflict

3 See Appendix I.2 for a visualization. Note that I assume that the simulated states would have built road networks in a similar manner than the observed states.

Figure 9.2: Counterfactual changes of developmental outcomes, by population quantile.
Note: Based on the results from the panel analysis in Chapter 7, Table 7.5. For counterfactual improvements based on the cross-sectional models from the same chapter, see Figure I.3 in the Appendix.

from Chapters 7 and 8. To do so, I multiply each unit's[4] difference between the logged travel times to its observed and simulated capitals with the corresponding coefficients from the respective models.[5] This produces continent-wide estimates of the change in development and conflict obtained by the simulated national borders and capitals. It is important to note that the econometric models used for this exercised are based on within-country variation. They do therefore not reflect any general equilibrium effects of countries' geographies.

Figure 9.2 provides an overview over the simulated deterioration and improvement of development outcomes across the percentiles of the African population. Since the simulations do not lead to large average improvements in travel times to capitals, average increases in education and infant survival rates as well as nightlight emissions are modest. For primary education rates, they amount to 0.64 [0.84, 0.44] percentage points. In parallel to the distribution of changes in travel times, parts of the population would see decreases in their developmental outcomes, while a larger share would see – in particular in the right tail of the distribution – more substantial increases. For

4 For models of survey outcomes (primary education, infant mortality, household wealth) units are grid cells. For nightlights, I draw on the respective Voronoi polygons from Chapter 7. For conflict outcomes, I return to the ethnic polygons used in Chatper 8.

5 For the main discussion, I draw on the panel analysis from Table 7.5 for assessing developmental effects, and the 2SLS-IV models from Table 8.4 for assessing conflict outcomes. See Appendix I.2 for results with the other main specifications from the respective Chapters.

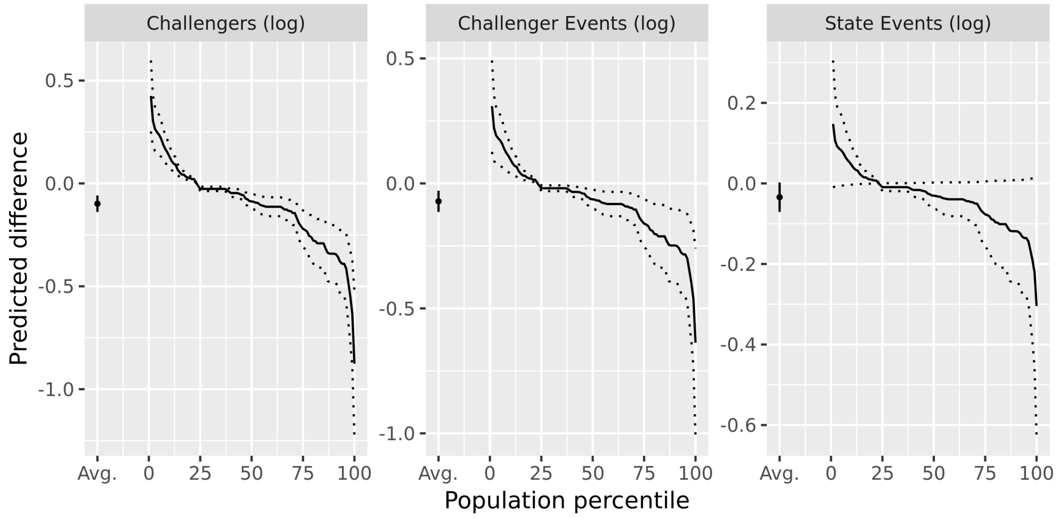


Figure 9.3: Counterfactual changes in conflict incidence in ethnic groups, by population quantile.
Note: Based on the results from the instrumental variables analysis in Chapter 8, Table 8.4. For counterfactual improvements based on the cross-sectional models from the same chapter, see Figure I.4 in the Appendix.

example, the primary education rates of the 90^{th} percentile are predicted to increase by 2.9 [3.8, 2] percentage points. Compared to the distribution of developmental outcomes today, the decreases in development are clustered in relatively wealthy populations that live in small countries and around today's capitals. In contrast, the increases in the right tail of the distribution are concentrated in populations with the lowest levels of development on the continent, such as people living in the Eastern DR Congo or Northern Mozambique (see Appendix I.3). While the average effect of better borders on development might not be great, this correlation between simulated improvements and low levels of observed development suggests that colonial borders are indeed responsible for pockets of poverty in densely populated regions far away from their national capitals.

The estimated differences in the intensity of conflict under the simulated and observed geographies of states give rise to similar conclusions. Figure 9.3 plots the predicted difference in the number of violent challengers to the state, conflict among them and between them and the state. Again, the median inhabitant of the continent is predicted to experience lower levels of conflict in the simulations, with the number of challengers decreasing approximately 8.1 [11, 4.9] percent. The effect in the parts of the ethnic landscape that has the highest simulated improvements is much more sizable – in the 90^{th} percentile, the simulation predicts ethnic groups to have about 25 [32, 17] percent less challengers. The simulated improvements in the number of battles among challengers, and between them and the state follow similar patterns. In addition, there is a strong correlation between observed levels of violence and greater simulated improvements. This is further support for Herbst's (2000) argument that subobtimally drawn national borders have contributed to excessive levels of violence in peripheral, but populated subnational regions across the continent.

CONCLUSION

This short excursus has shed light on the prominent hypothesis that the location of colonial borders and capitals created states with "difficult geographies," curtailed development, and fostered instability in parts of Africa (Herbst, 2000). I have combined counterfactual simulations of 'improved' national designs with the results from the analyses of the effects of travel times to national capitals on development in conflict in Chapters 7 and 8. The resulting patterns of differences in travel times to capitals, development, and conflict give rise to three main conclusions. First, the average African would not have benefited much from better borders and capitals. However and second, there are significant parts of the continent's population that live in populated areas far away from today's capitals. The prospects of peace and development of these regions would have greatly improved under a set of borders that placed borders and capitals in better accord with the continents' population distribution. Third, such 'better' national designs are far from pareto efficient, since they would have put some regions that today are close to their capitals farther away from them.

While the results support Herbst's hypothesis that colonial borders in some cases produced states with difficult geographies incapable of servicing and policing their peripheries, the design of the exercise has clear limitations. For one, I treat the continents' population as a homogenous block, disregarding the important interplay between ethnic identities and the geographic make-up of states (Alesina, Easterly and Matuszeski, 2011; Englebert, 2000; Englebert, Tarango and Carter, 2002; Michalopoulos and Papaioannou, 2016).[6] In addition, the analysis is based on local, within-country

6 I have also neglected geographic impediments to states' reach such as mountain ranges and rivers, as well as other potentially important factors such as (anticipated) transport infrastructure, or the location of historical states and their capitals.

effects of distances to capitals. It thus fails to capture potential general equilibrium effects of national geographies that affect development and conflict throughout the territory of a state. Lastly, the use of back-projected population data, though the best available to date, might decrease the estimated improvements in development and conflict. Together, these caveat and the small average improvements in the prospects of development and peace under a set of simulated borders only underline the initial note of caution that I am not intending to give any policy advise here. Redrawing the African map would have potentially catastrophic repercussions (Joseph, 1997) that are outside the scope of this analysis.

10

CONCLUSION

States' ability to govern their people and territory is central to the prospects of local development and peace in Africa. This dissertation has asked how and where African states were built and how their local capacity has affected development and conflict over the course of the 20^{th} century.

To address these two research questions, I have developed a theory of extractive state building. States are social organizations led by rulers who aim to maximize profits from extracting revenue from society. In return for taxation, they keep the peace, implement the law, and provide public services. In this trade of taxes for government services, rulers' profits depend on their bargaining power and the costs of transacting with their citizens. Rulers build their states to tilt both in their favor. They invest into crushing competing centers of social control to increase their bargaining power and make their population physically accessible to reduce transaction costs. As profit-maximizers, rulers place investments in response to their local costs and benefits. The results are geographically uneven state capacity and spatially varying patterns of local development and conflict.

I conclude by summarizing my main findings and most important contributions to the literature. Equally important, I highlight the limitations of the dissertation that give rise to promising directions for future research. I end with a discussion of tentative implications for policies that aim to strengthen states, foster development, and curb violent conflict on the continent and beyond.

10.1 SUMMARY OF THE MAIN FINDINGS

The first empirical part of this dissertation has examined two strategies of African states, colonial ones in particular, to extend their capacity to govern and extract revenue: Establishing direct or indirect rule to increase their bargaining power and building transport infrastructure to decrease transaction costs.

Zooming in on the first issue, Chapter 4 has analyzed the establishment of direct and indirect rule in French and British colonies. With more administrative resources at their disposal and constrained by a centralized metropolitan state, French colonial governments had greater incentives to establish direct rule than their British counterparts. While the British preferred indirect rule, the availability of centralized precolonial institutions to co-opt constrained this policy. In acephalous societies, the British had thus to opt for more direct forms of rule that filled the gap between central and local governments. With newly collected data I find that French colonization led to the demise of about 70 percent of all precolonial lines of successions. Under the more indirect style of British governance this number stands at "only" 30 percent. The data also shows that British colonial governance responded to the political organization of precolonial polities, employing less administrative effort and leaving native authorities with more power in precolonially centralized than in acephalous regions. This pattern is notably absent under more uniform direct rule in the French colonies.

As far as transaction costs are concerned, African states have invested into transport infrastructure that tears down the physical barriers between the state and its citizens. Such infrastructure is most profitable for rulers where it taps into resource-rich areas that promise large revenues. To test this argument, I have constructed a time-varying digital road atlas of the African continent based on colonial reports from British colonies as well as Michelin's road maps digitized with fully convolutional neural networks. The empirical analysis has shown that colonial transport investments followed a logic of facilitating resource extraction. Areas suitable for cash crop agriculture and mining exhibit denser road networks, better connections to ports, and increased access to regional and national capitals. The extractive character of African transport networks has persisted throughout the postcolonial period and remains visible until today. In this regard, postcolonial states in Africa have failed to overcome the extractive legacies they inherited from their colonial predecessors.

The second empirical part of this dissertation has analyzed the effects of local state capacity on development and conflict. Here again, the two dimensions of the state's bargaining power and its transaction costs appear to be crucial. The respective analyses have examined the impact of indirect rule on the colonial provision of public services as well as the effects of physical accessibility on local development and conflict.

First, Chapter 6 has studied how direct and indirect colonial rule affected the extent of public service provision in return for revenue extraction. I have argued that local populations under indirect colonial rule enjoyed more bargaining power and therefore received more public services in return for being taxed on their resource production, in particular cash crop agriculture. This logic is backed by empirical evidence that indirect rule increased the effect of local cash crop suitability on colonial education rates, a result that holds when analyzing only variation in indirect rule across colonial and ethnic borders. This result suggests that the relative bargaining power of local populations under indirect rule improved their terms of trade with colonial governments and constrained the latter's gains from extraction.

Beyond states' relative bargaining power, their transaction costs affect the trade of taxes for government services. Chapter 7 has argued that the extent of this trade decreases with higher state-society transaction costs and lower levels of physical accessibility. In consequence and supported by a cross-sectional analysis, governments interact more frequently with citizens close to their capitals, collecting more taxes and providing more services in return. This result is bolstered by evidence that local decreases in travel times to regional and national capitals increase education and infant survival rates as well as nightlight emissions.[1] This suggests that the trade between states and their citizens comes with developmental benefits for the latter.

Physical accessibility not only affects the prospects of local development, but also the dynamics of local conflict. For one, Chapter 8, co-authored with Philipp Hunziker and Lars-Erik Cederman, has demonstrated that roads can enable governments to uphold their monopoly of violence. However, roads can also help rebels to access, mobilize, and control local populations, thus creating opportunities and incentives to challenge state rule. The net effect of road networks on conflict depends on their structure, specifically on whether they facilitate transport from capitals more than connectivity within ethnic groups, which are frequently used as the basis of rebel mobilization. This result is supported by an instrumental variable strategy that draws on simulated road networks and emphasizes that rebels face the same problem of reaching local populations as the state does.

1 This is with the exception of infant mortality rates, which are unaffected by changes in travel times to regional capitals, and nightlight emissions, which show a meaningful but not statistically significant increase as travels to national capitals become faster.

With its theory of extractive state building and the accompanying empirical analyses, this dissertation makes four main contributions to the current literature on state capacity in Africa and its effects on development and conflict. First, the dissertation has gone beyond focusing on factors that constrain the ability of states to govern their societies. Highlighting, instead, variation in their incentives and efforts to mitigate the obstacles they face, the results stress that the fate of African states is not predetermined by their historical inheritance or geographic features of their territory (Herbst, 2000). African states can and do build their capacity to rule but in doing so seek to increase their revenue. Second and as a contribution to the "decompression of history" (Austin, 2008), the evidence on direct and indirect colonial rule underscores the importance of studying the interplay between precolonial and colonial institutions. European colonizers did not build their colonies on a tabula rasa but interacted with the social and geographic environments they had conquered.

Third, I have contributed to the literature on the effects of state capacity on patterns of development and conflict. In particular, my emphasis on local interactions between the state and its citizens goes well beyond research that treats state capacity as a national level phenomenon (e.g. Fearon and Laitin, 2003; Hendrix, 2010) and adds to a growing literature on spatial variation in states' capacity and its effects (e.g. Boone, 2003; Henn, 2018; Lee and Zhang, 2017). These three contributions have been made possible by a fourth: the collection of new, spatially and temporally disaggregated data that survey African states over the 20th century. After decades of debate over indirect rule in French and British colonies, the data on the fate of precolonial rulers and local (in)direct rule in their territories is the first to consistently capture within- and cross-colony variation in the local mode of colonial governance. Furthermore, the collection of data on road networks and postcolonial administrative geographies has produced substantive insights into how states' physical reach improved over time. These data are valuable for future research on local state capacity and other topics such as migration and trade on the continent (see also Jedwab and Moradi, 2016; Jedwab and Storeygard, 2018). In addition, Philipp Hunziker and I have demonstrated the value of using convolutional neural networks to digitize historical maps. Treating them as "satellite images from the past,"[2] future research can draw on the method to extract information from maps in a replicable and comparatively cheap manner.

2 Semantic segmentation of and object classification in satellite images has become one of the classic use-cases of convolutional neural networks (e.g. Castelluccio et al., 2015; Maggiori et al., 2017).

10.2 LIMITATIONS AND DIRECTIONS FOR FUTURE RESEARCH

Despite these contributions, this dissertation leaves important questions unanswered. These relate to the gaps left open by the theoretical argument, heterogeneities that affect the interactions between states and their societies, and the scope of the empirical analyses. Each limitation hints at avenues for future research that would extend our knowledge about state building and its effects on development and conflict.

The theoretical argument exhibits three particularly important gaps. First and in the interest of parsimony, I have neglected rulers' time horizons and their effects on decisions to invest in local state capacity. In particular, short time horizons come with generally lower investment levels (Besley and Persson, 2009, 2010; Levi, 1988), and may additionally shift the spatial allocation of these investments. Rulers that expect to be ousted soon prefer investments with a quick, high, but short-lasting return. In turn, governments with lower discount rates may spend their resources on investments where these yield small immediate benefits but high and sustainable profits over the long run.

Second, the theoretical argument has for the most part glossed over potentially important interactions between rulers' relative bargaining power and transaction costs. Such interactions might have important effects on how states invest into strengthening their position on either dimension and thereby affect processes of development and conflict. The analysis of the roads to rule and the roads to rebellion has only taken one first step in this regard, but has highlighted the potential for further inquiries.

Third, the idealized character of my theoretical argument has led me to treat all rulers as profit-maximizers in command of their states. With that, my theory of extractive state building has little to say about the effects of states' formal and informal institutions that constrain the power of rulers. While I have not found much change in road networks after the colonial period, the theoretical argument and empirical analyses remain silent about how, for example, the incentives of democratically elected leaders to build their states differ from those of autocrats (e.g. Bratton and Chang, 2006), or governments that rely on exclusive ethnic coalitions (e.g. Wimmer, 2018). There thus remains the need to explore the effects of institutional characteristics of states on how and where they invest in local state building.

Beyond these theoretical gaps, my empirical analyses on state building and its effects on development and conflict remain incomplete in four important ways. The first limitation originates from my focus on only two dimensions of state building, (in)direct rule and transport networks. These investments, though important, are by no means the only ones through which rulers can extend their bargaining power and reduce transaction costs. How and where do rulers invest to strengthen their states' military, bureaucracy, and their power over the economy? Where do they lower their transaction costs through technological innovations (Muralidharan, Niehaus and Sukhtankar, 2016), surveys (Scott, 1998; Lee and Zhang, 2017), and education (Gellner, 1983)? My theory's logic of revenue maximization would predict such investments to correspond to the uneven distribution of revenue potentials in society.

Second, my empirical inquiries have exclusively focused on 20$^{\text{th}}$ century Africa. While important in its own right and motivated by the continent's position in the global distribution of development, conflict, and state capacity, this geographic restriction looses sight of state-building trajectories elsewhere on the globe. Have rulers in other regions sought to increase their revenue through state building in a manner similar to the patterns observed in Africa, or is the colonial experience of the continent unique? In particular, a comparison with patterns of state building in non-colonies is necessary to test the external validity of my arguments and results. Broadening the empirical scope would also raise questions that go beyond what I have studied here. Do states, for example, treat 'internally colonized' regions (Hechter, 1975) in a similar manner as they treat external colonies? How do the patterns of state building and extraction differ between the core and peripheries of contiguous empires, such as the Russian, Habsburg or Ottoman empires? Answers to these questions lie outside of African states, founded as external colonies at the periphery of their overseas empires.

A third limitation of my analyses concerns their silence on general equilibrium effects. Almost exclusively exploiting variation within countries or colonies, the empirical models do not illuminate how local patterns of state building add up and produce macro-level effects. While empirically challenging, such inquiries are necessary to close the gap between the micro- and the macro-levels and assess how uneven state capacity can impair development and foster conflict beyond subnational areas with weak state capacity. Such questions are of high relevance, as demonstrated by the outbreak of civil war in Mali's far North and its implications for political order and development in the country as a whole. Analyses along the lines of Acemoglu, García-Jimeno and Robinson (2015) who study spillover effects of local state capacity in the network of Colombian municipalities bear the promise of bridging this gap.

Lastly, my empirical analyses of the effects of state capacity on development and conflict have concentrated on average effects. Yet and beyond the cross-country heterogeneity addressed above, states' local capacities may matter over-proportionally in times of crisis, ergo in times of rebellion, natural disasters, or famines (e.g. Detges, 2016; Devereux, 2009). Studying how states' local capacity impedes swift and effective responses to emergencies is highly relevant to identify areas that are at risk of being neglected. Such information might prove valuable for preventive action of governments, the international community, and NGOs.

10.3 POLICY IMPLICATIONS

These limitations in mind, the theoretical framework and empirical results of the dissertation nevertheless allow for discussing a set of tentative implications for policies that aim to strengthen states, foster local development, and prevent conflict.

Over the past two decades, scholars have noted an increasing "resurgence" of traditional authorities in some African states, where governments ceded power over land rights and development projects to local leaders (Englebert, 2002). Beyond promising electoral advantages for politicians (Baldwin, 2014), this strategy can make up for ineffective state administrations and increase the provision of local public goods (Baldwin, 2016). However, despite these benefits and beyond issues of accountability of unelected traditional leaders (e.g. Acemoglu, Reed and Robinson, 2014; Mamdani, 1996), governments that empower chiefs may in the future wish to regain direct control. Such efforts to strengthen their states' capacity may well spark resistance among local elites and their constituents who fear getting re-colonized by the state.

The second issue to highlight here is the necessity to counter the extractive roots of African states' reach. The geography of administrative divisions and transport infrastructure on the continent show substantive biases towards resource-rich areas. Areas without resources form "the empty panorama" that was left neglected by colonial governments (Fanon, 2004, p. 106), a pattern that has not changed much since independence (see also Boone, 2003; Roessler et al., 2018). Because high transaction costs between citizens and the state impair local development and increase the risk of conflict, investments that connect resource-poor areas to capitals likely benefit their inhabitants but will not be prioritized by governments. Such investments may thus have to be pushed by international actors with an 'altruistic' development agenda. These actors should however take care not to crowd out investments that pay off for governments and would therefore be implemented without aid from abroad.[3]

But road building alone may not necessarily pave the way for sustainable development and peace. For one, Chapter 8 shows that some roads can backfire by increasing the capacity of rebels to mobilize more than they increase states' reach. This would suggest building rigid hub-and-spoke networks that maximize the capacity to repress but severely constrain trade and exchange. More sensibly, those who aim to strengthen the state, necessarily at the expense of local power-holders, should devise mechanisms to do so without sparking rebellious defense in peripheral ethnic groups. Sharing political power and addressing socio-economic inequalities might be part of such a solution (e.g. Cederman, Gleditsch and Buhaug, 2013; Wucherpfennig, Hunziker and Cederman, 2016).

Beyond building roads to capitals, reforms of administrative geographies have come into fashion in some African countries. Indeed, Chapter 7 shows that proliferating administrative units can increase local development by bringing capitals and citizens closer together. But such reforms are no panacea and come with two risks in particular. First, Grossman, Pierskalla and Dean (2017) show that the effects of such reforms are decreasing and may turn negative when units become too numerous and small, thus loosing their economies of scale. The optimal size and geography of administrative units likely depends on local geographic and social contexts. The second caveat of redrawing administrative maps consists in the changes of subnational political competition they can bring about. In particular in multi-ethnic areas, reshaping administrative geographies causes shifts in the ethnic demography of administrative units (Englebert, Calderon and Jené, 2018). Where ethnic cleavages dominate politics, coalitions realign and conflict over power may ensue.

I have argued in this dissertation that rulers rule for material gain. They build their state to maximize the profit they derive from the exchange of taxes for law,

3 The problem of fungible development aid is well studied by a large literature on development economics (e.g. Pack and Pack, 1993, and studies that followed up on their findings). On fungible road infrastructure projects, see for example van de Walle and Cratty (2005) and van de Walle and Mu (2007).

order, and public services. My insights about rulers' incentives and their strategies to strengthen their state have allowed me to shed light on the origins of local state capacity in Africa and its effects on development and conflict. They may also prove valuable for those who are trying to strengthen states and reduce their geographically uneven capacity, in order to increase the benefits governments offer to the people they ought to serve.

APPENDICES

CONTENTS

Table A.1: Data coverage across chapters

Country	Chapter 4		Chapter 5		Chapter 6	Chapter 7		Chapter 8
	Rulers	Loc. adm.	Col.	Postcol.		DHS	Lights	
Algeria	x			x			x	x
Angola				x		x	x	x
Benin	x	x		x	x	x	x	x
Botswana	x			x			x	x
Burkina Faso	x	x		x	x	x	x	x
Burundi				x		x	x	x
Cameroon				x	x	x	x	x
Central African Republic				x	x	x	x	x
Chad	x			x			x	x
Congo				x			x	x
Côte d'Ivoire	x	x		x	x	x	x	x
Djibouti				x			x	x
DR Congo				x		x	x	x
Egypt				x		x	x	x
Equatorial Guinea				x			x	x
Eritrea				x			x	x
Ethiopia				x		x	x	x
Gabon				x	x	x	x	x
Gambia				x			x	x
Ghana	x	x	x	x	x	x	x	x
Guinea	x	x		x	x	x	x	x
Guinea Bissau				x			x	x
Kenya		x	x	x	x	x	x	x
Lesotho	x			x	x	x	x	x
Liberia				x		x	x	x
Libya	x			x			x	x
Madagascar	x				x			
Malawi		x	x	x	x	x	x	x
Mali	x	x		x	x	x	x	x
Mauritania	x	x		x			x	x
Morocco	x			x		x	x	x
Mozambique				x		x	x	x
Namibia				x		x	x	x
Niger	x	x		x	x	x	x	x
Nigeria	x	x	x	x	x	x	x	x
Rwanda				x		x	x	x
Senegal	x	x		x	x	x	x	x
Sierra Leone	x	x	x	x	x	x	x	x
Somalia				x			x	x
South Africa	x			x			x	x
South Sudan				x			x	x
Sudan	x			x			x	x
Swaziland	x			x	x	x	x	x
Tanzania	x	x	x	x	x	x	x	x
Togo	x			x	x	x	x	x
Tunisia				x			x	x
Uganda	x	x	x	x	x	x	x	x
Zambia	x	x	x	x	x	x	x	x
Zimbabwe	x			x	x	x	x	x

Notes: Countries refer to current countries as well as to colonies.

A

DATA ON INDIRECT COLONIAL RULE

This appendix provides an overview over the data collected for the analyses of indirect rule in British and French colonies conducted in Chapter 4. Section A.1 presents the data on precolonial polities and their lines of succession digitized from the encyclopedia of 'African States and Rulers' compiled by Stewart (2006). Section A.2 presents data on the budgets of native treasuries in four British colonies. Finally, Section A.3 briefly summarizes the correlations between the four main proxies of indirect rule in British colonies: the size of districts (this data is presented in Appendix C.1), the number of British administrators, the size of native treasuries, and the class of chiefs in Nigeria.

A.1 PRECOLONIAL POLITIES

Table A.1: Summary of data on lines of succesion

Statistic	N	Mean	St. Dev.	Min	Max
British rule	5237	0.787	0.409	0	1
French rule	5237	0.213	0.409	0	1
Year	5237	1923.307	22.528	1830	2006
Population (log)	5208	3.721	1.488	0.000	7.769
Distance to coast (log)	5208	5.505	1.827	0.355	7.397
Distance to nav. river (log)	5208	4.634	1.449	−0.223	7.724
Polity age (log)	5237	5.573	0.752	2.197	7.096
Dependence on agriculture	5016	1.902	1.371	0	8
Dependence on husbandry	5016	5.632	2.376	0	9
Intensity of agriculture	4792	2.109	0.984	0	4
Precol. centralization	4790	1.870	1.099	0	4
Altitude (median)	5208	503.906	437.447	5	1745
Slope (median)	5208	3.987	1.613	1	9
Temperature (mean)	5208	25.056	3.702	14.590	29.860
Evapotranspiration	5208	1730.642	333.677	1133	2347
Precipitation	5208	975.664	490.950	16	3006
Evapotransp. / precipitation	5208	3.869	1.334	1	7
Suitability for agr.	4902	0.333	0.210	0.001	0.785
Cash crop suitability	5208	0.351	0.161	0.000	0.863

Table A.1 summarized the data on precolonial polities digitized from the encyclopedia on 'African States and Rulers' compiled by Stewart (2006) and the covariates attributed to them. The map in Figure A.1 shows the spatial distribution of the polities' capitals.

In addition to providing these summaries, this section aims to test the possibility that the *observation* of polities by Stewart (2006) is biased, in particular by potential effects of precolonial centralization and the colonizing power on the probability of observing more (or less) polities in a certain area. As throughout the main chapter, the analysis is limited towards polities that were colonized by either the French or the British empire. I analyze the quality of information on precolonial polities in three ways:

First, since Stewart (2006) gives a short account of the history of polities, we can systematically assess the information available for each state. For each polity that was ever colonized, I thus code the simple length of the historical account in characters. Table A.2 reports the results of simple linear models of the logged number of characters on a 'British' rule dummy, the level of precolonial centralization of the (last) capital of a polity, and additional controls. The results show that Stewart does

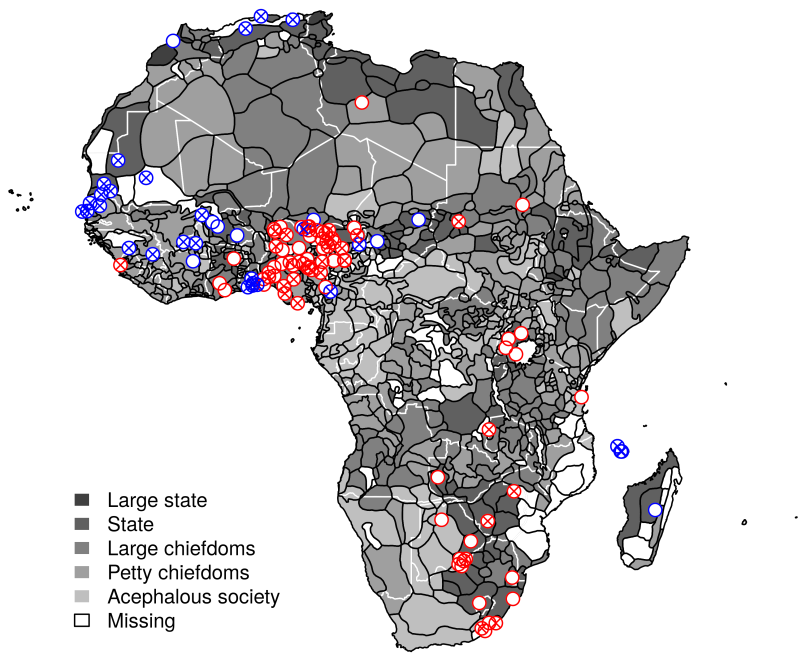


Figure A.1: Polities colonized by the French or British empire in Africa.
The capitals of polities colonized by the French (British) are drawn in blue (red). Precolonial polities with lines of succession that ended before the end of the colonial period are marked with a cross.

Table A.2: Information per polity

	log(Characters of historical account)		
	(1)	(2)	(3)
British colony	0.208	−0.056	0.079
	(0.172)	(0.213)	(0.223)
Precol. centralization	0.296***	0.250***	0.230**
	(0.076)	(0.079)	(0.099)
Population/km^2 (1880; log)	0.050	0.055	0.017
	(0.047)	(0.114)	(0.115)
Distance to coast (log)	0.015	−0.030	
	(0.054)	(0.063)	
Distance to river (log)			−0.053
			(0.068)
Nature controls:	no	yes	yes
Ethnic controls:	no	no	yes
Mean DV	6.28	6.28	6.28
F-Stat:	8.7	4	3.57
Observations	101	97	97
Adjusted R^2	0.278	0.289	0.300

Notes: OLS models. Robust standard errors in parenthesis. Nature controls. consist of median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls consist of the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01

not give more detailed accounts of polities that were colonized by the British than by the French. Not surprisingly, the grand kingdoms, those polities located in highly centralized areas, are described in more detail.

The second approach is based on Murdock's (1967) ethnic settlement areas and takes the colony-ethnic polygon as the unit of analysis. For each polygon, I count the number of years that polygon has featured a polity in all year before 1885. This count comes in three flavors: the first counts every unique polity-year. The second counts every year only once, even if there are multiple polities in the same settlement area. The third adds to that a simple discount rate of 5 percent per year, so as to not overweight long-lasting empires such as Bornu in Northern Nigeria. Lastly, a simple dummy encodes whether there has ever been a polity observed by Stewart in a particular polygon.

Table A.3: Observed polity-history per ethnic group: Difference between French and British colonizers

	Years of precolonial data:			
	P(any year)	discounted	unique	all
	(1)	(2)	(3)	(4)
British colony	0.019	4.159	13.871	26.461
	(0.024)	(3.245)	(21.802)	(37.744)
Precol. centralization	0.057^{***}	8.625^{***}	28.415^{***}	32.133^{**}
	(0.011)	(1.396)	(9.377)	(16.234)
Population/km^2 (1880; log)	0.098^{***}	12.757^{***}	68.290^{***}	105.552^{***}
	(0.012)	(1.619)	(10.877)	(18.830)
Distance to coast (log)	0.012	0.589	15.354	26.373
	(0.014)	(1.845)	(12.397)	(21.463)
Distance to river (log)	−0.004	−1.144	-14.607^{*}	-24.384^{*}
	(0.009)	(1.204)	(8.088)	(14.003)
Area (km^2, log)	0.062^{***}	7.420^{***}	33.462^{***}	47.721^{***}
	(0.006)	(0.860)	(5.780)	(10.006)
Ethnic controls:	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes
Mean DV	0.12	13.19	48.9	64.11
F-Stat:	13.94	12.67	7.78	5.98
Observations	893	893	893	893
Adjusted R^2	0.198	0.182	0.114	0.087

Notes: OLS models. Robust standard errors in parenthesis. Nature controls consist of median. altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls consist of the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01

None these four variables is significantly related to British colonization (see Table A.3). Highlighting the overlap between the polity data and Murdock's Ethnographic Atlas, the degree of precolonial centralization is highly correlated with the number of years an ethnic group is associated with a precolonial polity – no matter how the latter value is constructed. For example, Model 3 suggests that moving from an acephalous ethnic group to a centralized one (precolonial centralization = 3) adds 85 more 'state-years.' Figure A.2 furthermore shows, that this relationship is non-linear and strongest for the highest degrees of precolonial statehood as coded by Murdock. This underlines the quality of the data.

The third analysis (Table A.4) builds on this approach, only exchanging the ethnic polygons with a simple raster of a resolution of 0.417 by 0.417 decimal degrees.

Table A.4: Observed polity-history per raster cell

	Years of precolonial data:			
	P(any year)	discounted	unique	all
	(1)	(2)	(3)	(4)
British colony	0.003	0.199	−0.639	−0.156
	(0.003)	(0.374)	(1.700)	(2.017)
Precol. centralization	0.007***	0.754***	1.707**	1.792**
	(0.002)	(0.165)	(0.752)	(0.892)
Population/km^2 (1880; log)	0.040***	3.178***	15.295***	17.836***
	(0.002)	(0.191)	(0.868)	(1.030)
Distance to coast (log)	0.004**	0.083	0.260	0.393
	(0.002)	(0.196)	(0.890)	(1.055)
Distance to river (log)	−0.001	0.116	−1.312**	−1.749**
	(0.001)	(0.135)	(0.614)	(0.728)
Ethnic controls:	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes
Mean DV	0.02	1.41	5.16	5.64
F-Stat:	50.88	29.66	29.76	28.88
Observations	9,692	9,692	9,692	9,692
Adjusted R^2	0.076	0.045	0.045	0.044

Notes: OLS models. Robust standard errors in parenthesis. Nature controls consist of median. altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls consist of the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01

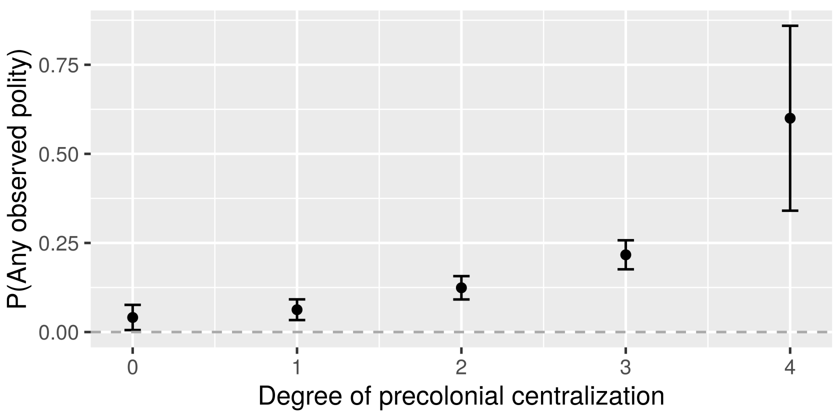


Figure A.2: Probability of an ethnic group featuring a precolonial state as coded by Stewart (2006) by level of precolonial centralization.

The results underline the weak relationship between British rule and information on precolonial polities provided by Stewart: none of the counts of polity-years is significantly correlated with the British rule dummy. In contrast, the pattern that the Murdock coding of precolonial centralization is a significant correlate of Stewart's polity data is also prevalent when using raster cells as the units of analysis.

A.2 DATA ON NATIVE TREASURIES IN BRITISH COLONIES

The digitization of reports on native treasuries' budgets proceeds in two stages. I first process the scanned images of the respective pages in the colonial reports automatically to extract structured data from the tables they enclose. I then postprocess the results to correct errors. Subsection A.2.1 provides details on this procedure, and Subsection A.2.2 enlists the sources and provides an overview over the resulting data set.

A.2.1 *Digitizing tables from British colonial reports*

I automatically extract structured information from tables in the scanned pages of the British Blue Books and Annual Departmental reports, information which is cleaned by hand in a second step. To this end, I developed an algorithm that transforms an image of a printed table[1] into a machine readable matrix of strings which is then stored in a relational database. The algorithm proceeds as follows:

1. *Image pre-processing:* Transforming the images into binary black and white pixels and turning to maximize the horizontal alignment of rows.[2]

2. *Table cell detection:* Segmenting the image into rows and columns based on (1) vertical lines that delimit columns, and (2) clustering of the x- and y-coordinates of connected image components retrieved by a horizontal blurring filter that produces probable text blocks. Warps in the image that stem from the physical wave of the pages of an open book are corrected by using horizontal (waved) lines as reverence lines to straighten the entire image.

1 It must be born in mind that conventional OCR programs fail at digitizing table from such deprecated scans as dealt with in the context of historical archives.
2 Doing so is achieved by maximizing the standard deviation of the row-wise sum of black pixels in the image.

3. *Optical character recognition (OCR):* Extracting text from the cell-images using the open source program Ocropy (Breuel, 2014). Ocropy is based on a recurrent neural network which is trained on 8000 cell images from the colonial Blue Books.

Although the automatic extraction of information from the scanned image is efficient, and the OCR attains an error rate of only about 3%, each page is post-processed by hand to correct remaining inaccuracies. Such errors are highly clustered, since they mostly stem from low-quality printing and scanning on certain pages or regions of a page. Where I extract numbers, such errors are in particular worrying, since they oftentimes introduce errors in the number of digits, thus altering a number's order of magnitude. Errors also emerge if pages are printed in a font for which the neural network used for the OCR is not trained.

A.2.2 *Budget data: sources and resulting data set*

Table A.6 enlists all Annual Colonial Reports from which I digitize financial information on the budgets of native administrations, with Figure A.3 showing an exemplary report page. Table A.6 provides the summary statistics of the digitized data, averaged by district and over all years in which a district is observed (see Chapter 4). Figure A.4 provides and overview over the development of per-capita revenues of the native treasuries in each district in the sample. As apparent in the plots, most districts developed very much in parallel without much variance in their rank. This strengthens the validity of the approach of using the average revenue and expenditure within a district as the main dependent variable in the respective analysis in Chapter 4. Lastly, Figure A.5 maps the average revenue by district in each colony in the sample.

Table A.5: Sources of native treasury data

Colony	Title	Pages	Microform ID
Gold Coast	Local Government Revenue and Expenditure, 1948-1951	6; 7; 11; 14; 15; 35; 36; 41; 42; 45; 46; 71; 73; 77; 78; 82; 83	73211B-13
Nigeria	Native Authority Estimates, North, 1929-1937	150; 160; 301; 302	73242B-22
Nigeria	Native Authority Estimates, North, 1934-1938	4; 147; 290; 435	73242B-23
Nigeria	Native Authority Estimates, North, 1938-1941	4; 266; 267	73242B-24
Nigeria	Native Authority Estimates, North, 1944-1946	135; 136; 425; 426	73242B-25
Nigeria	Native Authority Estimates, North, 1948-1950	345; 346; 675; 676	73242B-26
Nigeria	Native Authority Estimates, North, 1950-1952	334; 335; 336; 337; 340	73242B-27
Nigeria	Memoranda on Estimates, North, 1948-1960	43; 61; 80; 98; 113; 114; 115; 136; 156	73242B-35
Nigeria	Native Financial Statements, South, 1929-1937	98; 192; 193; 298; 299; 416; 417; 556; 557	73242B-36
Nigeria	Native Financial Statements, South, 1937-1939	143; 144; 342	73242B-37
Nigeria	Native Financial Statements, East, 1939-1943	157; 158	73242B-38
Nigeria	Native Financial Statements, West, 1939-1940	81; 82	73242B-40
Nyasaland	Native Affairs and Administration 1931-1945	214; 301; 334; 378; 414; 445; 484; 518; 548; 575; 613; 624; 637	73105A-01
Nyasaland	Native affairs and Administration 1946-1959	14; 31; 44; 61; 75; 91; 101; 114; 132; 150; 166; 189; 209; 225; 243; 262; 274; 285; 299; 309; 320; 335; 348; 360; 379; 391; 402; 417; 428; 441	73105A-02
Uganda	Provincial Commissioners, 1935-1938	7; 9; 10; 11; 12; 17; 18; 19; 20; 21; 22; 23; 24; 25; 26; 27; 28; 36; 55; 56; 57; 58; 59; 70; 71; 72; 73; 74; 75; 85; 98; 100; 101; 102; 103; 104; 116; 119; 120; 121; 133; 155; 157; 158; 159; 160; 161; 162; 173; 174; 185	73143A-01
Uganda	Provincial Commissioners, 1939-1946	30; 33; 34; 57; 58; 59; 60; 61; 62; 119; 120; 122	73143A-02
Uganda	Provincial Commissioners, 1947-1952	63; 97; 98; 116; 117; 142; 143; 181; 216; 233; 255; 256; 305; 368; 401; 453; 454; 487; 488; 511; 556; 557; 615; 616; 644; 676; 677; 715; 716; 762; 763; 804; 864	73143A-03
Uganda	Provincial Commissioners, 1953-1956	58; 59; 105; 106; 170; 223; 224; 270; 271; 316; 380; 433; 434; 484; 517; 518; 575; 576; 632; 696; 697	73143A-04

Microform ID denotes the ‘Reference ID’ used on www.britishonlinearchives.co.uk.

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SUMMARY OF REVENUE, 1931-1932.

Treasury.	Tribute.	Jangali.	Native Courts. Fees & Fines	Interest on Investments.	Other Receipts.	Total.	Total of Province.
	£	£	£	£	£	£	£
Adamawa :—							
Adamawa	21,000	7,420	850	900	240	30,410	
Muri	6,300	1,400	500	290	63	8,553	
Numan	2,000	1,020	190	174	190	3,574	42,537
							42,105
Bauchi :—							
Bauchi	23,000	11,000	1,450	1,900	3,320	40,670	
Dass	640	180	10	150	...	980	
Gombe	10,900	6,233	1,100	1,300	304	19,837	
Jamaari	920	450	80	70	475	1,995	
Katagum	13,920	5,880	790	800	1,253	22,643	
Misau	4,850	1,930	395	170	433	7,778	
Ningi	1,900	655	210	450	5	3,220	
Tangale-Waja	2,900	300	200	550	25	3,975	101,098
							97,522
Benue :—							
Abinsi	11,500	...	1,750	440	690	14,380	
Idoma	4,250	...	500	120	110	4,980	
Keffi	3,300	700	245	50	139	4,434	
Lafia	2,400	...	215	80	174	2,869	
Nasarawa	5,900	170	320	175	269	6,834	
Wukari	7,300	20	690	180	280	8,470	41,967
							39,510
Bornu :—							
Bedde	1,500	840	110	48	15	2,513	
Biu	5,460	1,330	645	200	65	7,700	
Bornu	40,000	26,700	2,900	2,288	3,430	75,318	
Dikwa	8,700	3,600	400	300	45	13,045	
Fika	3,400	1,110	325	102	40	4,967	103,543
							101,666
Ilorin :—							
Bussa	1,560	400	70	192	61	2,283	
Ilorin	41,100	2,000	3,000	2,000	360	48,460	
Kaiama	1,280	550	120	199	376	2,525	
Lafiagi	3,520	325	190	90	68	4,193	
Pategi	2,662	30	270	62	30	3,054	60,515
							60,346
Kabba :—							
Agbaja	1,070	...	230	90	50	1,440	
Ata Gala	14,250	30	1,950	900	1,720	18,850	
Igbirra	9,450	150	3,600	650	737	14,587	
Kabba	3,900	25	670	200	165	4,960	
Koton-Karifi	2,680	...	440	300	180	3,600	43,437
							41,866
Kano :—							
Daura	6,650	2,450	450	300	85	9,935	
Gumel	3,850	2,100	355	300	50	6,655	
Hadejia	10,500	2,940	380	700	850	15,370	
Kano	177,100	30,000	4,100	4,850	14,780	230,830	
Kazaure	5,390	1,820	310	250	25	7,795	270,585
							250,056
Niger :—							
Abuja	8,860	160	200	319	277	9,816	
Agaie	2,560	...	180	90	156	2,986	
Bida	20,245	1,190	850	1,100	1,240	24,625	
Kontagora	5,950	1,680	285	235	285	8,435	
Kuta	3,800	600	630	250	470	5,750	
Lapai	2,800	7	190	260	80	3,337	
Zungeru	2,400	200	160	120	113	2,993	57,942
							56,873
Plateau :—							
Akwanga	3,300	...	300	80	150	3,830	
Jemaa	4,800	500	600	180	196	6,276	
Jos	10,700	5,000	2,000	1,045	2,125	20,870	
Kanam	1,018	575	52	90	10	1,745	
Pankshin	4,432	3,655	600	100	220	9,007	
Shendam	3,970	270	375	110	60	4,785	46,513
							46,008
Sokoto :—							
Argungu	6,300	3,220	250	330	410	10,510	
Dabai	3,200	1,400	300	300	295	5,495	
Gwandu	24,000	7,200	830	1,000	1,310	34,340	
Sokoto	86,458	29,500	2,450	3,250	3,164	124,822	
Yauri	3,400	1,000	200	400	315	5,315	180,482
							179,932
Zaria :—							
Birnin Gwari	1,270	230	110	100	20	1,730	
Katsina	59,500	21,000	3,200	6,639	1,550	91,889	
Zaria	42,350	7,000	1,550	2,050	1,705	54,655	148,274
							147,760
Total £	768,315	198,135	45,322	39,868	45,253	1,096,893	1,096,893
	749,855	196,782	45,407	39,247	32,375	1,063,666	

Figure A.3: Detail of native treasuries' summary of revenues: Northern Nigeria, 1931-1932, Microform ID: 73242B-22, page 151.

Table A.6: Summary of British budget data

Statistic	N	Mean	St. Dev.	Min	Max
Total revenue (log)	147	13.38	1.22	9.01	16.42
Revenue from: Taxes (log)	147	12.78	1.22	8.56	16.26
... Fees & fines (log)	147	11.48	1.35	7.88	15.22
... Transfers (log)	147	6.76	5.65	0.00	15.18
... Other (log)	128	10.99	1.49	4.94	14.37
Total expenitures (log)	127	13.60	1.12	8.85	16.47
Expenditures on: Administration (log)	127	12.25	1.20	6.92	15.54
... Order (log)	127	11.77	0.89	8.25	14.34
... Education & health (log)	127	11.42	1.66	0.00	14.44
... Agriculture (log)	127	9.13	2.19	0.00	13.99
... Public works (log)	127	12.13	1.31	7.47	15.49
... Other (log)	127	10.93	1.57	5.81	15.65
Precolonial centralization	146	1.55	0.71	0.00	3.00
Population (log)	146	12.25	0.77	9.71	14.89
Area (log)	146	2.16	0.11	1.89	2.43
Population density (log, 1880)	146	3.00	0.96	0.84	5.25
Ethnic groups' pop. density (log, 1880)	146	3.03	0.87	1.02	4.52
Distance to coast (log)	146	5.31	1.28	0.14	7.07
Distance to nav. river (log)	146	4.41	0.84	2.48	5.99
Median altitude	146	383.91	386.85	8.77	1756.29
Median slope	146	3.78	1.18	1.67	7.49
Mean temperature	146	25.77	2.20	18.58	28.97
Evapotranspiration	146	1572.15	257.01	1195.29	2318.16
Precipitation	146	1444.26	530.15	485.48	2835.08
Evapotranspiration/Precipitation	146	5.08	1.30	2.04	8.00
Agricultural suitability	146	0.45	0.19	0.01	0.89
Cash crop suitability	146	0.40	0.11	0.08	0.72
Reliance on agriculture	146	1.37	0.72	0.00	3.78
Reliance on pastoralism	146	6.71	1.13	3.12	9.00
Intensity of agriculture	146	2.14	0.29	2.00	3.00

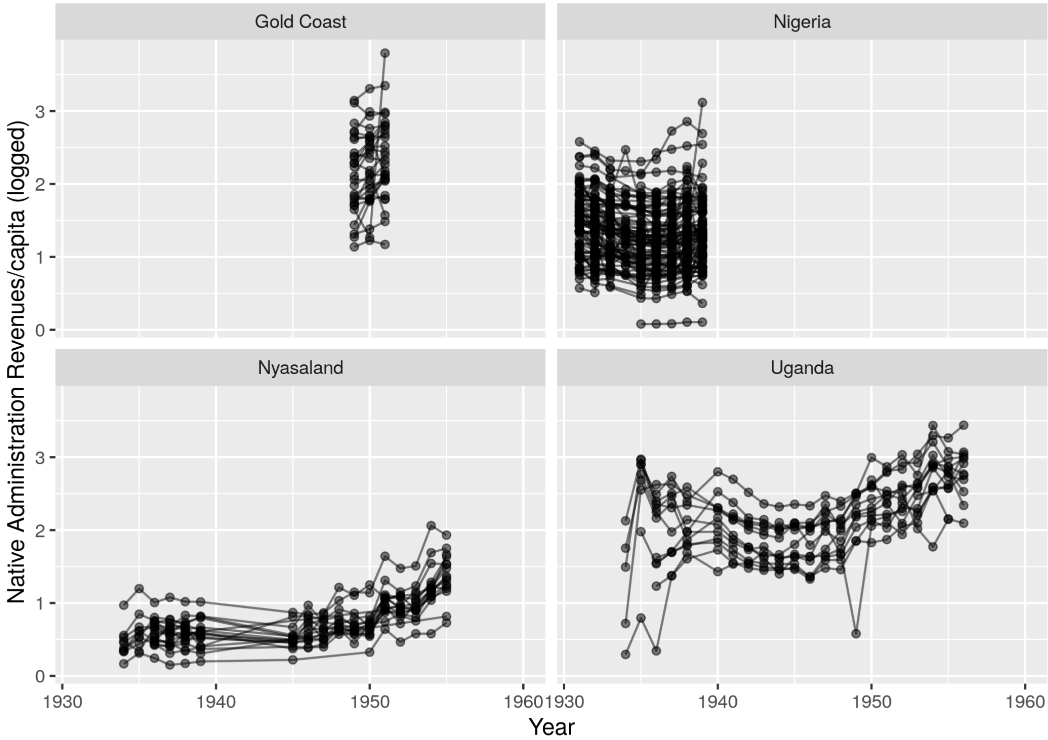
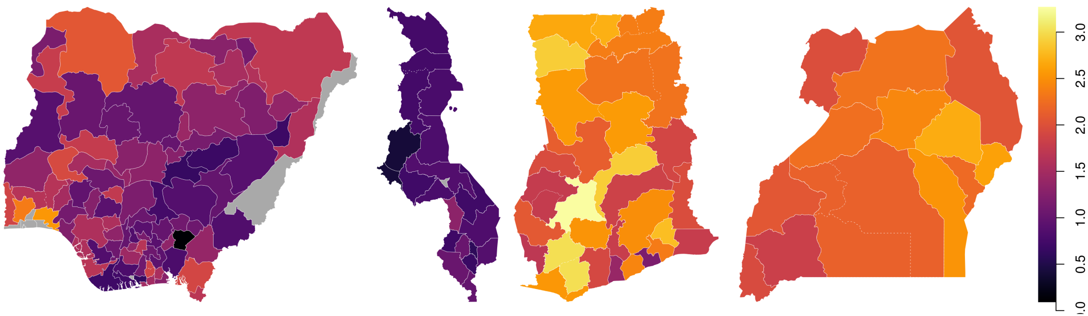


Figure A.4: Per-capita revenues of native treasuries over time (logged; 2016 £). Aggregated to the district level.



(a) Nigeria 1931-1939 (b) Nyasaland, 1934-1955 (c) Gold Coast, 1949-1951 (d) Uganda 1934-1956

Figure A.5: Per-capita revenues of native treasuries (logged; 2016 £). Aggregated to the district level and averaged over all observed years.

A.3 CORRELATIONS BETWEEN INDICATORS OF BRITISH INDIRECT RULE

In order to gauge in how far the various measures of indirect rule used as dependent variables in Chapter 4 correlated with each other and thus consistently capture "indirect rule," Figure A.6 displays the correlation matrix of all four measures. All outcomes are correlated with each other, but not perfectly. This supports the view that they capture varying aspects of indirect rule.

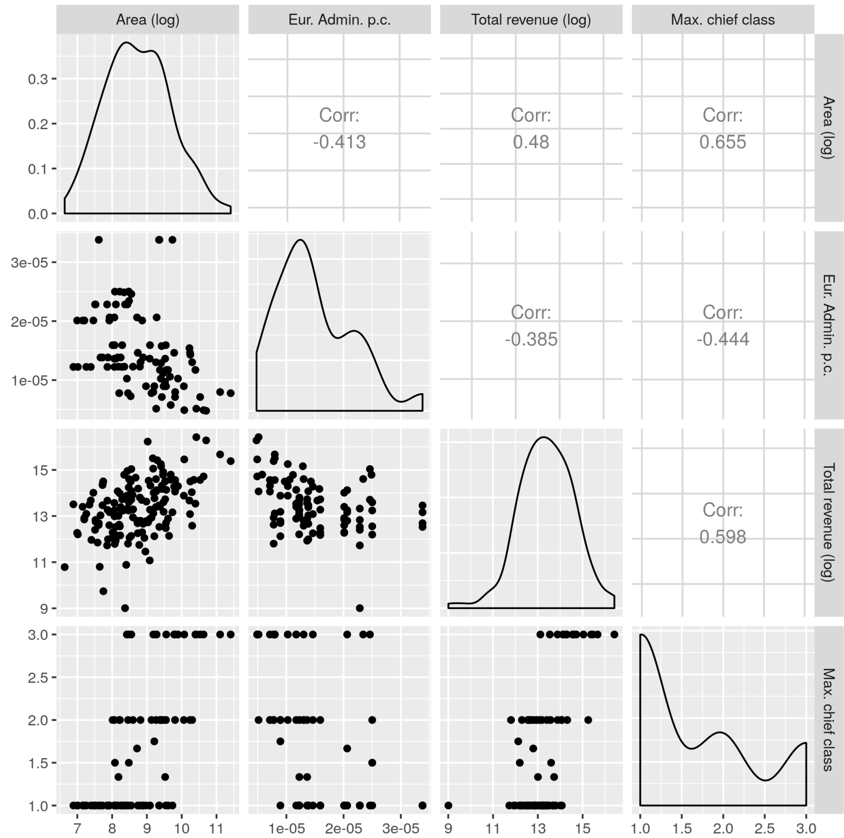


Figure A.6: Correlations between four main measures of British indirect rule.

B

ROAD NETWORKS IN AFRICA 1900–2017

This appendix describes the data collection and preparation processes underlying the road network data used in Chapters 5, 7, and 8. Section B.1 describes the road data extracted from British colonial reports, Section B.2 presents the procedure of digitizing the Michelin map corpus, and Section B.3 provides the details of the construction of planar road networks that I use to compute travel times and costs.

B.1 ROAD DATA FROM BRITISH COLONIAL REPORTS

B.1.1 *From lists to roads*

In order to digitize the road-lists from the British colonial Blue Books (for an example, see Figure B.1), I pre-process the scanned images of the lists following the same procedure used to digitize the budget data (Chapter 4). The procedure, described in detail in Appendix A.2.1 above, identifies cells in tables, and extract their content through Optical Character Recognition. The results are then corrected by hand to avoid errors.

In the next step, I map the digitized Blue Book road lists onto current road networks retrieved from the gROADS dataset (CIESIN, 2013). In the road lists, each road is encoded as a string that consists of a sequence of names of cities or villages.[1] This string serves as the primary input for the road mapping algorithm visualized in Figure B.2:

1. *Road string-coordinate matching:* Using fuzzy string matching, each place name in a road string is recursively matched with entries in the `geonames.org` database. Retrieving the coordinates of these matches produces a sequence of spatial points, each of which corresponds to a place name in the road string.[2]

1 Roads connecting a village with another road are encoded as "Village X - Road Y". In such cases, the geo-coded village is simply connected to the closest road in the network.

2 Place names can be matched with more than one coordinate if several places in a country have the same name. This complication is dealt with in step 3.

SECTION 32. 645

(3) ROADS.

Road.	Length open for Traffic. Miles.
COLONY AND SOUTHERN PROVINCES.	
Lagos–Abeokuta–Ibadan	112½
Agege–Iju Waterworks	3½
Ikeja–Isheri	6
Lagos–Apapa	2½
Apapa Township Roads	2
Iddo Roads	1½
Erinmo–Efon–Ara–Ido	41
Oshogbo–Illa	35
Ilugun–Olokemeji–Eruwa Road Jn.	16
Agbor–Jesse	40
Ijebu Ode–Epe	19
Ifaki–Ikole	21
Ondo–Oke Igbo	9
Abeokuta–Asha	28
Abeokuta–Ijebu Ode	60
Iperu–Ikorodu	29½
Ijebu Ode Town Roads	6
Ibadan–Ilorin	102
Papalanto–Agbesi	2
Papalanto–Ilaro	14
Lafenwa–Aiyetoro	25
Ilorin–Awtun	61
Ilorin–Share–Jebba	67½
Ajasse–Lafiaji	68
Ajasse–Offa	9
Oyo–Shaki	79
Ibadan–Ejinrin	62½
Oyo–Iwo	32
Ileigbo Station Road	4
Ogbomosho–Oshogbo	37
Oko–Ede–Ife	39
Ibadan Town Roads	16¼
Ibadan–Ilesha	75½
Oshogbo–Benin	179
Iseyin–Eruwa Road, Railway Station	46
Abeokuta Town Roads	12
Ijaye–Kajola	3½
Owode–Mowe	13*
Olorunda Market–Imala	4*
Aiyetoro–Ilaro	28½†
Ilaro–Ado	25½‡

Road.	Length open for Traffic. Miles.
Ibadan–Olodo	6
Ibadan–Akanran	13¾
Benin–Warri	63
Sapele–Kwale–Agbor	76
Benin–Asaba	86
Agbor–Uromi	40
Uromi–Illushi	26
Benin–Uromi	62
Irrua–Agbede	10
Akure–Ondo–Agbabu	73
Igbara–Oke–Ado–Awtun	60½
Warri Township Roads	8
Benin City Township Roads	5
Benin–Siluko	42‖
Ubiaja–Agenebode	49
Awka–Achalla	12
Forcados Station Roads	8
Agbor–Sapoba–Sapele	27
Warri–Okpara	18§
Sapele Township Roads	3
Onitsha Township Roads	10
Enugu Township Roads	15
Onitsha–Owerri	61
Uli–Oguta	7
Mbidi–Okwelli	24
Dikenafie Jn.–Awka	36
Okigwi–Afikpo–Ndibi Beach	51
Okigwi–Awgu	23
Onitsha–Enugu	67
Oji River–Ndeabo	32
Udi Switch Road	5
Onitsha, Enugu Road Junction–Nsukka	32
Eha Alumona–Obolo–Okwoga	39
Enugu–Abakaliki	52
Abakaliki–Obubra	24
Bansara–Ofun Atam	24‖
Okigwi–Owerri	37
Abakaliki towards Afikpo (Abakaliki–Afikpo Boundary)	22
Abakaliki–Bansara–Ogoja	74§‖

* Light Cars only. † Suitable for Motor Traffic. ‡ Open to Traffic for 10 miles only. § Now under bridging construction. ‖ Motorable—dry season only.

Figure B.1: Road list of the Colony of Lagos and Southern Nigeria, 1928.

2. *Coordinate-road vertex matching:* Each coordinate is then mapped onto the gROADS road network by assigning it to its closest vertex in the network.[3] Each road string is now a sequence of vertices.

3. *Vertex sequence to road mapping:* The vertices of each sequence are connected via the shortest geographical path on the gROADS network between them. In order to build a temporally coherent network, this procedure is started with the temporally most recent road list, and then proceeds backwards in time, ending with the oldest road list. This procedure gives preference to vertex-connecting paths that use roads which are present in the networks in the years to come, thus making the ensuing time-variant network temporally consistent. If a place name in a road string has been matched to more than one coordinate and vertex, all possible shortest paths of that particular road are calculated. The shortest one is then chosen as a conservative solution to the problem of duplicate placenames.

With the resulting data on colonial, I construct planar transport networks that consist of footpaths, railroads, and roads (see Section B.3 below). To do so, I additionally need data on transport costs, which are described in the next subsection.

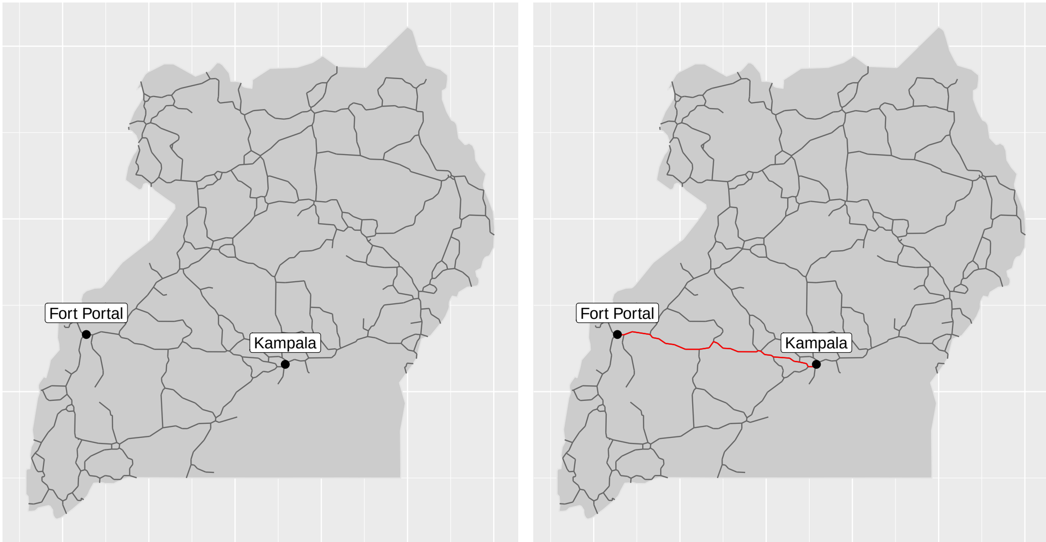
3 To avoid names to be matched with very distant vertices, I restrict this matching to a maximum distance of 0.25 decimal degrees ($\approx$ 25 km at the equator).

colony	year	road
Uganda	1940	Kampala - Fort Portal

(a) Original road string

name	longitude	latitude
Kampala	32.582	0.316
Fort Portal	30.275	0.662

(b) Matched to geonames



(c) Mapped onto road network

Figure B.2: From road lists to road maps

B.1.2 *Transport costs*

Since each edge in the network is assigned to being a footpath, a road, or a railroad, we need information on the transport cost per ton mile for each of these transportation modes in order to be able to calculate the transport cost on any path p consisting of edges $e_1, e_2, ..., e_p$ in the network:

$$cost_p = \sum_{e=0}^{E_p} length_e / quality_e$$

Information on the parameter $quality_e$ is scarce and imprecise. Transport costs by foot, truck, or rail varied widely over time and space (see Figure B.3). In particular, road transportation costs sharply declined as more vehicles toured the colonies and road quality improved.

To keep the analysis traceable, relative transportation costs are assumed to be stable over time and space. For all baseline estimates, I assume costs of $foot : road : rail$

to be 40 : 20 : 5 constant 1900 Pence. This approximates the values observed in the mid-1920s.

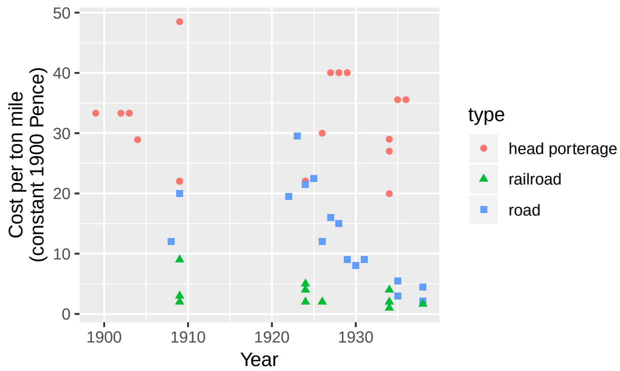


Figure B.3: Transportation costs in various British colonies.
Data from: Chaves, Engerman and Robinson (2013); Hailey (1945); Hopkins (1973), and the Annual Colonial Reports for the Gold Coast. I retrieve unskilled labor wage data from the latter (1899–1936). With these, I follow Chaves, Engerman and Robinson (2013) and calculate head porterage costs assuming that one man carried 60 lbs. (ca. 27 kg) over 15 miles (ca. 22.5 km) per day.

B.1.3 *Cross-validation with the Michelin data*

Table B.1: Correlation between road densities from Blue Books and Michelin

	Roads Blue Books, 1950			
	(1)	(2)	(3)	(4)
Constant	0.717***	0.906***		
	(0.247)	(0.279)		
Roads density, Michelin, 1966	0.870***		0.808***	
	(0.074)		(0.075)	
Roads qual.-weight., Michelin, 1966		0.016***		0.016***
		(0.001)		(0.001)
Colony FE:	no	no	yes	yes
Mean DV:	2.9	2.9	2.9	2.9
Observations	1,503	1,503	1,503	1,503
Adjusted R^2	0.317	0.356	0.514	0.564

*p<0.1; **p<0.05; ***p<0.01
Standard errors are clustered on the province level.

In order to gauge the validity of the reconstructed colonial road networks, I conduct a brief cross-validation exercise. I therefore use the Michelin data observed independently in 1966 (the data is introduced below in Section B.2). In particular, and drawing on the Voronoi units used as units of observation in Chapter 5, Table B.1 shows the results of a set of simple regressions of the road densities constructed from the Blue Books in 1950 and those observed on the Michelin maps in 1966. Model 1 regresses 1950 road densities on simple road densities in 1966 and Model 1966 improves this measure by taking into account the quality of roads as indicated on the Michelin maps. I expect that higher quality roads are more often enlisted in the Blue Books than dirt roads. The higher R-squared of Model 2 supports this expectation. Models 3 and 4 then add colony fixed effects. The models suggest that the Michelin data highly correlates with the Blue Book data, yielding an estimate that is barely lower than 1 in Model 1. Finally, the Michelin data explains between 32 and 36 percent of the variation in the local road densities constructed from the Blue Book data (see Models 1 and 2).

B.2 ROAD DATA FROM THE MICHELIN MAP CORPUS

This section provides the dateils on how Philipp Hunziker and I digize the Michelin map corpus. Subsection B.2.1 describes the map corpus, Subsection B.2.2 introduces the extraction of information from maps as a computer vision task, which leads us to discuss the architecture of our Fully Convolutional Neural network in Subsection B.2.3. The two following subsections present the results of the digitization procedure as well as the coding of travel speeds.

B.2.1 *The Michelin map corpus*

The source for road network data for post-colonial Africa is the African Michelin map corpus, a collection of large topographical maps at a resolution of 1:4,000,000. Each map shows detailed information on road infrastructure with a consistent cartographic symbology for about a third of the continent (see Figure B.4). While coverage before the 1960s is sporadic, Michelin has covered the entire African continent at intervals of approximately 5 years beginning in 1964 (see Figure B.5). This makes the Michelin corpus an unparalleled source for time-variant road-network information. In this dissertation, I make use of 34 map sheets published between 1964 and 2017, which combine into 23 maps of the entire continent.

We digitize this map collection automatically. Apart from being relatively cheap, the automatic digitization approach features a number of additional benefits:

- *Consistency:* The cartographic information is extracted in a highly consistent manner, avoiding errors due to human fatigue and less-than-perfect inter-coder reliability.
- *Replicability:* The entire data set can be reproduced at will.
- *Extendability:* After the initial system is set up, the marginal costs of adding new cartographic material (including from other sources) are low.

This section is slightly adapted from the Working Paper "Roads to Rule, Roads to Rebellion: Relational State Capacity and Conflict in Africa", co-authored with Philipp Hunziker (now at Google) and Lars-Erik Cederman (ETH Zurich). The neural network described below has been developed by Philipp Hunziker, we jointly implemented the machinery to transform the road data into planar networks, and I extended the corpus of networks from the initial 6 to the current number of 23.

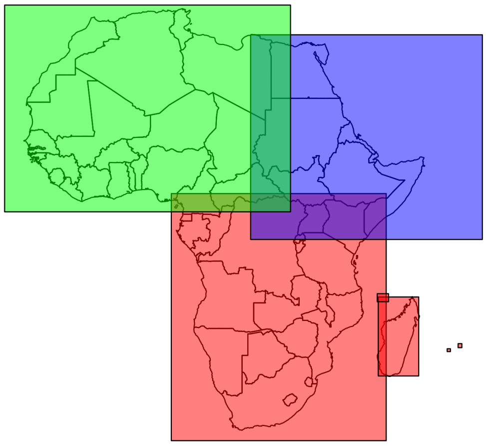


Figure B.4: Spatial coverage of Michelin Map types.
Note that Madagascar is not covered in 1966. Green: North-West. Blue: North-East. Red: Center-South.

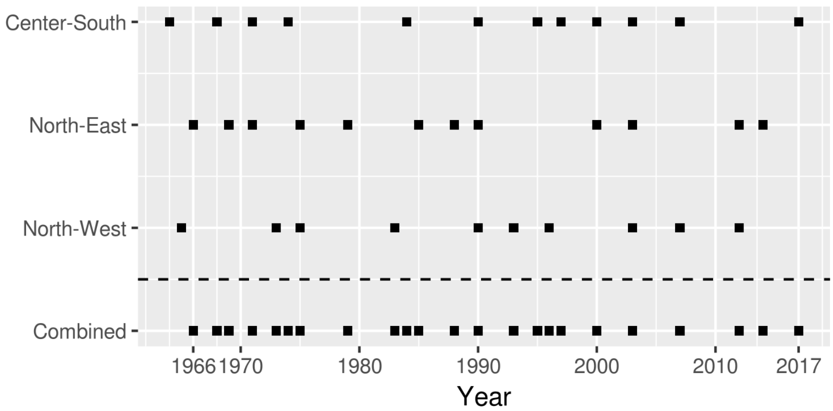


Figure B.5: Temporal coverage of Michelin Maps

B.2.2 *Map digitization as a computer vision task*

A critical first step for extracting information from geospatial imagery is to distinguish between areas representing objects of interest and background. Roads in the Michelin maps are drawn as complex features with multiple color and line-patterns, and often interrupted by other objects (see Figure B.6). Therefore, heuristic algorithms that distinguish only colors or lines fail to classify roads correctly. Instead, we implement

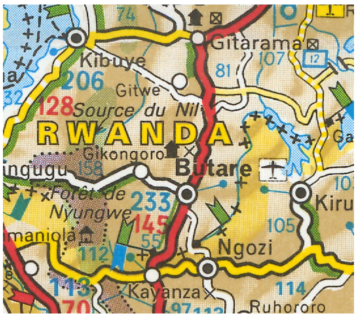


Figure B.6: Small excerpt from the 2003 Michelin map for Southern Africa, showing southern Rwanda.

a method that "looks at" entire map segments at once, and is able to distinguish between lines and other object types using contextual visual information.

To do so, we borrow from recent advancements in the machine learning literature and implement a *Convolutional Neural Network*-based system for road network extraction. Convolutional Neural Networks (CNNs) have recently emerged as a powerful method for computer vision applications, outperforming other approaches across a variety of classification problems (LeCun, Bengio and Hinton, 2015). Fundamentally, CNNs are feedforward artificial neural networks (ANNs). They consist of multiple layers of neurons, each neuron representing a non-linear function associated with a trainable weights vector, accepting a linear combination of inputs from the previous layer, and outputting a scalar that is passed on to the next layer.[4] In the language of ANNs, the vector of predictors associated with a single observation is then called the "input layer," whereas the prediction produced by the ANN is called the "output layer." Note that in computer vision problems, the input layer typically consists of raw image data, structured as a pixel-image with multiple color bands.

While the most basic variants of feedforward ANNs feature fully connected architectures where each neuron accepts inputs from *all* neurons of the previous layer, *convolutional* neural networks restrict the visual receptive field of each neuron to a small, spatially contiguous patch of input data, thus retaining the spatial structure of the inputs. Moreover, CNNs feature a shared-weights architecture, whereas neurons reuse the same set of parameters to "look" at all locations of the input image. This ensures that CNNs are shift-invariant: They are able to detect objects regardless of their spatial location in the input image.

4 The following discussion of ANNs and CNNs draws from Goodfellow, Bengio and Courville (2016, ch. 6 & 9).

Neurons in CNNs typically implement two types of operations. A *convolution* operation, computing the dot product between a patch of input data and the neuron's weights, and a pooling operation, which downsamples the input image to a lower resolution by some given factor. Productive CNNs typically feature multiple convolution- and pooling-layers in succession, giving rise to a complex non-linear function that transforms a given input image into a series of images with decreasing resolution, but higher depth, called *feature maps*.[5] This architecture gives rise to the key advantage of CNNs: their ability to learn features relevant for classification from raw, unprocessed input imagery (Zeiler and Fergus, 2014). Hence, instead of the researcher having to pre-process the input data and extract variables that are useful for classification (e.g. whether particular shapes or color patterns are present), CNNs are capable of learning important features by themselves. The layers close to the input image recognize low-level features such as edges or blobs of a particular color, which are then fed to the higher-level layers that capture more complex features at lower resolutions, like specific line patterns or shapes with particular textures.

For image segmentation, Shelhamer, Long and Darrell (2017) have recently proposed what they call a *fully convolutional* approach. Here, the feature maps produced by the regular convolutional and pooling layers are used as inputs for a set of upsampling, or *deconvolutional* layers. These consist of neurons that implement a reverse convolutional operation, mapping lower resolution feature maps onto higher resolution outputs via a trained interpolation function. Hence, fully convolutional neural networks (FCNNs) have an "abstraction stage," where convolutional and pooling layers learn to recognize complex image features, and a "drawing stage." Here, the information from the lower-resolution feature maps is mapped back onto the scale of the original input image, yielding a full semantic segmentation.

B.2.3 *Our FCNN: Architecture and training*

To solve the semantic segmentation problem on the Michelin map material we implement a version of Shelhamer et al.'s (2017) FCNN model that takes RGB image patches of dimension $512 \times 512 \times 3$ pixels as input, and maps them onto output segments of size $512 \times 512 \times 17$. The output image depth arises from the fact that

5 Here, "depth" refers to the third dimension of an image. An RGB image has depth 3.

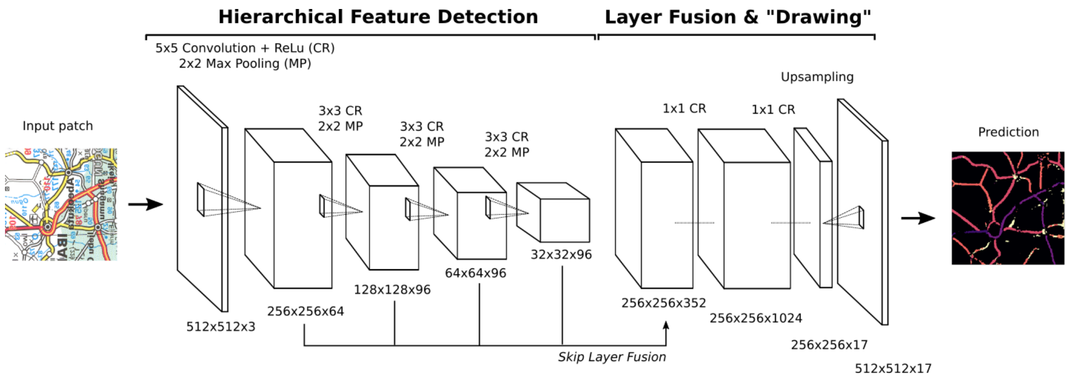


Figure B.7: Architecture of our custom fully convolutional neural network.

Michelin identifies 16 road categories.[6] The precise architecture of our model is shown in Figure B.7. The model is described in canonical notation, see Goodfellow, Bengio and Courville (2016) and Shelhamer, Long and Darrell (2017) for more information.

We pursue a transfer-learning approach and pre-train the FCNN on 2000 artificial map images.[7] These are color-images of dimension $512 \times 512 \times 3$ that superficially look like real road maps, but which we create programatically by drawing arbitrary planar networks together with other map-like shapes and text labels of arbitrary color, size, orientation, etc. Each simulated map image is paired with a "ground truth" label of dimension $512 \times 512 \times 2$ that highlights the location of the road-network to be detected. With the pre-trained model, we then proceed to the training of the main model using actual, hand-annotated training data from the Michelin maps.[8]

Interpreting trained artificial neural networks is notoriously difficult, as the learned parameters have little intuitive meaning by themselves. However, one commonly employed strategy is to show the neural activations of the network's feature maps for some input image. Six such feature maps shown in Figure B.8 demonstrate how different neurons capture different types of information. The feature maps in the top row appear to recognize numbers, those in the bottom row identify road-related features. They also show that feature maps at lower resolutions tend to capture more abstract, higher-level objects, reflecting the hierarchical logic of feature detection in CNNs.

6 An additional reference category identifies background pixels.

7 For pre-training, the outcome layer is only of depth two (instead of 17) because construct the artificial training labels such that they only identify the *presence* of roads, but not their type.

8 All layers up to the second-to-last one (exclusive) are initialized with pre-trained weights, whereas the weights of last two layers are initialized randomly. For training both the initial "artificial" model as well as the final model, we use the stochastic gradient descent (SGD) based optimizer introduced by Kingma and Ba (2015) with a batch-size of 2.

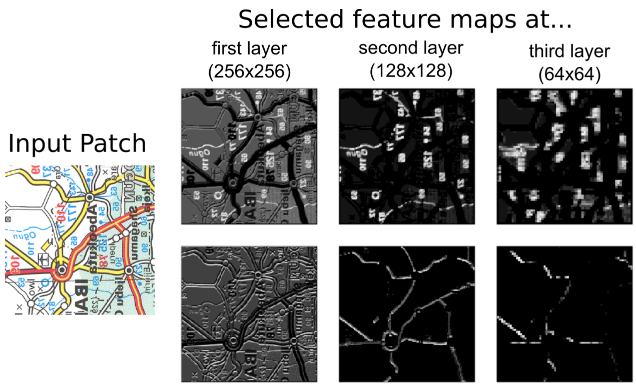


Figure B.8: Selected feature maps from the trained FCNN.

Finally, it is instructive to demonstrate the trained FCNNs predictive performance visually. Figure B.9 shows an excerpt from a 1965 map segment for Southern Nigeria (left panel), together with the corresponding road predictions obtained from the trained FCNN model (middle panel). The different colors in the predicted image correspond to different road types. We highlight that the FCNN is able to distinguish between even subtle differences in line types, e.g., lines of the same color, but with different border thickness.

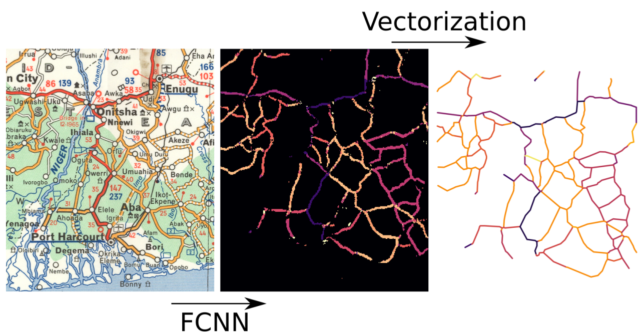


Figure B.9: Predictive performance of the digitization procedure.

B.2.4 *Vectorization and results*

Given the FCNN predictions, we implement and apply a four-step algorithm to convert the pixel-based FCNN output into vectorized road-network data:

1. The Zhang-Suen *topological thinning* algorithm is applied to the input images, leading to single-pixel-width road representations.

	Binary	Categorical
Precision	0.988	0.888
Recall	0.986	0.964

Table B.2: Evaluation statistics for the full digitization pipeline based on a hold-out sample.

2. The thinned images are fed to a *line-tracing* algorithm, transforming the road network information to a vector-based representation.

3. A *line-splicing* algorithm is then applied to fill small, unlikely gaps in the vectorized road network.

4. A sequential, *hidden-Markov* style model is used to smooth the road type classification, leading to the removal of short segments with misclassified road types.

The right-most panel of Figure B.9 illustrates the result of this vectorization procedure. To assess the accuracy of the digitization pipeline, we generate vectorized predictions for two hand-coded hold-out maps, each covering about 1000 square kilometers. We split the ground-truth and predicted networks into 5 km long road segments and calculate two evaluation metrics.[9] *Precision* measures the proportion of predicted road segments that are proximate to a ground-truth road segment. *Recall* measures the proportion of ground-truth road segments that are proximate to a predicted road segment. We use 5 km error bands (≈1mm on the original maps) to establish whether two road segments are proximate. We also calculate variants of these metrics that take road types into account. Here, predicted and a ground-truth segments are only coded as proximate if they are also of the same road type.[10]

The result of this evaluation exercise is summarized in Table B.2. We find that our digitization procedure is highly accurate. Over 98.8% of all extracted roads are present in the Michelin maps, and 98.6% of all Michelin roads are extracted. The corresponding figures are somewhat lower if we take road categories into account, but still 89.3 and 96.7, respectively. We note, however, that in those cases where the model misclassifies the road type, the error is typically small. Across all cases where roads are correctly extracted but assigned the wrong category, the mean absolute error on

9 All metrics are based on length-weighted averages.

10 Note that for this evaluation, we employ the 6-category road type coding used in the paper, not the 16-category coding used during digitization.

the ordinal road-type scale is 1.38. In other words, misclassifications typically take the form of a partially improved road erroneously being classified as an improved road, rather than an earth road being mislabeled as a highway. The lower category-precision is due to very small stretches of missclassified roads that should only marginally affect the estimates of travel times. In addition, we see no reason to believe that the FCNN introduces non-random errors.

B.2.5 *Retrieving travel speeds*

We obtain estimated travel speeds for each of the six main road categories in the Michelin data[11] by querying the mapping tool on the Michelin website (`www.viamichelin.com`. For each road category, we identify a random selection of trips on roads of that category, and record the travel speed returned by the Michelin querying tool (see Figure B.11a).[12] We set the traveling speed on foot-paths to 6 km (about 4 miles) per hour. This corresponds to walking-time estimates on `www.maps.google.com` (see also Jedwab and Storeygard, 2018).

B.3 NETWORK CONSTRUCTION

I transform the road data from the colonial Blue Books and the Michelin map corpus into planar graphs that uniformly covers geographic space. I do so in a step-wise process, developed together with Philipp Hunziker:

1. **Foot-path network:** The basis of the planar graphs consist of network of 8-connected 'foot-paths', shown for the case of Uganda in Figure B.10a. The graph's nodes are the centroids of a raster of population estimates from the HYDE 3.1 data (Goldewijk, Beusen and Janssen, 2010) for 1960 at a resolution of $.04167 \times .04167$ decimal degrees (or ca. 5 km at the equator) for the applications in Chapters 5 and 7, and of $.1667 \times .1667$ decimal degrees (or ca. 20 km at the

11 The 16 types of roads in the Michelin data are collapsed into the 6 main categories.

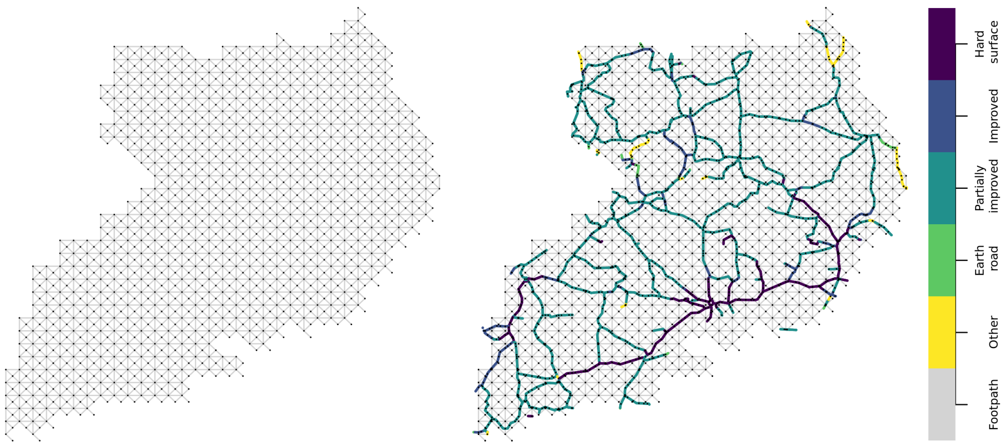
12 Note that the average speed returned for 'highways' is somewhat lower than that returned for 'hard surface' roads. Highways are almost non-existent in Africa. They constitute only .06 percent of the total road mileage observed in 1965 and cluster in the immediate neighborhood of large cities where speed is slowed by congestion. To preserve the rank-ordering of roads (which is important for our road simulation), we recode all highways as hard surface roads.

equator) for Chapter 8 which is computationally more demanding.[13] Each node is connected with a foot-path to its 8 nearest neighbors using queen moves. This setup allows for much more flexible applications than travel-query APIs such as Google Maps which do not process queries from/to points that are too distant from the next road.

2. **Adding roads:** I then overlay the basic foot-path network with the spatial lines extracted from each map corpus (see Figure B.10b). I create additional nodes wherever two roads or foot-paths cross, thus retaining the planar graph property. These additional nodes' purpose is to serve as intersections. They are not associated with any population data. Hence, travel between two populated nodes will typically start by taking a foot-path to a road, and end by traveling from a road to the target node on another foot-path. Note that the imperfect spatial alignment of road networks observed in consecutive Michelin maps causes variation in the length of these first and last foot-paths. The resulting variation in travel travel times is however deemed negligible and, importantly, random.

3. **Calculating edge weights:** Each edge on the network is associated with an edge weight which is equivalent to the estimated time it takes to traverse the edge, or, where transport costs are used, the cost of traversing it carrying one metric ton.

13 In particular, the Chapter requires the computation of the full distance matrix in each country, which is too demanding to compute on a 5×5 km raster.



(a) Foot-path network (b) ... adding Michelin roads observed in 1965

Figure B.10: Constructing road networks that regularly cover geographical space. Additional vertices are added to the graph where foot-paths and roads intersect.

B.3.1 *Network Validation: Michelin*

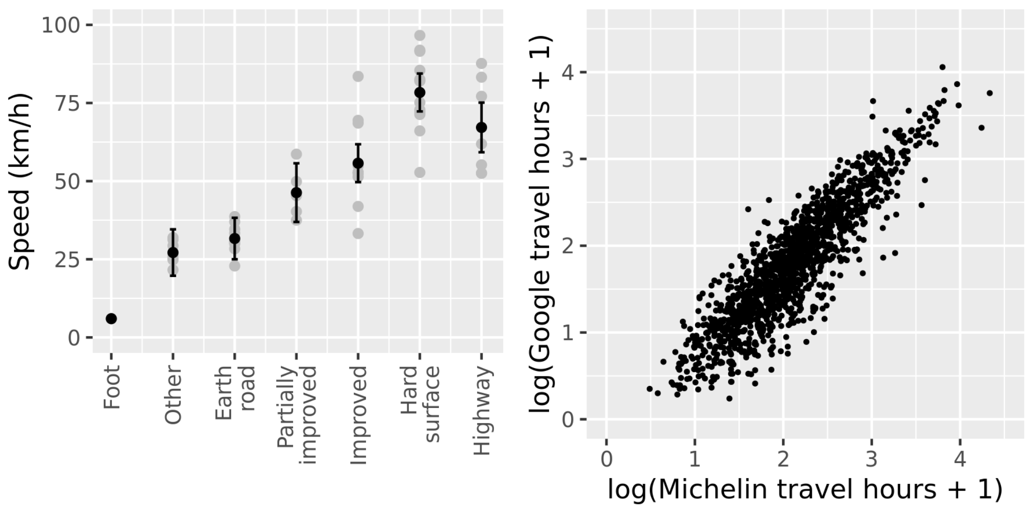
We validate the travel times computed on our Michelin-based network using the Google Maps API. Since Google only offers contemporary data, we base our comparison on road networks constructed with Michelin data from 2003.[14] For each country, we draw 50 source- and 50 destination nodes, each with a probability relative to a node's population size.[15] These nodes make up the start- and end-points of 50 paths, for which we compute both foot-travel and road-travel times on our network. We query the travel time between the two coordinates on the Google Maps API. Since the API allows only the search of geographical paths which start and end in close proximity to a road, only 60% of our queries are successful.

We compare the results from the 1,416 successful queries with our Michelin-based computations. Figure B.11b plots the two data sources against each other. The figure shows a high correlation of ≈ 1 which is least precise at low travel times. This imprecision likely results from the fact that, in certain areas, Google Maps uses data on very small roads whereas our Michelin-based networks approximate such roads as 'foot-paths'.

14 This daat validation was conducted with the original set of 6 digitized road maps, where the map from 2003 was the youngest.

15 Fewer if the country in question does not have 50 populated nodes.

This comparison does not only highlight the quality of our measurement of travel times. It also sheds light on one of the key shortcomings of Google Maps as an alternative resource for measuring travel times. Since Google Maps does not allow for querying paths between arbitrary coordinates, but makes such searches contingent on the presence of roads, it is impossible to use their services for our purposes.



(a) Estimate of travel speed on different road types

(b) Comparison of travel times on the Michelin-based road network (roads from 2003) and travel times queried from the Google Maps API.

Figure B.11: Construction and validation of edge-weights.

C

ADMINISTRATIVE UNITS IN AFRICA

C.1 DISTRICTS AND REGIONS IN BRITISH COLONIES

Table C.1: Summary of sources of district maps

colony	Observations	year	source
Côte d'Ivoire	19	1925	Huillery (2009)
Dahomey	13		
Gold Coast	35	1927	British War Office
Guinée	18	1925	Huillery (2009)
Haute Volta	11	1925	Huillery (2009)
Kenya	34	1962	George Philip and Son
Mauretanie	9	1925	Huillery (2009)
Niger	10	1925	Huillery (2009)
Nigeria	96	1962	Central Intelligence Agency
Northern Rhodesia	22	1948	British War Office
Nyasaland	21	1936	Annual Report
Senegal	14	1925	Huillery (2009)
Sierra Leone	13	1932	British War Office
Soudan	20	1925	Huillery (2009)
Tanganyika	58	1962	George Philip and Son
Uganda	15	1957	Annual Departmental Reports

Table C.1 enlists the sources for the district maps used in all analyses. Please note that I have only been able to locate precise and labeled maps on the district-boundaries in Nigeria, Kenya, and Tanganyika for the year 1962, that is shortly after these countries' independence. It seems however unlikely that the results of the analysis are purely driven by quick territorial reforms directly after independence, in particular also because the names of districts can be matched with those of local colonial administrations without problems (see below). To digitize the available maps, I use current districts obtained form the FAO (2014) GAUL Database. Since the number of districts has sharply increased over the past 60 years, I can use current units and align them to the units observed in the past, recoding boundaries only when they significantly deviate from a modern boundary. This facilitates the tracing of boundaries over time and makes up for some lack of detail in the colonial maps.

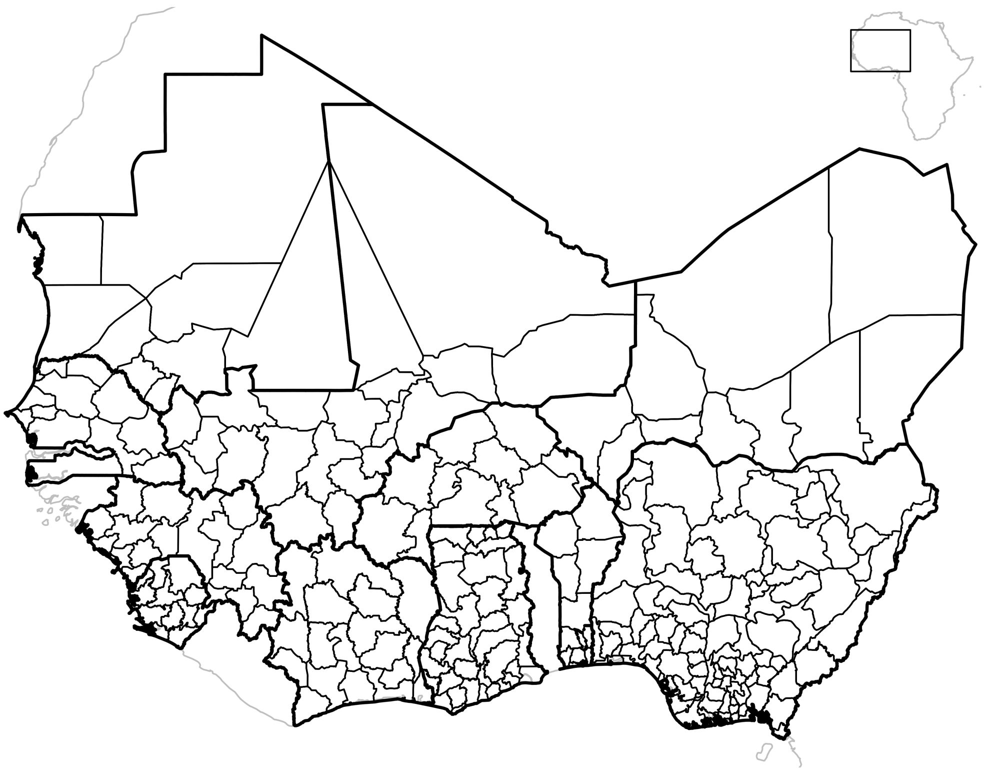


Figure C.1: Districts in West Africa.

Districts are then clustered into regions, according to the historical map material. To each district and region, I then attribute a capital by recurring to a number of sources, first the maps from the colonial period, the statoids.org data base, and where the two sources do not provide the name of district or regional capitals, a Google search. The names of capitals are then geocoded trough the geonames.org gazetteer. Table C.2 provides the summary statistics of the district-size data, and Figures C.1 and C.2 map all district boundaries used for the analyses in Chapters 4 and 5.

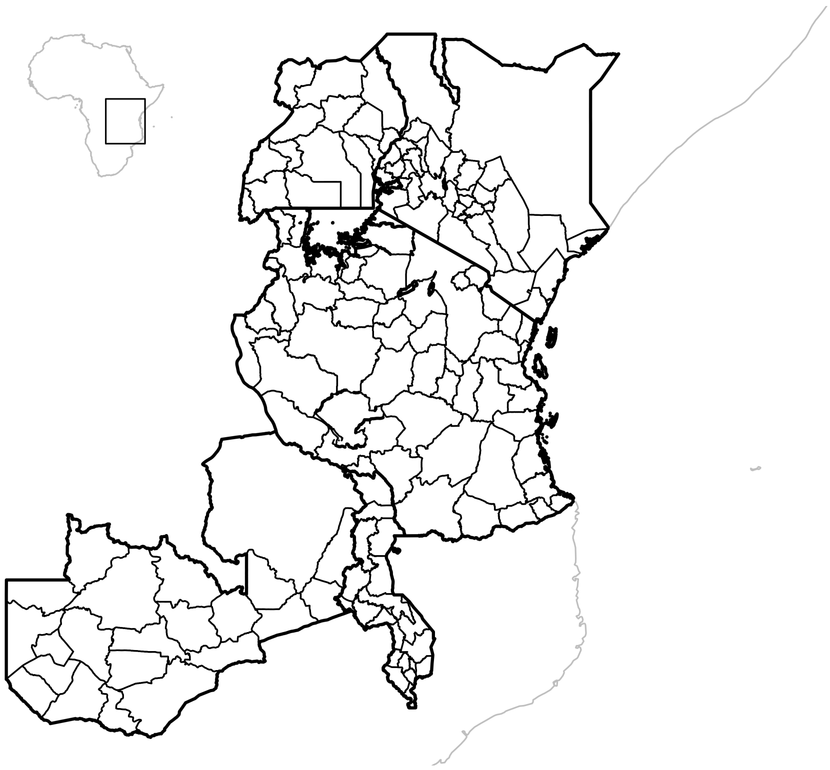


Figure C.2: Districts in Southern and East Africa.

Table C.2: Summary of district-area data

Statistic	N	Mean	St. Dev.	Min	Max
French	408	0.279	0.449	0	1
Area	408	20533.900	47162.950	42.942	520937.400
Precolonial centralization	405	1.521	0.772	0.000	3.000
Population density (log)	408	1.980	1.619	−5.029	7.475
Ethnic groups' population density (log)	406	1.973	1.449	−2.958	5.970
Distance to coast	408	4.190	3.155	−2.303	7.021
Distance to nav. river	407	4.824	0.962	2.394	6.748
Median altitude	408	529.020	526.416	3.000	2256.611
Median slope	408	3.815	1.160	1.000	9.000
Evapotranspiration	408	25.184	3.068	14.504	29.920
Precipitation	408	1652.711	253.904	1195.292	2414.136
Evapotranspiration/Precipitation	408	1258.852	598.442	56.293	3187.805
Mean temperature	408	4.535	1.462	1.000	8.000
Agricultural suitability	403	0.414	0.213	0.000	0.938
Cash crop suitability	407	0.371	0.130	0.000	0.721
Reliance on agriculture	406	2.012	1.509	0.000	8.507
Reliance on pastoralism	406	6.055	1.460	0.200	9.000
Intensity of agriculture	405	2.245	0.561	0.000	4.000

C.2 REGIONS IN POST-COLONIAL AFRICA

To collect a full panel on the borders and capitals of first-level administrative regions in Africa since countries' independence, I draw on the qualitative accounts of unit changes from the statoids.com database and encode each administrative unit change geographically. I therefore rely primarily on maps from the GAUL database, (FAO, 2014). Although this data is contemporary (reaching back until 1990), it allows me to trace back all unit-*splits* – this type of unit-change constitutes the vast majority of cases – by simply merging the units observed after the split which results in the original unit. Where units have been merged or the administrative map of a country has been redrawn completely, I make use of more than 100 digitized maps, mostly from the CIA Base Map series as well as other GIS data, such as the GADM database. Each region-period is associated with its capital, as listed in most cases by statoids.com. Missing capitals are searched on the maps and in secondary sources. The capitals are then geocoded using the geonames.org gazetteer. Changes in the location of capitals within the same boundaries of a region naturally result in new region-periods. Each region-period is associated with a start and end-year. To ensure consistent and temporally non-overlapping coding, region-periods that start after January 1st are coded as starting in the next year. The final data set covers 1763 unique region-periods, covering each African country from independence to 2016. The evolution of the number of regions in the data set is traced in Figure C.3. Figure C.4 plots the data for the year 2016.

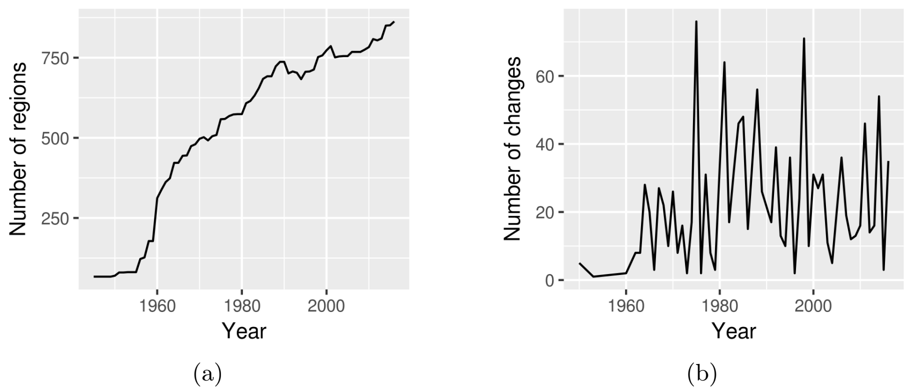
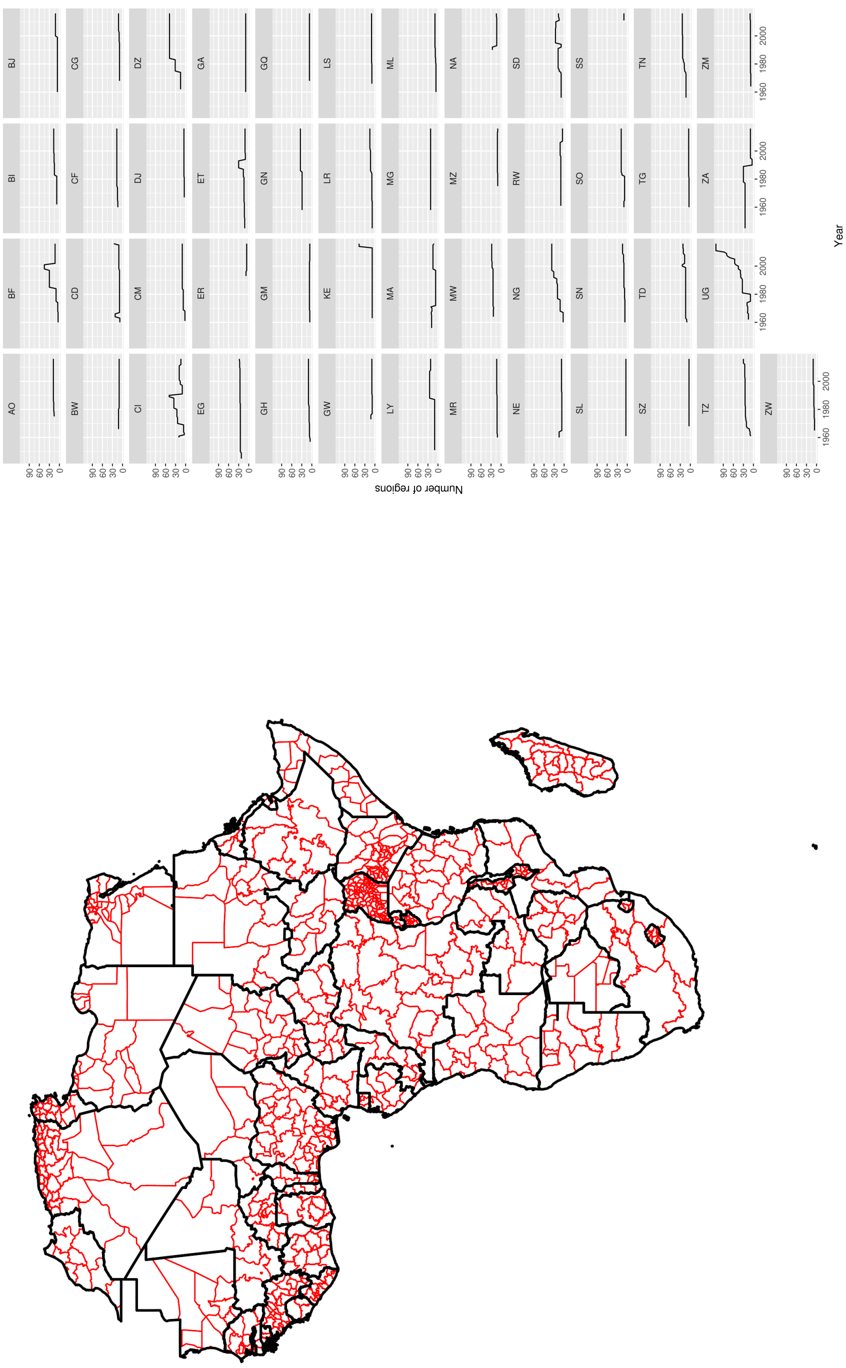


Figure C.3: Description of newly collected data on first-level administrative units in Africa.



(a) First-level administrative units in Africa 2016

(b) Number of admin-1 units by country and year

Figure C.4: Overview over first-level administrative unit data.

D

APPENDIX: DIRECT AND INDIRECT RULE

This appendix presents additional analyses and results that accompany the analysis of direct and indirect rule in French and British colonies conducted in Chapter 4. The first Section D.1 delves deeper into the analysis of the survival of precolonial states and lines of succession, and Sections D.2 and D.3 provide additional analyses of (1) the local administrative efforts of the colonizers and (2) the power of native authorities in British colonies.

D.1 SURVIVAL OF PRECOLONIAL INSTITUTIONS

This section presents a set of supplementary analyses of the effect of French and British rule on the demise of precolonial polities in Africa. Subsection D.1.1 discusses the robustness checks to the baseline models mentioned in Chapter 4. In Subsection D.1.4, I exploit variation within West Africa and along its coast to increase the internal validity of the research design. Lastly, Subsection D.1.1 shows how British and French colonization led to the death and deposition of individual rulers, mainly right after colonization.

D.1.1 *Main robustness checks*

Three types of robustness checks are applied to the baseline model including all control variables in Table 4.1 of the main text. First, Models 1 and 2 in Table D.1 address the imbalance in the sample with regards to the number of polities from the French and British empires as well as from the various colonies therein. Weighting observations such that each empire (Model 1) and colony (Model 2) receives equal weight substantially increases the coefficient associated with British rule from 1.8 to 2.4 and 2.9, respectively. This suggests that giving the British empire and the colony of Nigeria more weight in the baseline specification leads to more conservative estimates.

Model 3 stratifies the data by year[1] in order to avoid that different timings of the French and British colonization bias the results. Doing so does not change the the baseline coefficient but increases its standard error ($p < .1$). Thus, variation in the timing of colonization does not explain the difference between the French and British style of colonial conquest. Lastly, Model 4 adds additional control variables for the local disease environment measured through the local suitability for the transmission of the malaria vector between mosquitoes (Gething et al., 2011) and an estimate of the local suitability for the Tsetse fly (Programme Against African Trypanosomosis, 1999). Including the two additional controls slightly increases the estimated effect of British colonial rule.

Table D.1: British vs. French rule and the demise of precolonial polities: Robustness checks

	End of line of succession			
	(1)	(2)	(3)	(4)
British rule	-2.397^{***}	-2.914^{**}	-1.814^{*}	-2.357^{***}
	(0.847)	(1.265)	(1.036)	(0.712)
Robustness check:	empire weights	colony weights	stratified by year	desease controls
Baseline controls:	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes
Observations	4,581	4,581	4,581	4,581
R^2	0.00001	0.001	0.006	0.010
Max. Possible R^2	-0.00003	0.002	0.014	0.055
Log Likelihood	0.091	-2.248	-19.909	-105.331

Notes: Cox Proportional Hazard models. Standard errors are clustered on the polity-level. Baseline controls consist of the 1880 population density (logged), the distance to the coast (logged), the age of a polity (loged), and a linear time trend. Nature controls consist of median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls consist of the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

Lastly, the uncertainty associated with the results might vary with the level on which standard errors are clustered – so far on the level of individual polities. To gauge the effect of such clustering, Table D.2 clusters standard errors (1) not at all, (2) the level of polities (the baseline specification), (3) on the level of colonies, and (4) ethnic groups (from Murdock's Atlas, 1959). The results show that the baseline clustering on the level of polities produces the most conservative standard errors.

1 This is equivalent to adding year fixed effects.

Table D.2: British vs. French rule and the demise of precolonial polities: Standard error clustering

	End of line of succession			
	(1)	(2)	(3)	(4)
British rule	-1.785^{***}	-1.785^{***}	-1.785^{***}	-1.785^{***}
	(0.544)	(0.600)	(0.507)	(0.599)
SE clusters:	none	polity	colony	ethnic group
Baseline controls:	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes
Observations	4,581	4,581	4,581	4,581
R^2	0.009	0.009	0.009	0.009
Max. Possible R^2	0.055	0.055	0.055	0.055
Log Likelihood	-108.440	-108.440	-108.440	-108.440

Notes: Cox Proportional Hazard models. Standard errors are clustered on the polity-level. Baseline controls consist of the 1880 population density (logged), the distance to the coast (logged), the age of a polity (loged), and a linear time trend. Nature controls consist of median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls consist of the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

D.1.2 *Linear models*

Table D.3 presents the results of a linear probability model of the rate of survival of colonized polities until their respective countries' independence. The table substantiates the insights from the raw correlation of British rule with a higher survival rate plotted in Figure 4.3 in Chapter 4. The coefficient British rule in Models 1-3 shows that polities under British rule had a 25–32 percentage points higher chance of surviving colonial rule than those under French rule.

In a similar vein and to check whether the choice of estimating Cox Proportional Hazard Models drives the results, Table D.4 presents the results of liner hazard models. The models take the following specification:

$$\mathsf{h}_{i,t} = \alpha_t + \beta_1 \mathsf{British}_i + \boldsymbol{X}_1 \boldsymbol{\Lambda}_i + \boldsymbol{X}_2 \boldsymbol{\Omega}_i + \boldsymbol{X}_3 \boldsymbol{\Psi}_i + \epsilon_i,$$

where the hazard h of polity i to experience the end of its line of succession is dependent on the baseline hazard in a given year after colonization t, the identity of the colonizer (British), and the series of control variables (see above). Sequentially adding the vectors of control, the results show that, in any given year, polities under

Table D.3: British vs. French rule and the demise of precolonial polities: OLS

	Reaches independence		
	(1)	(2)	(3)
British rule	0.319^{***}	0.310^{**}	0.251^{*}
	(0.097)	(0.123)	(0.134)
Baseline controls:	yes	yes	yes
Nature controls:	no	yes	yes
Ethnic controls:	no	no	yes
Observations	116	112	102
R^2	0.128	0.227	0.272
Adjusted R^2	0.080	0.116	0.114

Notes: Linear probability models. Standard errors are clustered on the polity-level. Baseline controls consist of the 1880 population density (logged), the distance to the coast (logged), the age of a polity (loged), and a linear time trend. Nature controls consist of median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls consist of the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

British rule had a 1.6 percentage points lower hazard of experiencing the end of their line of succession. Although the uncertainty associated with this estimate increases as more control variables are added to the model, the point estimate does not change. If we aggregate this difference in the yearly hazard up to the total duration of colonial rule ($\approx$ 80 years), we arrive at almost the same difference in the average probability of surviving colonial rule than estimated above in Table D.3: $(1 - .016)^{80} = .275$. Hence, the linear models reaffirm the main insight from the Hazard Models, namely that polities under French rule were 30 percentage points less likely to survive colonial rule than those under British rule.

Table D.4: British vs. French rule and the demise of precolonial polities: OLS

	End of line of succession		
	(1)	(2)	(3)
British rule	-0.016^{***}	-0.016^{**}	-0.016^{*}
	(0.006)	(0.007)	(0.009)
Year since conquest FE:	yes	yes	yes
Baseline controls:	yes	yes	yes
Nature controls:	no	yes	yes
Ethnic controls:	no	no	yes
Observations	5,208	4,902	4,581
R^2	0.078	0.086	0.066
Adjusted R^2	0.059	0.065	0.042

Notes: Linear probability models. Standard errors are clustered on the polity-level. Baseline controls consist of the 1880 population density (logged), the distance to the coast (logged), the age of a polity (loged), and a linear time trend. Nature controls consist of median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls consist of the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

D.1.3 *Survival of rulers before and after colonization*

The data gathered from Stewart's (2006) encyclopedia on states and rulers in Africa allows us to further zoom into the demise of polities. Using the tenure time of each individual ruler for which Stewart provides us with data,[2] we can compare the average tenure lengths of rulers of the same polity before and after colonization by either the British, the French, or another colonizer. By restricting the analysis to variation within a polity, I control for all polity-specific attributes that might affect the length of rule of one ruler – in particular its political system and natural (disease) environment.

Figure D.1 plots the basic intuition behind the approach. It shows descriptively how up to the point of colonization, the probability of a ruler to be deposed in a given year does not change much. However, it rises sharply with colonization by either the British or the French although substantively more so in the case of the latter. In the first year of colonization by the French almost 50% of all rulers got deposed. In the case of British colonization, that percentage stands at 35%, as compared to a baseline probability of around 10%.

2 10 % of all rulers are associated with missing start or end dates.

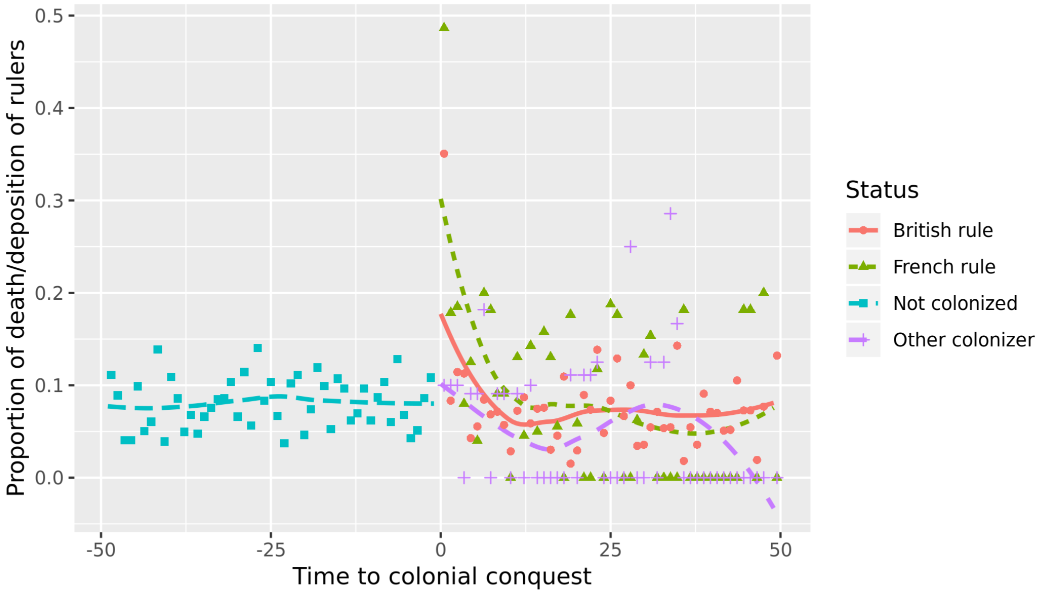


Figure D.1: Proportion of deposed rulers before and after colonization by the British, French, and other colonizers.

Modeling the data in a Cox Proportional Hazard Models stratified by each polity's capital[3] in Table D.5 shows that the difference between the effect of British and French colonization on ruler's deposition is indeed statistically significant and of meaningful size. Model 1 compares the average deposition probabilities within polities before and after colonization, in a sample restricted to observations post-1500. Colonization by the French is treated as the reference category to ease the interpretation of the coefficient of British rule which is statistically significant and of meaningful size: In a given year, rulers under British rule are 73% as likely to get deposed as under French rule. To identify the effect of colonization right when it began, Model 2 then adds linear pre- and post-trends for each of the four states in which polities can be: either not colonized, or colonized by the British, the French, or another colonizer (see Figure D.1). Model 3 adds also quadratic terms of these. Because they pick up the non-linear increase in survival rates in the years after colonization (see Figure D.1), the models with trends yield larger differences between the French and the British colonization: with quadratic trends, rulers under British rule are only 62% as likely as French rulers to be deposed or killed in the year of colonization. Lastly, by adding a dummy for the demise of a ruler's polity to the estimation, Model 4 shows that these difference

3 Stratifying by polity-capital rather than polity has the advantage that doing so holds all environmental variables constant.

are mostly due the comparatively heavy hand of the French towards the colonized polities and not only to their rulers. While the coefficient of the demise of a polity is (naturally) highly significant, the difference between the French and the British is now associated with a smaller coefficient that is statistically insignificant.

Table D.5: Death/deposition of rulers before and during colonial rule (1500–): Cox Proportional Hazards

	Death/deposition of ruler			
	(1)	(2)	(3)	(4)
British rule	-0.311^{**}	-0.431^{**}	-0.478^{**}	-0.285
	(0.158)	(0.200)	(0.230)	(0.235)
Other colonizer	0.329	0.056	-0.338	-0.127
	(0.308)	(0.367)	(0.441)	(0.443)
Not (yet) colonized	-0.278^{**}	-0.601^{***}	-0.766^{***}	-0.473^{**}
	(0.139)	(0.164)	(0.188)	(0.192)
Polity age (log)	0.001^{***}	-0.003	-0.005	-0.006^{**}
	(0.0003)	(0.003)	(0.005)	(0.003)
End of line of succession				2.614^{***}
				(0.284)
Strata:	capital	capital	capital	capital
Running linear:	no	yes	yes	yes
Running quadratic:	no	no	yes	yes
Sample:	post-1500	post-1500	post-1500	post-1500
Observations	25,281	25,281	25,281	25,281
R^2	0.001	0.002	0.002	0.005
Max. Possible R^2	0.240	0.240	0.240	0.240
Log Likelihood	-3,454.902	-3,448.180	-3,442.354	-3,415.138

*p<0.1; **p<0.05; ***p<0.01. Standard errors are clustered on the ruler-level.

D.1.4 *The demise of polities in (coastal) West Africa*

One question the previous analyses cannot fully answer is whether the results are driven by endogenous colonization choices of the French and British conquerors. Although the ruler-level analysis above (Subsection D.1.3) exploits within polity variation, it might still be that various local (e.g. environmental) factors make a certain area more or less difficult to colonize. If the British systematically colonized areas in which indirect rule was inherently easier to carry out, the results above might be solely due to that choice rather than due to the fact that the *British* rather than the *French* conquered a certain precolonial polity. In order to further zoom in on that relevant counterfactual, I exploit variation in the demise of polities first in West Africa and particularly along its coast, where the regions which where colonized by the British and the French are arguably exogenous. In that regard, Models 1 ad 2 in Table D.7 restrict the sample to all French and British colonies in West Africa. Models 3 and 4 only rely on polities observed in colonies along the West African coast.

Finally, Models 5 and 6 exploit only variation across French-British borders that run perpendicular to the West African coast.[4] Because they resulted from the race of the colonizers towards the inner parts of the continent (Cogneau and Moradi, 2014; Wesseling, 1996), these borders run at an angle of 90° from the coast and come closest to a "natural experiment" that allows us to draw counterfactual inferences. These last two specification thus stratify the Cox Proportional Hazard estimate by the perpendicular border closest to each polity. Stratified baseline hazards are estimated as a conditional logistic regression, avoiding the incidental parameter problem. The models thus compare polities only across these borders, similar to a linear model with border and year-since-colonization fixed effects. Because the relatively few polities around the perpendicular borders are unevenly distributed in space, I cannot estimate a sharp discontinuity at the borders.

Indeed, the balance Table D.6 shows that the strategy of sequentially narrowing the range of comparisons to polities in ever closer geographical areas is successful in reducing the imbalance of the sample on pre-treatment covariates of polities. However, significant imbalances of polities' distance to the costs and navigable rivers as well as of their agricultural suitability remain so that even the cross-border sample is not perfectly balanced. This underlines the need to control for observed covariates.

The results of this analysis point towards even greater differences in the probability of polities' demise in the British and French colonies than estimated at baseline. While the size of the estimated hazard ratios ranges significantly – in particular once covariates are added in Models 2, 4, and 6 – but mostly smaller than the one estimated at baseline (.23).[5] Once the model is stratified across borders, the estimates are less precise $p < .1$). In sum, these patterns suggest that the baseline results are not caused

4 From West to East: Côte d'Ivoire–Gold Coast–French Togo Mandate–Dahomey–Nigeria–Cameroon.

5 This means that, in a given year, a polity is a quarter as likely to be demised under British than under French rule.

Table D.6: Balance test, standardized coefficients

	All	West Africa	West African Coast	X-Border
Indep. variable	British	British	British	British
Dep. variable				
Population (log)	0.042	0.773^{***}	0.763^{*}	0.066
	(0.269)	(0.288)	(0.440)	(0.421)
Distance to coast (log)	0.524^{*}	0.161	0.927^{**}	1.067^{**}
	(0.307)	(0.346)	(0.412)	(0.480)
Distance to river (log)	−0.265	0.193	−0.054	-0.363^{***}
	(0.317)	(0.346)	(0.294)	(0.137)
Polity age (log)	-0.453^{**}	-0.358^{*}	−0.114	0.060
	(0.202)	(0.211)	(0.194)	(0.257)
Dependence on agriculture	-0.419^{*}	-0.558^{*}	−0.732	0.387
	(0.229)	(0.331)	(0.594)	(0.345)
Dependence on husbandry	−0.228	0.723^{**}	0.753	0.108
	(0.185)	(0.299)	(0.511)	(0.502)
Intensity of agriculture	-0.825^{***}	−0.457	-1.038^{*}	−0.764
	(0.273)	(0.375)	(0.573)	(0.571)
Precol. centralization	-0.752^{***}	-0.663^{***}	−0.317	0.205
	(0.240)	(0.253)	(0.317)	(0.253)
Altitude (median)	0.483^{**}	0.122	0.430	0.223
	(0.221)	(0.240)	(0.310)	(0.369)
Slope (median)	0.327	0.384	0.705^{*}	0.797
	(0.249)	(0.306)	(0.396)	(0.568)
Temperature (mean)	−0.287	-0.591^{**}	−0.168	0.129
	(0.300)	(0.239)	(0.312)	(0.259)
Evapotranspiration	0.068	0.107	0.487^{*}	0.433
	(0.256)	(0.251)	(0.251)	(0.267)
Precipitation	0.148	0.603^{**}	0.292	−0.215
	(0.249)	(0.258)	(0.362)	(0.283)
Evapotransp. / precipitation	0.182	0.509^{*}	0.160	−0.255
	(0.262)	(0.276)	(0.379)	(0.277)
Suitability for agr.	0.258	0.533^{**}	0.340	-0.798^{**}
	(0.272)	(0.271)	(0.396)	(0.374)
Cash crop suitability	0.395^{**}	0.423^{*}	0.743^{**}	0.193
	(0.194)	(0.251)	(0.306)	(0.407)
X-Border FE	no	no	no	yes
Obs	5208	3424	3026	2845
British	0	0	0	0
French	0	0	0	0

*p<0.1; **p<0.05; ***p<0.01. Standard errors are clustered on the polity-level.

by endogenous choices of the French and British which areas of the African continent to colonize.

Table D.7: British vs. French rule and the demise of precolonial polities in West Africa

	End of line of succession					
	All West Africa		Coastal West Africa		x-Border Coastal W. A.	
	(1)	(2)	(3)	(4)	(5)	(6)
British rule	-1.29^{**}	-2.17^{***}	-1.67^{**}	-2.52^{**}	-1.46^{*}	-5.56^{*}
	(0.54)	(0.83)	(0.74)	(1.21)	(0.86)	(3.07)
Strata:	–	–	–	–	Border	Border
Baseline controls:	yes	yes	yes	yes	yes	yes
Nature controls:	no	yes	no	yes	no	yes
Ethnic controls:	no	yes	no	yes	no	yes
Observations	3,424	3,144	3,026	2,746	2,845	2,611
R^2	0.01	0.01	0.01	0.01	0.003	0.01
Max. Possible R^2	0.06	0.06	0.06	0.06	0.03	0.03
Log Likelihood	-100.95	-82.75	-81.45	-64.52	-34.87	-20.79

Notes: Cox Proportional Hazard models. Standard errors are clustered on the polity-level. Baseline controls consist of the 1880 population density (logged), the distance to the coast (logged), the age of a polity (loged), and a linear time trend. Nature controls consist of median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls consist of the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

D.2 ADMINISTRATIVE EFFORT: ROBUSTNESS CHECKS

This section presents additional results for the analysis of the first dimension of indirect rule: the administrative effort employed by the colonial governments. This effort is proxied by two main variables: the size of colonial district, and the number of European administrators deployed at the local level. The main focus of the analysis lies on the size of districts for which data is more abundant and comparable across the French and British empires. Subsection D.2.1 presents the main robustness checks and Subsection D.2.2 discusses the more controlled comparisons of districts' sizes in the French and British colonies along the West African coastline. Lastly, Subsection D.2.3 presents the results of the analysis of the association of precolonial centralization and the number of local British administrators.

D.2.1 *Districts' size: Robustness checks*

Table D.8 presents the robustness checks to the main analysis of the effect of precolonial centralization on districts' size (Chapter 4, Table 4.4). It addresses a number of issues which might bias the baseline results. First, Model 1 drops all outliers from the sample, some of which might drive the relationship between precolonial centralization and size in British colonies. Outliers are defined as very small and large districts in the upper and lower 2.5 percentiles of the data. Dropping them does not change the positive relation between precolonial centralization and districts' size in the British colonies. In the French colonies, this relation is slightly less negative than at baseline but significantly different from that in the British sample (see the interaction term). In order to avoid excessive weight for the large colonies – in particular Nigeria – which might bias the results, Model 2 weights each observation by the inverse of the number of observations from the colony it belongs to. Giving each colony equal weight leads to very similar results as at the baseline.

I then proceed as with the analysis of the survival of precolonial politites and add a districts' disease environment (Malaria and Tsetse suitability), both of which might have reduced the administrative effort of the British. These additional control variables do not change the results (Model 3). Model 4 replaces the measure of precolonial centralization provided by Murdock (1959) with a dummy for whether a district featured a capital in 1885 of one of the polities listed in Stewart's (2006) encyclopedia of African states and rulers (see Appendix A.1 above) or not. The emerging pattern is consistent with the previous results: Districts in the British empire that featured a capital in 1885 are about 65 percent bigger than those that did not.[6] This pattern is not discernible in French West Africa. This shows that the results are not due to arbitrary codings which might bias Murdock's data. Model 5 replace the the mapping of Murdock's Ethnographic Atlas (1967) to his ethnic map (1959) conducted by Nunn and Wantchekon (2011) with the slightly different coding from Michalopoulos and Papaioannou (2013*b*). While their data lead to five more missing values, the results are very similar to the baseline estimates.

6 This percent estimate results from the following equation: $(exp(\beta) - 1) * 100$

Table D.8: Precolonial centralization and the size of districts: Robustness checks

	log(District Area)				
	No outlier	Col.-weight	Disease	Cap. 1885	Alt. PCC
	(1)	(2)	(3)	(4)	(5)
Precol. centralization	0.14^{**}	0.13^{***}	0.13^{***}		
	(0.05)	(0.05)	(0.05)		
Precol. centr. × French	-0.29^{***}	-0.27^{***}	-0.33^{***}		
	(0.11)	(0.10)	(0.10)		
Capital 1885				0.50^{***}	
				(0.14)	
Capital 1885 × French				-0.36^{**}	
				(0.18)	
Precol. centr. (MP)					0.17^{***}
					(0.05)
Precol. centr. (MP) × French					-0.30^{***}
					(0.11)
Colony FE:	yes	yes	yes	yes	yes
Colony weights:	no	yes	no	no	no
Desease controls:	no	no	yes	no	no
Baseline controls:	yes	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes	yes
Mean DV	9.11	9.14	9.14	9.14	9.15
Observations	383	400	400	400	395
Adjusted R^2	0.69	0.81	0.72	0.73	0.74

Notes: OLS models. Standard errors are clustered on the province-level. Baseline controls include the local population density, ethnic groups' population density, and the distance to the coast as well as the closest navigable river. Nature controls consist of the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls are the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Additionally, all covariates are interacted with 'French rule'. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

D.2.2 *Districts' size across French-British borders*

In the baseline specification, the identification of differences in districts' sizes within colonies and the difference of these patterns across the two empires rests on the assumption that there are no omitted variables. We can weaken this assumption and focus only on the *difference* of the effect of precolonial centralization on district sizes between the French and British empire. The identification of this difference rests on the assumption that French and British rule resembled a natural experiment, which is clearly not the case when comparing colonies across the entire continent. I therefore turn towards plausibly exogenous variation in the assignment of the ruling empire, the Nigeria-Benin and Gold Coast-Côte d'Ivoire borders in West Africa. Both borders are perpendicular to the coast line and emerged from a race of both colonizers towards the inner part of the continent (Wesseling, 1996). They can therefore be treated as-if random (Cogneau and Moradi, 2014) to identify the difference in the effect of precolonial institutions on administrative effort under French direct and British indirect rule.

In order to exploit the change in the effect of precolonial centralization on districts' size at the border, I turn towards an approach based on the centroids of grid cells. Using grid cell centroids as the main unit of the regression discontinuity design is warranted by the need to balance the number of observations across the French-British borders. Such balance is not achieved if one compares districts of varying size (which is the dependent variable), because larger districts are observed less often. Grid-cells in the main analysis[7] have a size of .0833 decimal degrees or about 10km at the equator. Each cell centroid is associated with the size of its district, the precolonial centralization of the ethnic group settling in it (from Murdock, 1959), as well as its distance to the next border. Because I am interested not in the pure effect of British or French colonial rule at the border, but its effect on the marginal effect of precolonial centralization, I estimate the following regression discontinuity:

$$\begin{aligned} y_i = \alpha_c + \gamma_b + \beta_1 \mathsf{precol.\ centr.}_i \times \mathsf{French}_i + \tau_1 \mathsf{Empire}_i \times \Delta_i + \\ \tau_2 \mathsf{precol.\ centr.}_i \times \mathsf{Empire}_i \times \Delta_i + \tau_3 \mathsf{precol.\ centr.}_i \times \mathsf{Border}_i + \epsilon_{i,p} \end{aligned} \tag{D.1}$$

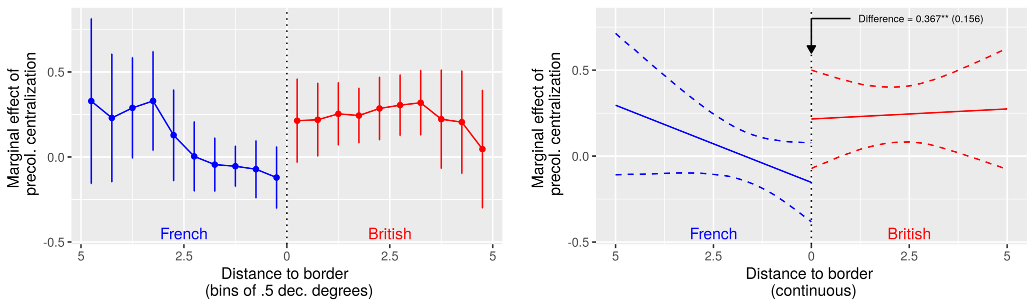
The logic of this RD-design is illustrated in Figures D.2a and D.2b. The first figure plots the coefficient of precol. centralization on districts' size left and right of French-British borders in intervals of .5 decimal degrees. The second plots the marginal effect of centralization as a linear function of the distance to the border. As in common RDDs, we notice the trends in the effect of centralization on district sizes on both sides of the border. With Δ_i denoting the distance to the border, the absolute trends in district sizes are controlled for by the term $\mathsf{Empire}_i \times \Delta_i$, while the trend in the effect of centralization is captured by the term $\mathsf{precol.\ centr.}_i \times \mathsf{Empire}_i \times \Delta_i$. To account for different levels in the effect of centralization in the two border-regions, I

7 See Figure D.4 for a robustness check that varies the size of grid cells.

Table D.9: Balance test: Grid-cell level

	All	RDD	RDD
Indep. variable	Centr. × French	Centr. × French	Centr. × French
Dep. variable			
Distance to coast (log)	-0.429^{***}	-0.177^{*}	-0.060
	(0.109)	(0.101)	(0.100)
Distance to nav. river (log)	-0.140	-0.236	-0.350
	(0.116)	(0.302)	(0.214)
Population density (log)	0.345^{***}	0.194	0.147
	(0.100)	(0.167)	(0.120)
Ethnic groups' pop. dens. (log)	0.490^{***}	0.240	0.065
	(0.108)	(0.184)	(0.124)
Dependence on agriculture	-0.257^{*}	0.064	0.331
	(0.150)	(0.361)	(0.327)
Dependence on husbandry	0.113	-0.491	-0.503
	(0.138)	(0.441)	(0.436)
Intensity of agriculture	0.401	0.208	0.368
	(0.261)	(0.350)	(0.338)
Altitude	-0.064	-0.441^{**}	-0.617^{***}
	(0.087)	(0.220)	(0.167)
Slope	0.195^{*}	-0.283	-0.065
	(0.101)	(0.179)	(0.136)
Temperature	0.058	0.149	0.418^{***}
	(0.072)	(0.231)	(0.153)
Evapotranspiration	-0.089	0.059	0.135^{*}
	(0.091)	(0.066)	(0.073)
Precipitation	0.183^{**}	0.087	-0.073
	(0.072)	(0.173)	(0.159)
Evapotransp. / precipitation	0.153^{**}	0.125	-0.098
	(0.071)	(0.125)	(0.144)
Suitability for agr.	0.063	-0.366^{**}	0.021
	(0.074)	(0.152)	(0.164)
Cash crop suitability	0.156^{**}	0.074	-0.461^{**}
	(0.062)	(0.193)	(0.191)
RD-Design	no	yes	yes
Cutoff (dec. degrees)	–	5	2.5
Obs	92954	13455	6456
British	0	0	0
French	0	0	0

*p<0.1; **p<0.05; ***p<0.01. Standard errors are clustered on the province-level.



(a) RDD-plot discrete (b) RDD-plot continuous

Figure D.2: Marginal effect of precolonial centralization left and right of French-British borders.
Point estimates in (a) and liner trends in (b) results from estimating Equation D.1. For point estimates in (a), the continuous measure of the distance to the border Δ_i is cut into categorical bins of a size of .5 decimal degrees.

include the fixed slopes $\mathsf{precol.\ centr.}_i \times \mathsf{Border}_i$. Adding colony and border-segment fixed effects[8] α_c and γ_b, the main coefficient of interest, β_1 is driven by the jump in the marginal effect of precolonial centralization right at the border. To account for interdependencies between grid-cells and districts that are part of the same region, standard errors remain clustered on the level of provinces.[9]

Figure D.2a shows that the trend in the effect of centralization on both sides of the border is reasonably smooth and well approximated by a linear term. Also, the plot shows a discrete jump of centralization's marginal effect on districts' size at the border. Lastly, the RD-design requires that precolonial centralization has no such jump in its marginal association with any other pre-treatment variable. If that is the case, these pre-treatment variables, rather than precolonial centralization might drive the results. Table D.9 shows few signs of such a jump. Choosing different distance cutoffs for the analysis at 5 and 2.5 decimal degrees ($\approx$500 and 250 km) balance is best for the wider bandwidth. Here, $\mathsf{precol.\ centr.}_i \times \mathsf{French}_i$ is only significantly related to cells altitude and agricultural suitability. Because this imbalance might drive the results, I estimate models with and without all co-variates as well as their interaction with French rule.

Table D.10 presents the results. The first two columns show that precolonially centralized cells in the whole sample have become part of larger districts in the British,

8 Note that I cut borders into segments according to distance-bins to the coastline of 100 km in order to increase the balance in the sample. This avoids that points in the North of Nigeria are compared with those in the South of Dahomey (Benin).
9 Note that clustering on the level of districts leads to slightly smaller standard errors.

but not the French colonies. This suggests that the results from the district level analysis carry over to the cell-level analysis. Models 3 and 4 then implement the RDD with a bandwidth of 5 decimal degrees, Models 5 and 6 with one of 2.5 decimal degrees, each time first without and then with all covariates. They all show that, at the border, the effect of precolonial centralization on district sizes decreases by about .35 log-points as one crosses from a British to a French colony. This effect of French rule on the marginal effect of precolonial centralization on district sizes is insignificantly bigger than that estimated at the baseline (.29-.33). The results are robust to the choice of bandwidth and adding the vectors of covariates. The latter suggests that the remaining and observed imbalances do not drive the results and further support the baseline estimates.

Table D.10: Precolonial centralization and the size of districts: Grid-cells, RDD at French-British borders

	All cells		Regression Discontinuity Design			
	(1)	(2)	(3)	(4)	(5)	(6)
Precol. centralization	0.266^{***}	0.184^{***}				
	(0.087)	(0.045)				
Precol. centr. × French	-0.472^{***}	-0.154^{**}	-0.367^{**}	-0.351^{**}	-0.370^{**}	-0.331^{*}
	(0.133)	(0.075)	(0.156)	(0.154)	(0.178)	(0.175)
Colony FE:	yes	yes	yes	yes	yes	yes
Border-region FE:	no	no	yes	yes	yes	yes
Dist2border × French:	no	no	yes	yes	yes	yes
Dist2border × French × Precol. centr.:	no	no	yes	yes	yes	yes
Dist. cutoff (dec. degr.):	–	–	5	5	2.5	2.5
Baseline controls:	no	yes	no	yes	no	yes
Nature controls:	no	yes	no	yes	no	yes
Ethnic controls:	no	yes	no	yes	no	yes
Mean DV	1.51	1.52	0.17	0.19	0.17	0.19
Observations	92,954	92,065	13,455	13,141	7,899	7,745
Adjusted R^2	0.574	0.823	0.687	0.767	0.696	0.793

Notes: OLS models. Standard errors are clustered on the province-level. Baseline controls include the local population density, ethnic groups' population density, and the distance to the coast as well as the closest navigable river. Nature controls consist of median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls are the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Additionally, all covariates are interacted with 'French rule'. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

I implement two robustness checks to this analysis. First, I continuously vary the distance-to-border cutoff between .1 and 5 decimal degrees. Figure D.3 shows no

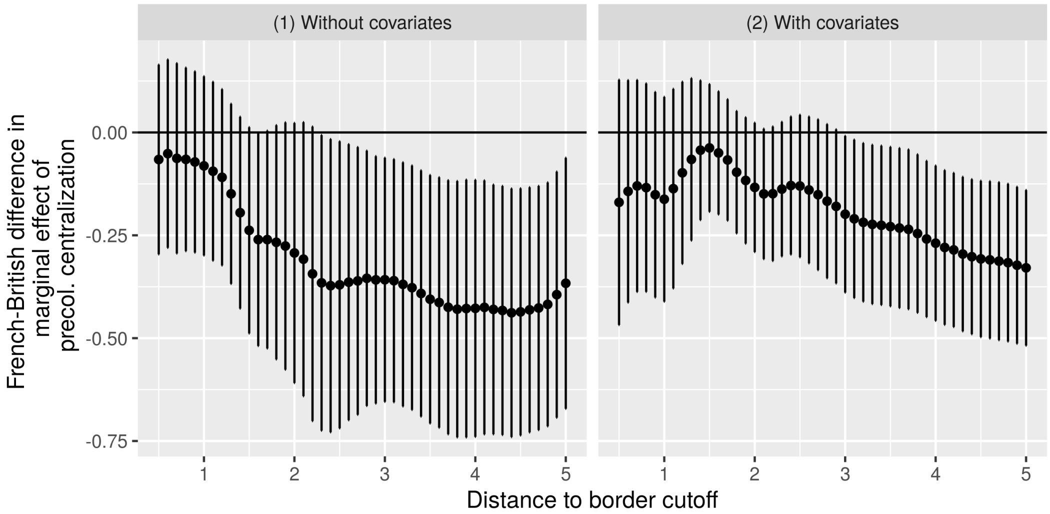


Figure D.3: French-British difference in the marginal effect of precolonial centralization with varying cutoffs of the maximum distance to the closest border.
Point estimates with 95% confidence intervals represent β_1 from Equation D.1 estimated with varying distance-to-border cutoffs.

statistically significant discontinuity in the effect of precolonial centralization on district sizes once when I restrict the sample to units very close to the border. The discontinuity becomes statistically significant with the sample of cells closer to 1.5 (2) decimal degrees in the Model without (with) covariates. The second robustness check tests whether the choice of the size of grid cells affects the analysis. Figure D.4 suggests the results to be robust to variation in the size of grid cells. It plots the results from RDD-estimates based on the the centroids of grid cells of a resolution of .083 (the baseline), .17, .25, and .33 decimal degrees. The estimated difference in the effect of precolonial centralization on district sizes at the French-British border hardly varies between the models.

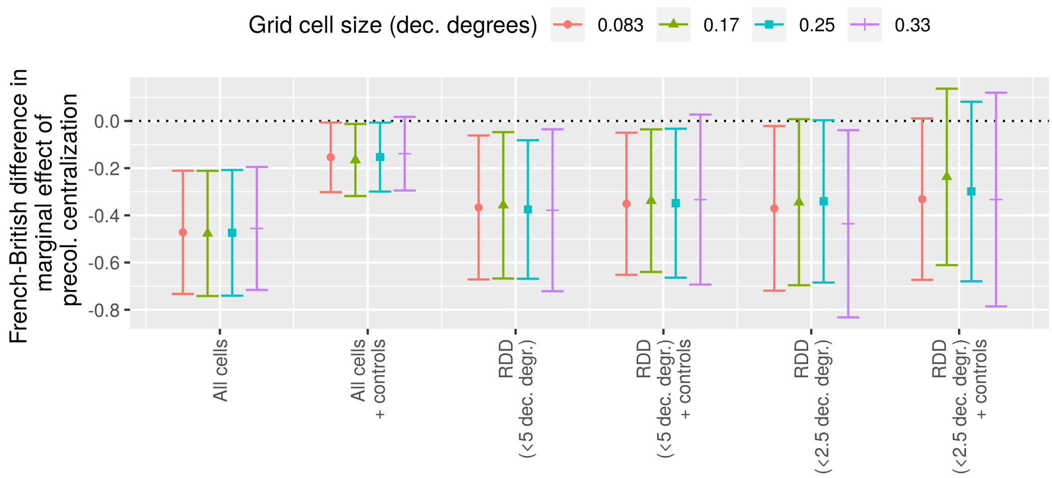


Figure D.4: Re-estimating all models in Table D.10 with varying sizes of centroids' grid cells. Point estimates with 95% confidence intervals represent β_1 from Equation D.1.

D.2.3 *British administrators*

Table D.11: Local-level European Administrators: Nigeria and Uganda

	European administrators per million			
	(1)	(2)	(3)	(4)
Precol. centralization	-2.395^{**}	-0.701	-3.597^{**}	-1.330
	(1.157)	(1.760)	(1.551)	(1.929)
Colony FE:	yes	yes	yes	yes
Baseline controls:	yes	yes	yes	yes
Nature controls:	no	yes	no	yes
Ethnic controls:	no	no	yes	yes
Mean DV:	15	15	15	15
Observations	34	34	34	34
Adjusted R^2	0.495	0.506	0.497	0.446

Notes: OLS models. Standard errors are clustered on the district-level. The sample consists of the colonies of Nigeria and Uganda. Baseline controls consist of the logged 1880 population density of the district and its ethnic groups, the logged distance to coast and closest navigable river, and, for per-capita outcomes, the logged district area and population. Nature controls are the median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls include the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

In order to test whether precolonial centralization affects not only the size of British districts, but also directly the administrative effort exerted by the British colonial government, Table D.11 presents models of the association between the centralization of precolonial polities and the number of British administrators per million inhabitants in 34 Nigerian and Ugandan provinces and districts. Although the number of observations is very small, the correlation is substantive. Without the vectors of controls added in Models 2–4, one additional level of centralization is associated with 2.4 administrators per million – a variables with a mean of only 14.8 in the sample. Adding the vector of ethnic control (Model 3) increase the size of the coefficient of precolonial centralization. However, adding the vector of 'nature' controls renders the association smaller and insignificant. While this casts doubt on the stability of the results, cautious interpretation is necessary here. First, none of the additional variables is either significant or improves the fit of the model by much. Furthermore, with 34 observations in the sample, the addition of the rather long vector of eight and later eleven controls in Model 4 likely causes multicolinearities that render the coefficients meaningless.

D.3 BUDGET ANALYSIS: ROBUSTNESS CHECKS

The following section presents additional analyses on the effect of precolonial centralization on the indigenous side of local governance, in particular native authorities' budgets. Subsection D.3.1 presents all robustness checks highlighted in the main text. Subsection D.3.2 discusses the results of an analysis of public finance data from French West Africa. And lastly, Subsection D.3.3 presents analysis on the association between precolonial centralization and the status of chiefs in colonial Nigeria.

D.3.1 *Robustness checks*

Following the robustness checks conducted in the analyses of polities' survival and district sizes, I test whether the results are driven by (1) potential omitted variables, specifically the disease environment (Malaria and Tsetse suitability), (2) the unequal weight of colonies, and (3) outliers. Furthermore, I test whether collapsing the original panel data on budgets into a cross-sectional data set biased the results. To that intent, I (4) estimate a district-weighted panel model. Furthermore, I (5) model the data in a hierarchical manner, including colony fixed effects and district random effects. As Table D.12 demonstrates, none of these changes the estimated effect of precolonial centralization on native treasuries' revenues. The estimated association between revenues per capita and precolonial centralization remains stable in size and statistical significance.

Table D.12: Per-capita revenues: Robustness checks

	Revenues p.c. (log)				
	Desease	Col.-weight	No outlier	Wght. panel	HLM
	(1)	(2)	(3)	(4)	(5)
Precol. centralization	0.24***	0.24***	0.19***	0.23***	0.23***
	(0.09)	(0.07)	(0.07)	(0.07)	(0.08)
Fixed effect:	colony	colony	colony	col.-year	col.-year
Baseline controls:	yes	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes	yes
Mean DV:	1.1	1.1	1.2	1	1
Observations	146	146	138	1,315	1,315
Adjusted R^2	0.62	0.79	0.64	0.64	
Log Likelihood					-535.27
Akaike Inf. Crit.					1,214.54
Bayesian Inf. Crit.					1,587.62

Notes: OLS models in 1–4, hierarchical linear model in 5. The sample includes the colonies of the Gold Coast (Ghana), Nigeria, Nyasaland (Malawi), and Uganda. Standard errors are clustered on the province-level. Baseline controls are the logged 1880 population density of the district and its ethnic groups, the logged distance to coast and closest navigable river, and the logged district area and population. Nature controls include the median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls are the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

Noting that the measurement of precolonial centralization might be imperfect, I also reestimate the main model using the alternative proxies for precolonial centralization in Table D.13. First, I use the Murdock-mapping of Michalopoulos and Papaioannou (2013*b*) (Models 1–2). This does not affect the estimated effect of pre-colonial centralization. I then draw on a dummy for whether a district comprises a polity's capital in 1885, based on the data on precolonial polities collected for this chapter. Districts with a capital in 1885 exhibit 76 percent larger budgets (Model 3), but not on a per-capita basis (Model 4). This might be indicative of differential effectiveness of indirect rule in rural and urban(izing) areas that developed around the old centers of society. However, it must be noted that a simple "capital in 1885" dummy is not precise enough to mirror variation in the level of centralization of precolonial polities and does not provide information about the spatial extent of its polity. Because of the resulting measurement error, the results might also biased towards zero. Also, in order to gauge the consistency of the budget data with that on the power of chiefs (see Subsection D.3.3, Models 5 and 6 finally test whether, in Nigeria, the class of the most powerful chief in a district is indeed associated with the size of native treasuries. It emerges that treasuries were 130 (58) percent bigger in absolute (per-capita) terms in districts with a "first class" as compared to those with a "second class" chief.

Table D.13: Revenues (2016 £): Alternative specifications

	Revenues (log):					
	Total	Per capita	Total	Per capita	Total	Per capita
	(1)	(2)	(3)	(4)	(5)	(6)
Precol. centr. (M&P)	0.55***	0.21***				
	(0.13)	(0.07)				
Capital 1885			0.57**	0.13		
			(0.22)	(0.15)		
Chief class					0.85***	0.46***
					(0.15)	(0.09)
Colony FE:	yes	yes	yes	yes	yes	yes
Baseline controls:	yes	yes	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes	yes	yes
Mean DV:	13	1.1	13	1.1	13	0.87
Observations	144	144	146	146	86	86
Adjusted R^2	0.58	0.61	0.52	0.60	0.50	0.24

Notes: OLS models. Standard errors are clustered on the province-level. The sample includes the colonies of the Gold Coast (Ghana), Nigeria, Nyasaland (Malawi), and Uganda. Baseline controls consist of the logged 1880 population density of the district and its ethnic groups, the logged distance to coast and closest navigable river, and, for per-capita outcomes, the logged district area and population. Nature controls are the median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls include the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

Lastly, detailed information on budget lines retrieved from the official reports allows me to further explore the implications of precolonial institutions on the financial governance of native treasuries. Although standardizing budget items across many and changing formats adds uncertainty and noise to the data,[10] it is of substantial interest to know whether the above reported patterns are driven by only a few or all budget lines. All respective results are reported in Tables D.14 and D.15. Disaggregating the revenue side shows that all revenue items are positively related to precolonial institutions, while the largest effects are visible for per capita revenues from 'fees and fines' and a category of 'other' revenues, which, inter alia, includes revenues from interests on savings. Unfortunately, the financial reporting of taxation was such that it is impossible to disentangle the amount of collected taxes from the amount of rebated taxes, which is ultimately reported in the budgets. On the expenditure side reported in Table D.15, we see significant and positive effects of precolonial centralization across almost all items, in particular items relating to per capita spending on administration, social services such as education and health, as well as expenditure for agricultural development. The one insignificant but also positive coefficient is estimated for lines spent on 'law and order'. This might be of substantive importance, given that a reading of historical accounts suggests that areas under direct control were more prone to violent resistance against British rule (e.g. Martin, 1988).

10 I standardize the varying items into their smallest common denominator in order to derive the most consistent data set possible.

Table D.14: Native treasury revenues per capita by type (2016 £)

	Revenues/capita (log)			
	Taxes	Fees & fines	Transfers	Other
	(1)	(2)	(3)	(4)
Precol. centralization	0.168**	0.272***	0.285	0.382**
	(0.081)	(0.103)	(0.607)	(0.164)
Colony FE:	yes	yes	yes	yes
Baseline controls:	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes
Mean DV:	0.53	-0.77	-5.5	-1.4
Observations	146	146	146	127
Adjusted R^2	0.412	0.707	0.680	0.473

Notes: OLS models. Standard errors are clustered on the province-level. The sample includes the colonies of the Gold Coast (Ghana), Nigeria, Nyasaland (Malawi), and Uganda. Baseline controls consist of the logged 1880 population density of the district and its ethnic groups, the logged distance to coast and closest navigable river, and, for per-capita outcomes, the logged district area and population. Nature controls are the median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls include the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

Table D.15: Native treasury expenditures per capita by type (2016 £)

	Expenditures/capita (log)					
	Admin.	Order	Educ. & Health	Agric.	Works	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Precol. centralization	0.27**	0.13	0.38**	0.94***	0.29**	0.35***
	(0.10)	(0.09)	(0.17)	(0.26)	(0.11)	(0.12)
Colony FE:	yes	yes	yes	yes	yes	yes
Baseline controls:	yes	yes	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes	yes	yes
Mean DV:	-0.098	-0.57	-0.94	-3.2	-0.23	-1.4
Observations	126	126	126	126	126	126
Adjusted R^2	0.51	0.27	0.53	0.35	0.41	0.75

Notes: OLS models. Standard errors are clustered on the province-level. The sample includes the colonies of the Gold Coast (Ghana), Nigeria, Nyasaland (Malawi), and Uganda. Baseline controls consist of the logged 1880 population density of the district and its ethnic groups, the logged distance to coast and closest navigable river, and, for per-capita outcomes, the logged district area and population. Nature controls are the median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls include the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

D.3.2 *French West Africa*

To explore whether district finances in French colonies were marked by similar or opposite dynamics, I make use of Huillery's (2010) data on tax collection, public investments, and the number of teachers and doctors in 109 French West African cercles. Unfortunately, the data are not of the same format as those collected from the British colonies. They do neither contain total local revenues and expenditures, nor do they allow for a breakdown of local budgets. With that limitation in mind, I proceed in parallel to the analysis of the British budget data, reporting results of analyses of absolute outcomes in Table D.16 and of per-capita outcomes in Table D.17. The results show that precolonially centralization had, if at all, a negative effect on the size of district budgets in French West Africa. They are thus similar in direction but not precision to those of the analysis of districts' sizes. Centralized districts had no differential tax collection, but featured lower rates of investments and numbers of teachers and doctors employed by the French. This is similar to results previously reported by Huillery (2010). In per-capita terms, only the number of doctors is significantly lower in centralized districts than elsewhere. All other indicators yield statistically insignificant results. The negative or insignificant associations highlight once again the different pattern of local governance by direct and indirect means apparent in the British and French colonies.

Table D.16: Precolonial centralization and absolute local revenues & expenditures (logged): French West Africa

	Taxes	Public works	Teachers	Doctors
	(1)	(2)	(3)	(4)
Precol. centralization	0.012	−0.347	-0.200^{**}	-0.343^{***}
	(0.180)	(0.215)	(0.098)	(0.112)
Colony FE:	yes	yes	yes	yes
Baseline controls:	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes
Mean DV:	13	13	1.8	2.2
Observations	109	109	109	109
Adjusted R^2	0.682	0.839	0.521	0.325

Notes: OLS models. Standard errors are clustered on the province-level. The sample consists of all French colonies in West Africa. Baseline controls consist of the logged 1880 population density of the district and its ethnic groups, the logged distance to coast and closest navigable river, and, for per-capita outcomes, the logged district area and population. Nature controls are the median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls include the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

Table D.17: Precolonial centralization and local revenues & expenditures per-capita (logged): French West Africa

	Taxes	Public works	Teachers	Doctors
	(1)	(2)	(3)	(4)
Precol. centralization	0.153	−0.143	−0.117	-0.274^{**}
	(0.186)	(0.189)	(0.084)	(0.104)
Colony FE:	yes	yes	yes	yes
Baseline controls:	yes	yes	yes	yes
Nature controls:	yes	yes	yes	yes
Ethnic controls:	yes	yes	yes	yes
Mean DV:	1.6	1.4	-9.6	-9.2
Observations	109	109	109	109
Adjusted R^2	0.701	0.901	0.776	0.651

Notes: OLS models. Standard errors are clustered on the province-level. The sample consists of all French colonies in West Africa. Baseline controls consist of the logged 1880 population density of the district and its ethnic groups, the logged distance to coast and closest navigable river, and, for per-capita outcomes, the logged district area and population. Nature controls are the median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls include the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

D.3.3 *Chiefs' class in colonial Nigeria*

Table D.18 summarizes the results from an analysis of the association between the level of precolonial centralization and the highest class of chiefs in Nigerian districts. The results point to a significant correspondence of the two: The most powerful chief in a district has a class (ranging from 1 to three, the highest) that increase between .28 and .48 points with each level of hierarchy featured in the districts' precolonial institutions (0-3). This is further evidence that the British devolved more power to local authorities that could build on pre-existing institutions.

Table D.18: Highest class of chief in district: Nigeria (1924/1929)

	Highest class of chief (1-3)		
	(1)	(2)	(3)
Precol. centralization	0.477***	0.288*	0.276*
	(0.088)	(0.146)	(0.139)
Colony FE:	yes	yes	yes
Baseline controls:	yes	yes	yes
Nature controls:	no	yes	yes
Ethnic controls:	no	no	yes
Mean DV:	1.6	1.6	1.6
Observations	86	86	86
Adjusted R^2	0.427	0.454	0.470

Notes: OLS models. Standard errors are clustered on the district-level. The sample consists of the colony of Nigeria. Baseline controls consist of the logged 1880 population density of the district and its ethnic groups, the logged distance to coast and closest navigable river, and, for per-capita outcomes, the logged district area and population. Nature controls are the median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, agricultural suitability, and soils' suitability for cash crop production. Ethnic controls include the reliance on agriculture and pastoralism, as well as the intensity of agricultural activities. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

E

APPENDIX: ROADS TO EXTRACTION

This appendix provides additional information to the analyses conducted in Chapter 5. Section E.1 discusses the details of the construction of Voronoi cells and compares their properties with regular, quadratic grid cells. Section E.2 presents summary statistics of the data used for the analyses, and Section E.3 presents the results of all robustness checks.

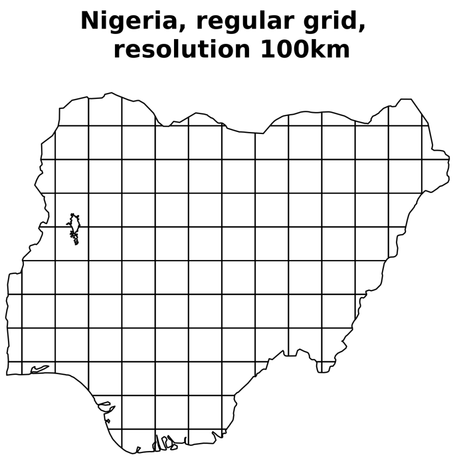
E.1 UNITS OF ANALYSIS: VORONOI CELLS VS. REGULAR GRID CELLS

In order to divide an arbitrary geographical space into units of roughly equal size and high levels of compactness (i.e. similarity to a circle),[1] this section introduces a spatial clustering algorithm that is combines the advantages of the k-means clustering algorithm (Lloyd, 1982) that requires a finite sample of points to cluster, and the Voronoi tesselation that is used to transform the centers of the k-means clusters into continuous areas. The algorithm proceeds as follows:

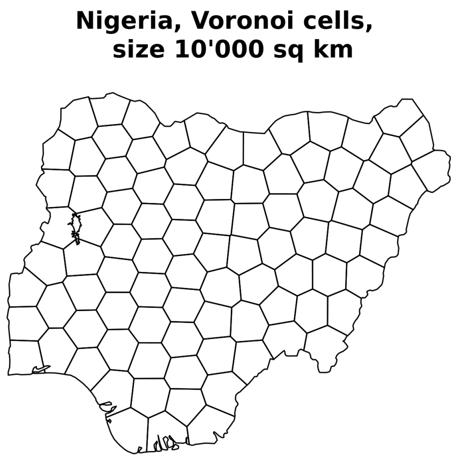
1. Draw a large number of points P from the area of polygon T (in the present case, the shape of each country / colony in the sample). This is done by intersecting a raster with a resolution of 5 kilometers (.04167 decimal degrees) with T.

2. Conduct a k-means clustering (Lloyd, 1982) of points P into N clusters, with $N = round(A_T/A_{target})$, thus N being the number of units to create so that the average area of each unit comes closest to the target size of units. For the main analysis in Chapter 5, the target size is 2500 km^2, further variations are conducted in the robustness check presented in the Section E.3 of this Appendix. For best results, initialize the k-means algorithm with a random spatial sample of N points from P.

1 The shape that can continuously cover an area with the highest level of compactness is the hexagon. However, where the honeycomb reaches a border, hexagonal cells must be cut or reshaped, thus deviating from the requirements of uniform size and compactness.



(a) Regular grid cells



(b) Voronoi cells

Figure E.1: Regular grid and Voronoi cells for contemporary Nigeria.

3. Take the centroids of the clusters thus computed and conduct a Voronoi tesselation around them.
4. Crop the resulting Voronoi polygons with the target polygon T.

We can now compare the Voronoi cells with the more commonly used quadratic grid cells. Across various target sizes, the Voronoi cells resulting from the relatively simple (and fast) algorithm significantly improve over the quadratic grid cells, both in terms of their distribution around the target size and in terms of their level of compactness. First, Figure E.2 shows that many regular quadratic grid cells are smaller than the target size – this occurs wherever a cell is cut by a border or the African coast line. Thus, the heterogeneity in units' sizes is correlated with their closeness to the coast and border, a feature which might introduce slight bias into an analysis. Second, Figure E.3 proves that Voronoi cells are much more compact than grid cells, which – as quadratic shapes and in particular where cut by borders – are shaped in a more irregular and less "circle-like" manner. Though not particularly relevant in the African context where standard geographic projection do not introduce too much of a distortion of geographical space, the Voronoi approach offers an additional benefit of being able to cover the earth with units of regular size without being affected by the geographic projections.[2]

2 This advantage is only achieved when the k-means clustering algorithm clusters points on the basis of geodesic distances between them that take into account spherical effects.

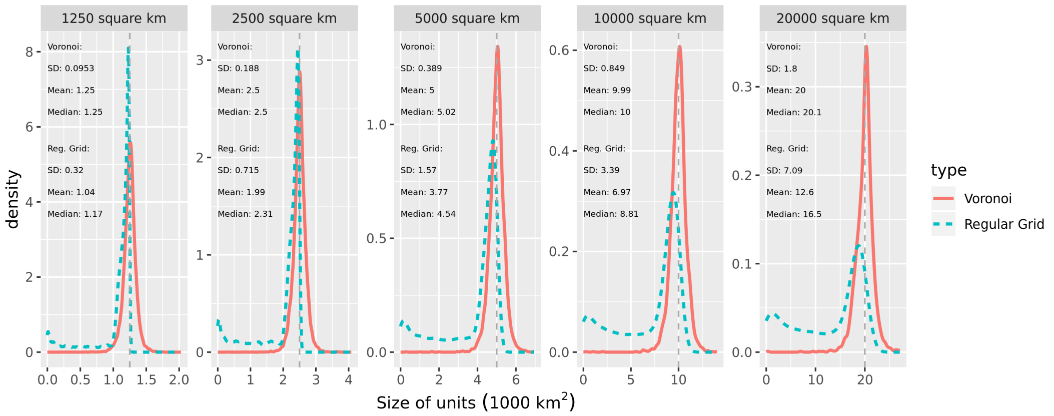


Figure E.2: Size of Voronoi and regular grid cells for varying target sizes.
Vertical dashed line indicates the target size of units. Cells are constructed for all countries in mainland Africa, using borders from the year 2000.

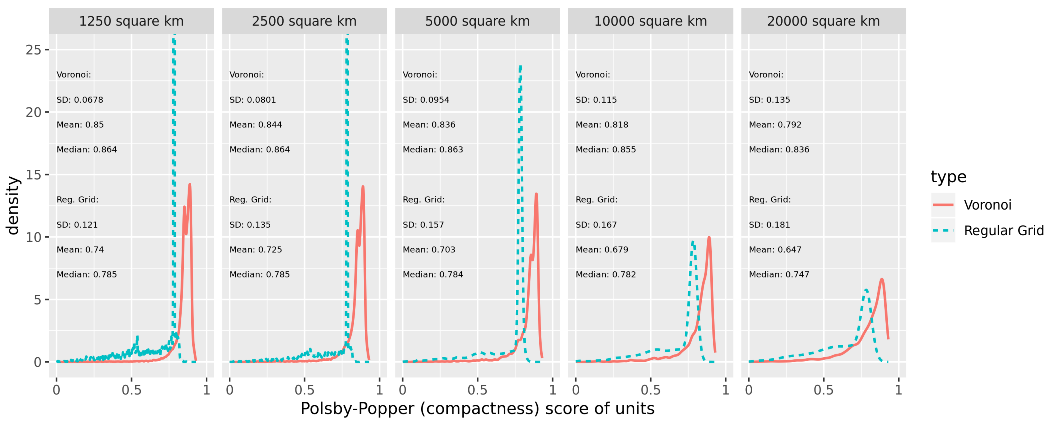


Figure E.3: Compactness of Voronoi and regular grid cells for varying target sizes.
The compactness of each unit is calculated according to Polsby and Popper (p. 349 1991) as: $(4\pi A_i)/P_i^2$, where A_i is the size of unit i and P_i its perimeter. Cells are constructed for all countries in mainland Africa, using borders from the year 2000. Note that densities above 25 are censored to improve the readability of the graphs.

E.2 SUMMARY STATISTICS

Table E.1: Summary statistics of cells in British colonies

Statistic	N	Mean	St. Dev.	Min	Max
Railroad dummy 1950	1503	0.15	0.35	0	1
Road length (km/100 km^2) 1950	1503	2.86	3.06	0.00	25.44
Transport cost to port 1950	1501	21.14	12.80	1.31	68.05
Cash crop suit. (max; 5 crops)	1503	0.37	0.12	0.03	0.77
Cash crop suit. (max; 8 crops)	1503	0.39	0.12	0.04	0.77
Cash crop suit. (mean; 5 crops)	1503	0.18	0.08	0.01	0.51
Cash crop suit. (mean; 8 crops)	1503	0.17	0.07	0.01	0.49
Mineral dep. (0/1)	1503	0.06	0.24	0	1
Population 1880	1503	23873.76	47344.27	109.65	786827.60
Area (km^2)	1503	2500.30	207.87	1019.54	3631.07
Distance to coast	1503	577.29	335.06	0.05	1269.12
Distance to border	1503	114.79	89.55	0.06	426.20
Distance to navigable river	1503	211.45	160.78	6.08	694.37
Altitude	1503	769.64	501.20	8.64	2638.75
Slope	1503	4.05	1.05	1.11	7.50
Mean temperature	1503	24.00	2.98	12.02	29.25
Evapotranspiration	1503	1715.31	253.62	1158.79	2461.00
Precipitation	1503	1119.70	431.82	282.11	3231.27
Evapotransp./Precip.	1503	4.19	1.12	2.00	8.00
Agricultural suitability	1501	0.40	0.22	0.004	0.98

Table E.2: Summary statistics of cells in post-colonial Africa

Statistic	N	Mean	St. Dev.	Min	Max
Road density (km/100 km^2) 1966	11634	1.91	1.98	0.00	11.36
Road density, (qual.*km/100 km^2) 1966	11634	86.65	103.81	0.00	775.12
Distance to port 1966	11629	29.75	22.53	1.28	136.94
Distance to nat. capital 1966	10439	29.95	23.92	1.28	143.22
Distance to reg. capital 1966	10760	18.03	19.84	0.90	134.00
Cash crop suit. (max; 5 crops)	11632	0.22	0.20	0.00	0.77
Cash crop suit. (max; 8 crops)	11632	0.24	0.21	0.00	0.94
Cash crop suit. (mean; 5 crops)	11632	0.10	0.11	0.00	0.51
Cash crop suit. (mean; 8 crops)	11632	0.10	0.11	0.00	0.50
Mineral dep. (0/1)	11634	0.05	0.21	0	1
Population 1880	11634	9238.49	26853.29	0.00	731593.40
Area (km^2)	11634	2500.72	188.41	285.08	4081.51
Distance to coast	11634	667.64	451.91	0.03	1808.09
Distance to border	11634	835.43	661.33	0.02	3308.55
Distance to navigable river	11633	431.65	490.74	6.00	2401.66
Altitude	11634	627.82	432.74	−83.47	2980.96
Slope	11634	3.85	1.39	1.00	9.00
Mean temperature	11634	24.08	3.57	2.79	30.55
Evapotranspiration	11634	1766.56	340.69	439.00	2903.50
Precipitation	11634	662.18	584.74	0.00	3231.27
Evapotransp./Precip.	11634	2.99	1.79	1.00	8.00
Agricultural suitability	11628	0.19	0.24	0.00	1.00

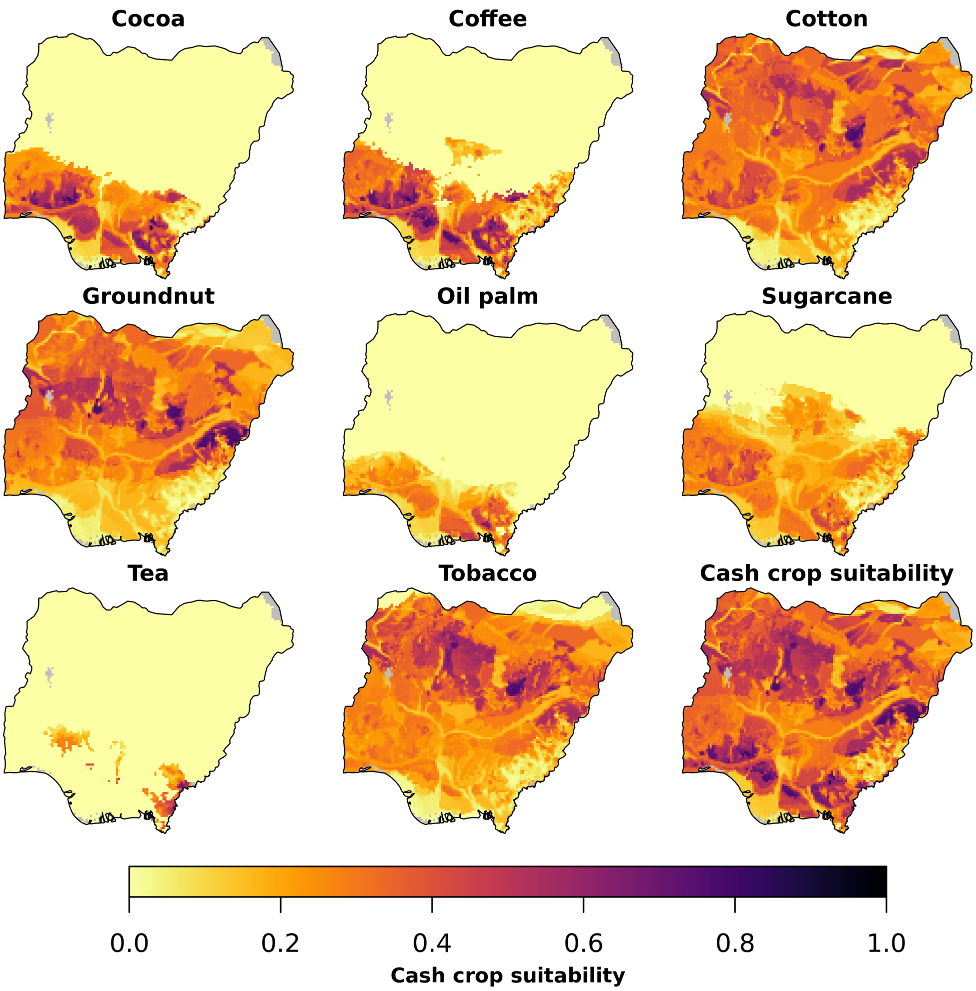


Figure E.4: Cash crop suitabilities for the eight main cash crops and the aggregate cash crop suitability score in Nigeria.
Grey value indicate missing raster data in water-covered areas. The eight cash crop suitabilities are aggregated by taking the maximum value in each grid cell.

E.3 RESULTS AND ADDITIONAL ROBUSTNESS CHECKS

This section presents all results mentioned in the discussion of the empirical analysis in Chapter 5. Moving beyond the summary plot presented in the main text, Table E.3 presents the results of the baseline specification estimated to assess the impact of local cash crop suitability and the presence of mineral deposits on the outcomes measured in the 1950 (eight British colonies) and 1966 (entire continent). Figure E.5 presents the results of most robustness checks mentioned in the text in visual form. As discussed in the main text, the robustness checks lead to few changes of the baseline results. The exceptions to this general pattern pertain in particular to the models that add (1) local road densities, (2) foot-travel times, and (3) those that drop the local population count in 1880 from the vector of control variables. The following subsections discuss the remaining sensitivity analyses. These are based on variations in the size of Voronoi cells (E.3.1), differing measures of local cash crop suitability (E.3.2), shed light on the design of administrative units and the location of capitals (E.3.3), and lastly drop the Gold Coast, which features regionally inconsistent data, from the colonial sample (E.3.4).

Table E.3: Natural resources and transport infrastructure in Africa: Baseline results

	Road 1950 (dens., log)	Road 1966 (qual.-dens., log)	Port 1950 (£, log)	Port 1966 (hrs, log)	Nat. Cap. 1950 (£, log)	Nat. Cap. 1966 (hrs, log)	Reg. Cap. 1950 (£, log)	Reg. Cap. 1966 (hrs, log)	Dist. Cap. 1950 (£, log)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cash crop suit.	1.630***	1.437***	−0.563***	−0.188***	−0.828***	−0.704***	−0.485**	−0.759***	−0.540***
	(0.587)	(0.511)	(0.184)	(0.070)	(0.243)	(0.123)	(0.241)	(0.122)	(0.171)
Mineral dep. (0/1)	0.956***	0.740***	−0.303***	−0.077***	−0.291***	−0.131***	−0.362***	−0.178***	−0.276***
	(0.209)	(0.192)	(0.073)	(0.026)	(0.088)	(0.043)	(0.077)	(0.046)	(0.066)
Country FE:	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls:	no	no	no	no	no	no	no	no	no
Mean DV:	-0.59	1.2	2.8	3.1	2.9	3.1	2.2	2.5	1.8
Observations	1,501	11,626	1,501	11,626	1,501	10,436	1,501	10,757	1,501
Adjusted R^2	0.494	0.411	0.626	0.846	0.496	0.708	0.433	0.648	0.510

Notes: OLS linear models. Standard errors are clustered on the province-level. Controls consist of the estimated population in 1880, units' area, their distance to the coast, border, and the closest navigable river, their median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Significance codes: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

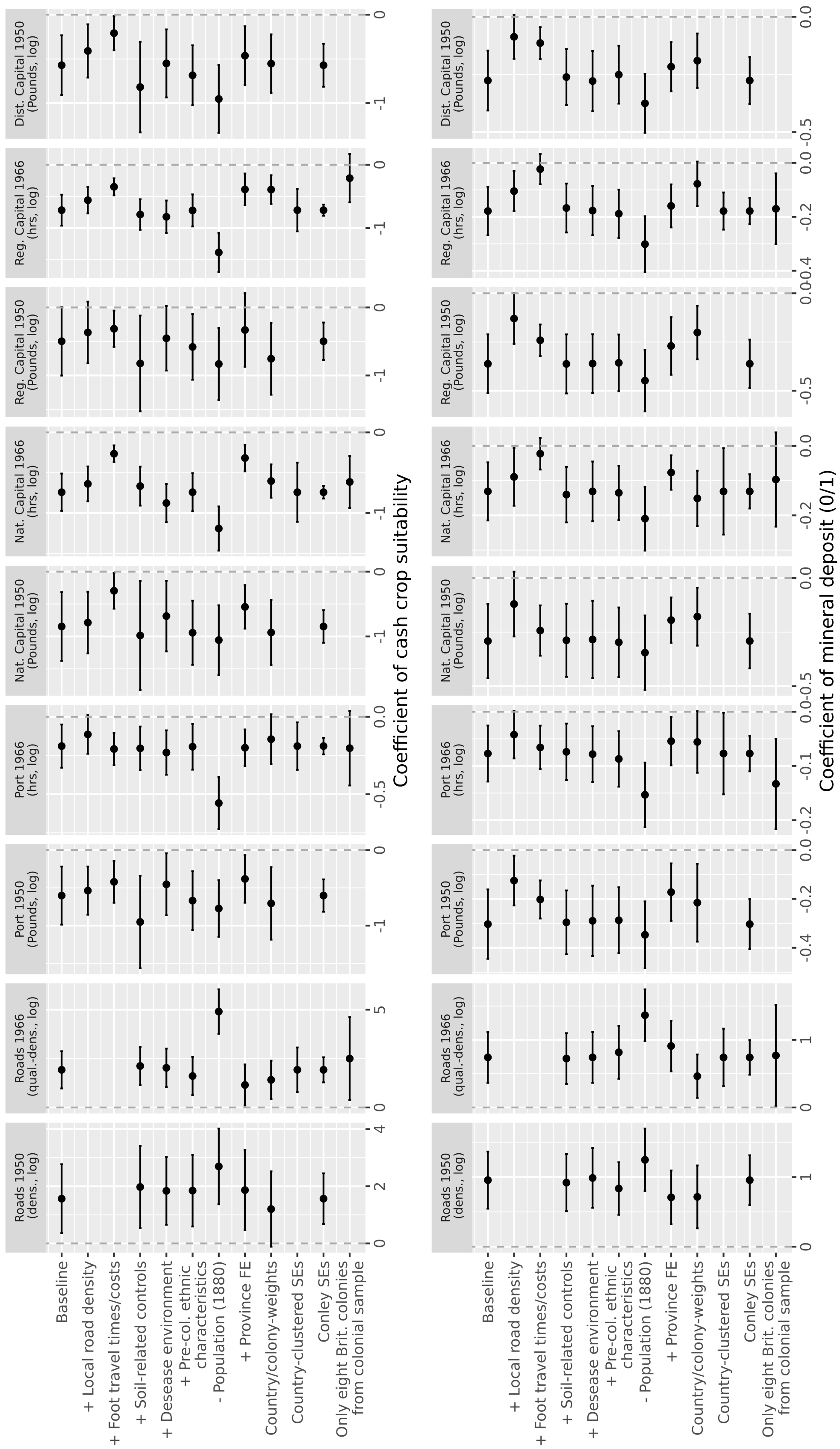


Figure E.5: Summary of the main additional models.

E.3.1 *Varying unit sizes*

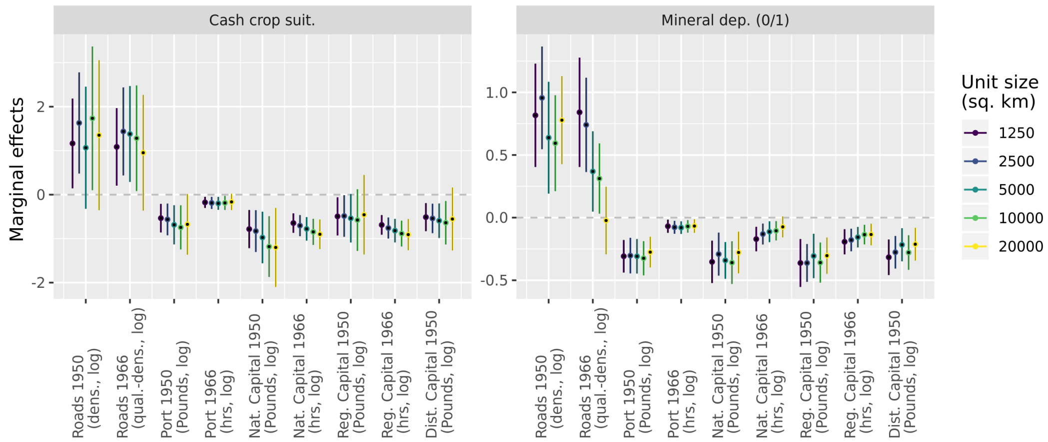


Figure E.6: Robustness check: Varying the size of units between 1250 and 10000 km^2. Estimates based on the baseline specification from Equation 5.1, using Voronoi cells of varying sizes as units of analysis.

The choice of a size 2500 km^2 (equivalent to a quadratic region of 50×50 km) for the Voronoi cells might appear arbitrary to some.[3] In order to gauge the impact of this choice on the results, I re-estimate the baseline specifications using Voronoi cells of varying size, starting with small cells of 1250 km^2 and increasing their size in exponential steps up to 20'000 km^2. This is equivalent to cutting space into grid cells of 140×140 km. The resulting coefficients for all outcomes are plotted in Figure E.6. In general, the point-estimates are quite robust, for the most part remaining stable in size, but associated with larger standard errors as the number of units decreases. Three observations can be made. First, the increase in the size of coefficients is limited to the effect of cash crop suitability on some of the outcomes. In contrast, the estimate of the effects of mineral deposit dummy decreases for some outcomes (in particular, local road density and the distances to administrative capitals in 1960). This suggests that mineral deposits have a locally concentrated effect, which coincides with their point-like nature. Their effects then delude over relatively short distances, thus making large Voronoi cells incapable of capturing them.

3 Its is based on the resolution of the PRIO grid (Tollefsen, Strand and Buhaug, 2012), which covers the earth with regular grid cells of that size at the equator.

E.3.2 *Varying the indicator of cash crop suitability*

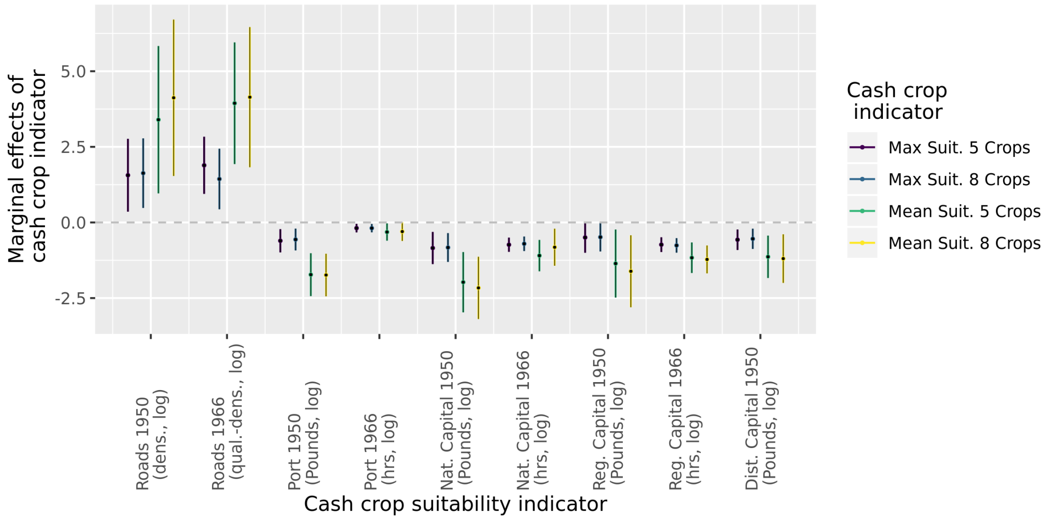


Figure E.7: Robustness check: Varying the indicator of local cash crop suitability. The estimated marginal effects are based on the baseline specifications, with the exception of replacing the indicator of cash crop suitability with the one indicated in the legend.

Another potentially arbitrary decision relates to the cash crop indicators and the functional form of aggregation used to derive the local potential for cash crop agriculture. Although the eight crops used in the main specification – coffee, cocoa, cotton, groundnuts, oil palms, sugarcane, tea, and tobacco – together made up the bulk of agricultural exports from Africa (Hance, Kotschar and Peterec, 1961), sugarcane, tea, and tobacco made up only a relatively small portion of agricultural exports. Furthermore, taking the max value across all crop-suitabilities may lead to different results than taking the mean. The latter form of aggregation might be relevant, if areas in which more than one cash crop could be planted were of more value than areas where only one crop could be produced. Figure E.7 shows that using eight or five cash crops and taking the mean or max as method of aggregation at the grid-cell level[4] does not change the results. The fact that the estimates of the suitability scores produced by averaging across crops is consistently about twice the size of the baseline estimate is due to the 50 percent lower mean of the respective variables (see the summary statistics in Tables E.1 and E.2),

4 The reader may be reminded, that I first construct the cash crop suitability on the level of the original FAO raster, and then aggregate it up to the level of Voronoi cells used as units of observation. That latter aggregation is always done by taking the mean suitability score in an area.

E.3.3 *Simple foot-transport costs and times*

Mentioned in the main empirical discussion in Chapter 5, Table E.4 shows that indeed, the locations of administrative capitals and borders in Africa respond to local natural resource potentials. The table shows the results of the baseline models of the costs and times of transport to ports and capitals. However, to single out mere geographic distances, the costs and times are computed based on pure 'foot-travel' networks, in which all edges are regarded as being foot-paths only. The results show that ports are located in a neutral manner. This may well be a result of the very constrained number of suitable indentations on the African shoreline (Austen, 1983), and can act as a 'placebo-foil' against which we can judge the biased placement of capitals. These are quite consistently located closer to cash-crop suitable areas and mineral deposits. Although the size and precision varies across outcomes, this finding highlights that state-builders may not only build roads for extractive purposes, but may also (and maybe even preferably) locate their administrative headquarters close to where they can extract most revenue.

Table E.4: Resources and *foot*-transport costs/times to ports and capitals 1950 & 1966

	Port 1950 (£, log)	Port 1966 (hrs, log)	Nat. Cap. 1950 (£, log)	Nat. Cap. 1966 (hrs, log)	Reg. Cap. 1950 (£, log)	Reg. Cap. 1966 (hrs, log)	Dist. Cap. 1950 (£, log)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cash crop suit.	−0.154	−0.026	−0.648***	−0.637***	−0.254	−0.572***	−0.387**
	(0.134)	(0.070)	(0.220)	(0.139)	(0.257)	(0.129)	(0.186)
Mineral dep. (0/1)	−0.103	−0.017	−0.055	−0.139**	−0.135*	−0.208***	−0.176**
	(0.064)	(0.027)	(0.080)	(0.059)	(0.078)	(0.052)	(0.079)
Country FE:	yes	yes	yes	yes	yes	yes	yes
Controls:	yes	yes	yes	yes	yes	yes	yes
Mean DV:	3.7	4.6	3.7	4.6	2.7	3.5	2.1
Observations	1,501	11,626	1,501	10,436	1,501	10,757	1,501
Adjusted R^2	0.818	0.877	0.617	0.592	0.369	0.556	0.468

Notes: OLS linear models. Standard errors are clustered on the province-level. Controls consist of the estimated population in 1880, units' area, their distance to the coast, border, and the closest navigable river, their median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Significance codes: *p<0.1; **p<0.05; ***p<0.01

E.3.4 *Dropping the Gold Coast from the colonial sample*

As briefly mentioned in the discussion on the road network data retrieved from the British colonial Blue Books, the lists from the Gold Coast do not consistently cover the colony's Northeast, where they show no roads under the administration of the central government. In order to gauge whether this regional bias has an effect on the results, I rerun the baseline specification for the colonial outcomes measured in 1950 and drop the Gold Coast from the sample. This does not change the results in a significant manner.

Table E.5: Robustness check: Dropping the Gold Coast

	Road 1950 (dens., log)	Port 1950 (£, log)	Nat. Cap. 1950 (£, log)	Reg. Cap. 1950 (£, log)	Dist. Cap. 1950 (£, log)
	(1)	(2)	(3)	(4)	(5)
Cash crop suit.	1.682***	−0.465**	−0.848***	−0.514**	−0.537***
	(0.620)	(0.182)	(0.252)	(0.245)	(0.171)
Mineral dep. (0/1)	0.930***	−0.242***	−0.291***	−0.353***	−0.275***
	(0.240)	(0.067)	(0.097)	(0.086)	(0.072)
Country FE:	yes	yes	yes	yes	yes
Controls:	no	no	no	no	no
Mean DV:	-0.54	2.9	2.9	2.3	1.8
Observations	1,409	1,409	1,409	1,409	1,409
Adjusted R^2	0.496	0.598	0.472	0.426	0.509

Notes: OLS linear models. Standard errors are clustered on the province-level. Controls consist of the estimated population in 1880, units' area, their distance to the coast, border, and the closest navigable river, their median altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Significance codes: *p<0.1; **p<0.05; ***p<0.01

F

APPENDIX: INDIRECT RULE AND DEVELOPMENT

This appendix presents the data, the main results in table form, as well as additional results to Chapter 6. Section F.1 presents summary statistics and balance test, Section F.2 presents the results from the main specification and the more controlled specifications that exploit differences between neighboring ethnic groups and variation within groups split by French-British boundaries. Thereafter, Section F.3 presents the main robustness checks, Section F.4 the analysis of potential mechanisms, and Section F.5 presents the results of the analysis of contemporary development outcomes.

F.1 SUMMARY STATISTICS AND BALANCE TESTS

Table F.1: Summary statistics of individuals in the colonial sample

Statistic	N	Mean	St. Dev.	Min	Max
Primary education	361700	34.99	47.69	0	100
Secondary education	361700	9.21	28.92	0	100
Tertiary education	361700	2.41	15.32	0	100
Precol. centr. (PCC)	380769	1.69	0.97	0	3
Cash crop suit. (CCS)	402750	0.38	0.18	0.00	1.00
Distance to coast	402814	402.17	324.24	0.003	1768.57
Distance to navigable river	401906	158.18	146.53	0.06	1018.72
Distance to border	402814	72.41	71.69	0.01	570.38
Altitude	402785	579.46	576.99	−2	3589
Slope	402785	4.04	1.58	1	9
Mean temperature	402785	24.53	4.16	5.85	30.23
Evapotranspiration	402785	1650.98	274.00	1004	2678
Precipitation	402785	1164.85	581.39	1	3291
Evapotransp./Precip.	402785	4.34	1.45	1	8
Agricultural suitability	390927	0.39	0.23	0.00	0.99
Population density 1880 (log)	401906	63.54	208.51	0.00	2365.89
Dependence on agriculture	392808	2.02	1.23	0	9
Dependence on husbandry	392808	5.96	1.27	0	9
Intensity of agriculture	381036	2.27	0.57	0	4
Cash crop suit. (max; 5 crops)	402750	0.34	0.18	0.00	1.00
Cash crop suit. (mean; 5 crops)	402750	0.17	0.11	0.00	0.75
Cash crop suit. (mean; 8 crops)	402750	0.16	0.10	0.00	0.67

Table F.2: Balance test of pre-treatment covariates and precolonial centralization × cash crop suitability

	Baseline			Ethnic pairs			X-Border
Dep. variable	British	French	Diff.	British	French	Diff.	Diff.
Distance to coast	−0.58	0.02	−0.56	0.19	0.01	0.16	−0.05
	(0.39)	(0.23)	(0.44)	(0.14)	(0.11)	(0.17)	(0.26)
Distance to navigable river	0.35	0.39	−0.10	−0.03	0.07	−0.10	0.52
	(0.24)	(0.37)	(0.46)	(0.09)	(0.28)	(0.30)	(0.42)
Distance to border	−0.28	−0.48*	0.21	0.10	−0.03	0.14	0.82
	(0.31)	(0.27)	(0.41)	(0.19)	(0.15)	(0.24)	(0.53)
Altitude	−0.36	0.27	−0.53	−0.08	0.32*	−0.24	0.57
	(0.29)	(0.37)	(0.36)	(0.28)	(0.18)	(0.33)	(0.46)
Slope	−0.44***	0.37	−0.76***	−0.10	−0.23	0.08	0.50
	(0.15)	(0.23)	(0.24)	(0.21)	(0.21)	(0.28)	(0.55)
Mean temperature	0.25	−0.09	0.31	0.10	−0.21*	0.22	−0.35
	(0.28)	(0.25)	(0.32)	(0.29)	(0.12)	(0.30)	(0.38)
Evapotranspiration	0.18	0.01	0.14	0.07	−0.04	0.09	0.01
	(0.35)	(0.31)	(0.45)	(0.30)	(0.09)	(0.26)	(0.11)
Precipitation	−0.11	0.27	−0.38	−0.05	−0.08	0.03	−0.31*
	(0.16)	(0.22)	(0.27)	(0.05)	(0.09)	(0.10)	(0.17)
Evapotransp./Precip.	−0.26	0.08	−0.31	−0.09	−0.15*	0.09	−0.24
	(0.23)	(0.23)	(0.33)	(0.12)	(0.08)	(0.13)	(0.18)
Agricultural suitability	0.00	0.99***	−0.92***	−0.22	0.09	−0.29	−0.09
	(0.23)	(0.24)	(0.31)	(0.21)	(0.15)	(0.25)	(0.59)
Population density 1880 (log)	0.81***	0.04	0.77**	0.34**	−0.47	0.81**	0.90*
	(0.22)	(0.26)	(0.34)	(0.15)	(0.35)	(0.38)	(0.51)
Dependence on agriculture	−0.33	−0.27	−0.14	−0.46**	0.26	−0.73**	0.00
	(0.21)	(0.23)	(0.30)	(0.19)	(0.35)	(0.34)	(0.00)
Dependence on husbandry	−0.21	0.90***	−1.00**	−0.10	0.12	−0.21	0.00
	(0.33)	(0.32)	(0.45)	(0.35)	(0.32)	(0.47)	(0.00)
Intensity of agriculture	−0.02	0.29	−0.33	−0.22	0.26**	−0.48	0.00
	(0.37)	(0.27)	(0.45)	(0.44)	(0.13)	(0.45)	(0.00)
Colony×Birthyear FE	yes	yes	yes	yes	yes	yes	yes
Survey FE	yes	yes	yes	yes	yes	yes	yes
Ethnic pair FE	no	no	no	yes	yes	yes	no
Ethnic group FE	no	no	no	no	no	no	yes
Cutoff (km)	–	–	–	100	100	100	–
Obs	192650	150072	342722	132407	101611	234018	40162

Notes: Balance tests for the French and British samples result from split sample regressions, whereas the estimate of the differential effect of precolonial centralization × cash crop suitability is estimated from the pooled sample. Standard errors are clustered on the country-specific ethnic group. Significance codes: *p<0.1; **p<0.05; ***p<0.01.

F.2 MAIN RESULTS

Table F.3: Indirect rule, cash crops, and colonial education: British vs. French rule

	Primary Education (0/100)		
	(1)	(2)	(3)
Precol. centr. (PCC)	5.908***	72.634**	59.737**
	(2.179)	(35.677)	(26.392)
Cash crop suit. (CCS)	19.932	10.596**	47.375*
	(13.008)	(4.692)	(25.968)
PCC × British	−13.486***	−30.956	1.486
	(3.578)	(42.837)	(33.910)
CCS × British	−29.001	−21.695***	−87.968**
	(17.794)	(7.653)	(34.896)
PCC × CCS	−11.404*	−8.681***	−1.481
	(6.309)	(2.736)	(3.127)
PCC × CCS × British	23.648***	21.816***	8.491**
	(8.268)	(4.026)	(3.921)
Colony×Birthyear FE	yes	yes	yes
Survey FE	yes	yes	yes
Ind. controls:	yes	yes	yes
Geo. controls:	no	yes	yes
Ethn. controls:	no	no	yes
Mean DV:	35	35	35
Observations	342,722	332,411	332,411
Adjusted R^2	0.306	0.362	0.377

Notes: OLS linear probability models. Standard errors are clustered on ethnic groups within colonies. Individual controls consist of respondent's sex and age. Geographical controls consist of their distance to the coast, border, and the closest navigable river (all logged), the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and ethnic groups' dependence on agriculture and husbandry as well as the intensity of agriculture. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

Table F.4: Indirect rule, cash crops, and colonial education: Compairing neighbouring ethnic groups (100km cutoff)

	Primary Education (0/100)					
	British colonies			French colonies		
	(1)	(2)	(3)	(4)	(5)	(6)
Precol. centr. (PCC)	−3.551*	43.402***	35.348**	2.885**	8.915	2.605
	(2.061)	(13.534)	(15.584)	(1.215)	(23.583)	(21.877)
Cash crop suit. (CCS)	−10.530	−5.965	−16.758	2.020	8.347	−27.872
	(7.524)	(6.184)	(18.184)	(5.173)	(5.271)	(17.595)
PCC × CCS	8.324**	7.950***	3.979*	−6.805**	−6.711**	−6.112***
	(4.125)	(2.763)	(2.334)	(3.068)	(3.134)	(2.350)
Ethnic pair × Birthyear FE:	yes	yes	yes	yes	yes	yes
Colony×Birthyear FE	yes	yes	yes	yes	yes	yes
Survey FE	yes	yes	yes	yes	yes	yes
Ind. controls:	yes	yes	yes	yes	yes	yes
Geo. controls:	no	yes	yes	no	yes	yes
Ethn. controls:	no	no	yes	no	no	yes
Mean DV:	44	45	45	17	16	16
Observations	132,407	128,640	128,640	101,611	99,589	99,589
Adjusted R^2	0.271	0.283	0.293	0.332	0.338	0.348

Notes: OLS linear probability models. Standard errors are clustered on ethnic groups within colonies. Individual controls consist of respondent's sex and age. Geographical controls consist of their distance to the coast, border, and the closest navigable river (all logged), the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and ethnic groups' dependence on agriculture and husbandry as well as the intensity of agriculture. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

Table F.5: Indirect rule, cash crops, and colonial education: Across borders, within ethnic groups

	Primary Education (0/100)		
	(1)	(2)	(3)
Cash crop suit. (CCS)	−0.962	14.119**	−30.395
	(9.843)	(6.399)	(55.377)
PCC × British	−3.966	203.890	403.548***
	(2.766)	(137.552)	(144.550)
CCS × British	1.076	−19.618	−84.720
	(14.168)	(14.416)	(71.487)
PCC × CCS	−6.864	−14.189***	−9.183**
	(5.172)	(4.993)	(4.407)
PCC × CCS × British	9.694	18.137**	11.738*
	(6.597)	(7.206)	(6.718)
Ethnic group × Birthyear FE:	yes	yes	yes
Colony×Birthyear FE	yes	yes	yes
Survey FE	yes	yes	yes
Ind. controls:	yes	yes	yes
Geo. controls:	no	yes	yes
Ethn. controls:	no	no	yes
Mean DV:	17	17	17
Observations	40,162	39,025	39,025
Adjusted R^2	0.277	0.295	0.306

Notes: OLS linear probability models. Standard errors are clustered on ethnic groups within colonies. Individual controls consist of respondent's sex and age. Geographical controls consist of their distance to the coast, border, and the closest navigable river (all logged), the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and ethnic groups' dependence on agriculture and husbandry as well as the intensity of agriculture. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

F.3 ROBUSTNESS CHECKS

F.3.1 *Alternative education-related outcomes*

In order to address concerns that the results might be unstable across different education-related outcomes, the baseline results are replicated for dummies of completed secondary and tertiary education. The results, presented in Table F.6, exhibit the same pattern as those reported at the baseline. Point estimates for the British sample are of similar size than those for primary education rates and significant throughout. For the French, the interaction terms of interest are again negative and significant. The results thus indicate that cash crop suitability generally increased higher education provision under British indirect rule, but decreased it under French direct rule.

Table F.6: Indirect rule, cash crops, and alternative education outcomes

	Primary Education (0/100)			
	British colonies		French colonies	
	Sec. Educ.	Tert. Educ.	Sec. Educ.	Tert. Educ.
	(1)	(2)	(3)	(4)
Precol. centr. (PCC)	27.364**	5.270	29.902**	6.471
	(11.591)	(3.876)	(12.182)	(3.963)
Cash crop suit. (CCS)	−20.232	−0.276	79.247***	25.177***
	(12.401)	(5.641)	(28.001)	(9.392)
PCC × CCS	6.866***	3.001***	−4.366**	−1.622**
	(1.562)	(0.877)	(2.185)	(0.716)
Colony×Birthyear FE	yes	yes	yes	yes
Survey FE	yes	yes	yes	yes
Ind. controls:	yes	yes	yes	yes
Geo. controls:	yes	yes	yes	yes
Ethn. controls:	yes	yes	yes	yes
Mean DV:	12	3.3	6.1	1.3
Observations	184,872	184,872	147,539	147,539
Adjusted R^2	0.157	0.050	0.154	0.051

Notes: OLS linear probability models. Standard errors are clustered on ethnic groups within colonies. Individual controls consist of respondent's sex and age. Geographical controls consist of their distance to the coast, border, and the closest navigable river (all logged), the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and ethnic groups' dependence on agriculture and husbandry as well as the intensity of agriculture. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

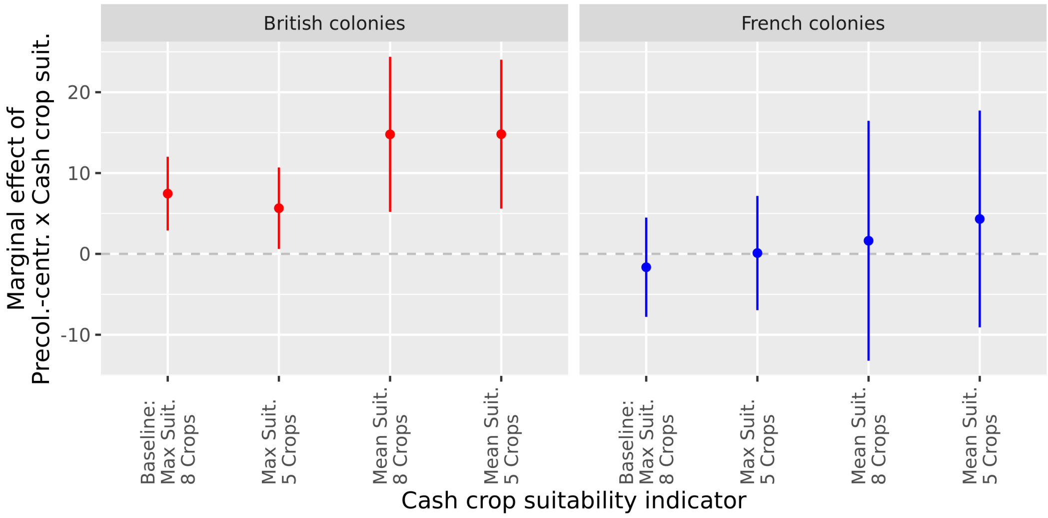


Figure F.1: The marginal effect of precolonial centralization × cash crop suitability, using alternative cash crop suitability indicators.
Based on the main specification in Equation 6.1 in the main text, replacing the main cash cash crop suitability measure with the one indicated on the x-axis.

F.3.2 *Alternative measures of cash crop suitability*

I here test the extent to which measuring the cash crop suitability on the basis of the local maximum suitability of eight crops[1] affects the results. First, I build an alternative measure by taking the local maximum of the five most important cash crops, coffee, cocoa, cotton, groundnuts, and palm oil (Hance, Kotschar and Peterec, 1961). Second, I construct the measure by taking the local average of the eight and five crops rather than their maximum value. The results, visualized in Figure F.1, remain substantially unchanged. The larger coefficients of the interaction terms of the mean suitability scores are due to the lower sample mean of the respective variables as compared those constructed by taking the local maximum (see summary statistics, Table F.1). Throughout, I do not find an interactive effect of precolonial centralization and cash crop suitability in the French colonies.

F.3.3 *Alternative measures of precolonial centralization*

Beyond the choice of the cash crop suitability indicator, the results might be driven by the particular measure of precolonial centralization. This is based on Murdock's (1967)

1 Coffee, cocoa, cotton, groundnuts, palm oil, sugar cane, tea, and tobacco.

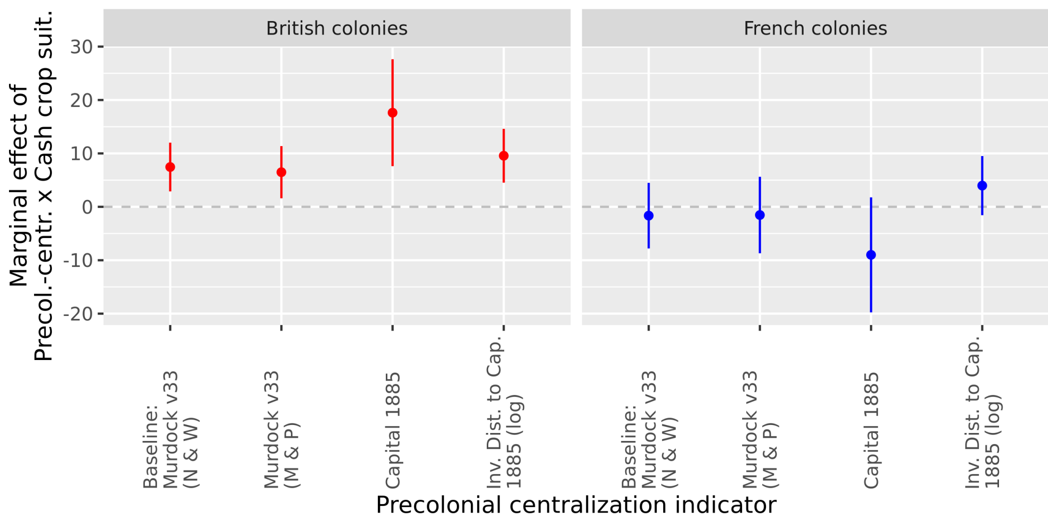


Figure F.2: The marginal effect of precolonial centralization × cash crop suitability, using varying measures of precolonial centralization.
Based on the main specification in Equation 6.1 in the main text, replacing the main variable precolonial centralization with the measures indicated on the x-axis.

coding of political complexity in his *Ethnographic Atlas* which was linked to Murdock's map of ethnic groups (1959) by Nunn and Wantchekon (2011). An alternative matching with few but potentially important differences has been implemented by Michalopoulos and Papaioannou (2013*b*). I use their coding as the first alternative source. In addition, we might fear that Murdock's coding was biased by colonial policies. To construct alternative and independent data on precolonial centralization of ethnic groups, I first map the capitals of precolonial polities observed in 1885 (see Chapter 4) to ethnic groups from Murdock's (1959) map, creating a dummy in each group that was home to a capital. Second, I calculate the distance of DHS clusters to the closest capital in 1885 and use the logged inverse distance as a rough indicator of the strength of precolonial institutions.

Re-estimating the baseline model for the British and the French samples with these indicators for precolonial centralization strengthens the confidence in the results. Figure F.2 shows that all three alternative meaures are consistently associated with increases in the marginal effect of local cash crop suitability. This is not the case in the French sample. Here, the alternative Murdock coding produces results equivalent to the baseline, ethnic groups with a capital in 1885 feature a lower marginal effect of cash crop suitability. Lastly, French areas close to historical capitals feature slightly higher but imprecisely estimated marginal effects of cash crop suitability.

F.3.4 *Selection through migration:*

I here test whether the effects from the baseline analysis are driven by biased migration patterns. In particular, migration decisions of well- or non-educated people might have differed systematically between ethnic groups and colonial empires so that individuals sampled by the DHS have self-selected themselves into or out of treatment over their lifetime. In order to test whether this is a caveat to the analysis, I draw on the DHS's Individual and Male's Recodes. This data stems from the smaller sample of respondents to the DHS which have gone through the entire interview.[2] I therefore loose 90% of all observations but gain information on whether an individual has always lived in the same location or has moved at some point of her life. I transform this data into a simple migrant-dummy and then interact it with the main interaction term of interest. Models 1–3 in Table F.7 show that the interactive effect of PCC $\times$ CCS is is no different among migrants and non-migrants in the British sample. In the French sample (Models 4–6), migrants are associated with a much stronger negative interaction effect.[3] However, even among non-migrants, the interaction effect is negative in the lesser specified Models 4 and 5 and turns positive but statistically insignificant in the fully specified Model 6.

2 For the baseline results based on this smaller sample of DHS respondents see the respective robustness check in Figure F.3 in Subsection F.3.5 below.

3 A number of education based selection patterns could drive this results, which one exactly we cannot infer from the data lacking information on the places of origin of the migrants.

Table F.7: Indirect rule, cash crops, and colonial education: Controlling for migrants

	Primary Education (0/100)					
	British colonies			French colonies		
	(1)	(2)	(3)	(4)	(5)	(6)
Precol. centr. (PCC)	−9.086**	88.371***	127.108***	3.635	47.176	13.181
	(4.007)	(31.297)	(27.776)	(2.472)	(40.475)	(38.948)
Cash crop suit. (CCS)	−11.997	−11.385	−60.626**	13.333	1.554	18.029
	(17.859)	(12.636)	(25.118)	(15.514)	(10.354)	(39.414)
PCC × CCS	17.191**	16.528***	12.904**	−5.523	−3.030	1.224
	(7.095)	(5.540)	(5.381)	(6.659)	(5.104)	(4.730)
Migrant	27.903***	15.070*	12.117	−10.040**	−7.630**	−5.811*
	(8.560)	(9.059)	(8.828)	(4.712)	(3.663)	(3.206)
Migrant × PCC	−3.323	−0.129	0.847	6.532***	5.590***	4.095***
	(3.184)	(3.646)	(3.486)	(2.378)	(1.772)	(1.505)
Migrant × CCS	−35.634**	−16.875	−11.368	27.317*	17.118	9.881
	(16.247)	(16.514)	(16.477)	(14.364)	(11.667)	(10.104)
Migrant × PCC × CCS	8.395	1.749	−0.813	−14.986**	−11.952**	−7.659
	(6.751)	(7.407)	(7.254)	(7.073)	(5.652)	(4.710)
Colony×Birthyear FE	yes	yes	yes	yes	yes	yes
Survey FE	yes	yes	yes	yes	yes	yes
Ind. controls:	yes	yes	yes	yes	yes	yes
Geo. controls:	no	yes	yes	no	yes	yes
Ethn. controls:	no	no	yes	no	no	yes
Mean DV:	59	59	59	18	18	18
Observations	12,702	12,389	12,389	15,413	15,305	15,305
Adjusted R^2	0.197	0.267	0.282	0.185	0.245	0.267

Notes: OLS linear probability models. Standard errors are clustered on ethnic groups within colonies. Individual controls consist of respondent's sex and age. Geographical controls consist of their distance to the coast, border, and the closest navigable river (all logged), the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and ethnic groups' dependence on agriculture and husbandry as well as the intensity of agriculture. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

F.3.5 *Additional specifications*

This section describes a number of further model specifications mentioned briefly in the main empirical analysis of Chapter 6. To ease the interpretation of the results and reduce complexity, all findings from these robustness checks are summarized in Figure F.3.

FIXED EFFECTS SPECIFICATIONS: In order gauge the robustness of the results, I rerun the analysis varying the main fixed effects employed. In comparison to the main colony×birth-year and survey fixed effects, I first introduce sole colony and survey

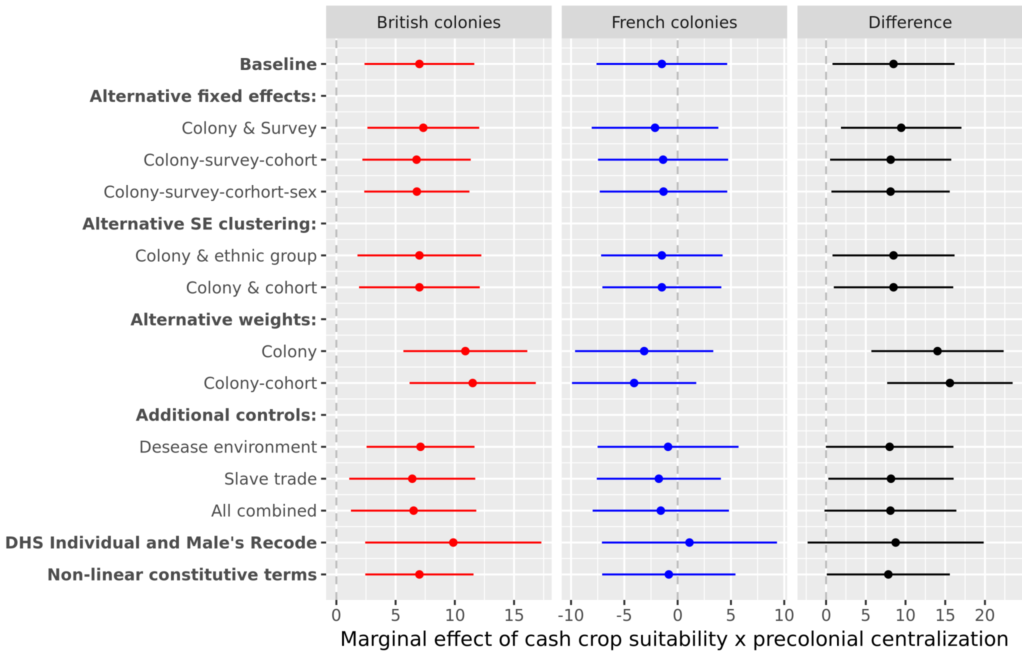


Figure F.3: Marginal effect of precolonial centralization × cash crop suitability in a variety of additional specifications.
Based on the main specification in Equation 6.1 in the main text, implementing the changes indicated on the y-axis and discussed in detail in Section F.3.5 of this Appendix.

fixed effects. I then increase the amount of variation soaked up by estimating the main specification with colony×survey×birth-year and lastly colony×survey×birth-year×sex fixed effects. As shown in Figure F.3, these variations lead to substantively unchanged results.

ALTERNATIVE STANDARD ERROR SPECIFICATIONS: To gauge the influence of the main model's custering of standard errors on the ethnic group by colony level, Figure F.3 presents results from the fully specifed baseline models, clustering standard errors first separately on the colony and ethnic group level[4] and second on the colony-cohort level. The first alternative leads to lightly larger standard errors, the second to much smaller ones. Both versions do not affect the statistical significance of the results.

WEIGHTING BY COLONY: The DHS has not been fielded symmetrically in the countries studied here. Instead, more developed countries and those with better governance have been surveyed more often and more extensively. In a similar vein,

4 This is possible because a number of ethnic groups are present in several colonies, which might affect standard errors if errors are systematically correlated within colonies but also within ethnic groups

cohorts born early in the 20^{th} century are underrepresented in the surveys as many individuals have died over the years. In order to gauge whether the results are driven by 'oversampled' countries and cohorts, Figure F.3 presents results from running the fully specified baseline specification and weighting respondents according to the inverse of the number of respondents living in their respective colony and attributed to their respective colony-cohort. Doing so leads to slightly larger estimates of the main interaction term PCC × CCS of interest, indicating the the oversampling of countries and cohorts leads me, if at all, to underestimate the impact of indirect rule on the marginal effect of cash crop suitability on primary education.

ADDITIONAL CONTROLS: As in the previous Chapters, I also control for the robustness of the results after adding vectors of control that capture important causes of long-term development in Africa. The first is the local disease environment, that had important effects on European's health and thus colonial state building. Second, I control for the extent of the precolonial slave trades, which might have affected the development of precolonial institutions. Figure F.3 shows that the baseline results are robust to these additions.

DHS INDIVIDUAL AND MALE'S RECODE DATA: The baseline results rely on the full set of individuals born at least six years before independence in the Personal Recode Data of the DHS (2018). About 90%[5] of these individuals have not been interviewed in person but rather reported on by members of their household that went through the entire DHS interview. In order to gauge whether drawing on this 'second-hand' information inserts bias into the analysis, Figure F.3 reports the results of estimating the baseline specification on the sample of respondents that have gone through the full interview and are therefore included in the DHS Individual and Men's Recodes. The results show that, in the British sample, the interaction effect between cash crop suitability and precolonial centralization is estimated to be larger, in particular in the models without control variables. This deviation is likely due to the fact that women in the reduced sample are born later since the DHS only interviews women up to age 49, thus limiting the extent to which we can travel back into the colonial period. The effects observed in the French sample are very similar to those estimated on the full sample.

5 This figure varies across households, countries, and surveys.

NON-LINEAR EFFECTS OF THE CONSTITUTIVE TERMS: A last robustness check addresses the caveat that the main interaction of interest, $\mathsf{PCC} \times \mathsf{CCS}$, might be driven by non-linear effects of its constitutive terms. Adding the quadratic terms PCC^2 and PCC^2 to the baseline model, Figure F.3 shows that this worry in unwarranted.

F.4 MECHANISMS

F.4.1 *Mixed evidence from Native Treasuries*

In order to test whether areas with suitable soils for cash crop production led to more public service provision under indirect than direct rule, I here briefly return to the data on native administration's budgets introduced in Chapter 4. From this data, I take the total revenues, expenditures, as well as the specified expenditure on education and social services as the main outcomes. These measures capture not only bigger budgets, but also more expenditures for education. The main shortcoming of the data is, that it is available only for 140 native administrations in the four colonies: the Gold Coast, Nigeria, Nyasaland, and Uganda. Many times, the native administrations ruled over large areas, in particular where indirect rule was applied (see Chapter 4 for the respective results). The variance in the size of districts and their large size makes the data much less suitable to detect *local* effects of soil's suitability for and ensuing production of cash crops.

Estimating the main specifications with the three main outcomes leads to results that confirm the main argument at first sight. As shown in Table F.8, cash crop suitability has an effect on the size of revenues and expenditures that is strongly increasing in the level of local precolonial centralization. However, upon further inspection of the data and removing 2.5 percent of the observations at each extreme of the distributions of the outcomes, the results presented in Table F.9 show that this result is mostly driven by outliers, in particular a few areas with very small budgets in an acephalous and highly suitable region of Southeastern Nigeria. While historical evidence from that region bears witness to unresponsive and corrupt local rule by warrant chiefs (e.g. Afigbo, 1972), this result leaves us unable to draw a firm conclusion on the viability of the purposed mechanism through local native administrations.

Table F.8: Indirect rule, cash crops, and Native Treasuries' revenues and expenditures

	Revenue (2016£)			Expenditure (2016£)	Educ. & Social (2016£)
	(1)	(2)	(3)	(4)	(5)
Precol. centr. (PCC)	−0.535	5.257	9.001	10.163	14.477
	(0.418)	(5.264)	(5.601)	(9.251)	(19.715)
Cash crop suit. (CCS)	−2.834	−2.813	−13.608**	−19.031***	−25.849**
	(2.214)	(2.045)	(5.537)	(6.590)	(11.935)
PCC × CCS	1.825	1.921*	2.407**	2.179**	5.581**
	(1.122)	(1.082)	(1.074)	(0.918)	(2.345)
Colony FEs:	yes	yes	yes	yes	yes
District controls:	yes	yes	yes	yes	yes
Geo. controls:	no	yes	yes	yes	yes
Ethn. controls:	no	no	yes	yes	yes
Mean DV:	13	13	13	14	11
Observations	146	146	146	126	126
Adjusted R^2	0.766	0.782	0.783	0.733	0.642

Notes: OLS linear models. Standard errors are clustered on the province level. District controls consist of their area, and their poplation in 1880 (both logged). Geographical controls consist of districts' distance to the coast, border, and the closest navigable river (all logged), the average altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and the average dependence of districts' ethnic groups on argiculture and husbandry as well as their intensity of agricultural activities. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

Table F.9: Indirect rule, cash crops, and Native Treasuries: Dropping outliers

	Revenue (2016£)			Expenditure (2016£)	Educ. & Social (2016£)
	(1)	(2)	(3)	(4)	(5)
Precol. centr. (PCC)	0.067	3.416	6.896	4.802	−6.569
	(0.318)	(4.362)	(4.595)	(6.859)	(9.910)
Cash crop suit. (CCS)	0.381	0.744	−8.710	−15.149**	−8.900
	(1.361)	(1.115)	(6.106)	(6.608)	(9.112)
PCC × CCS	0.172	−0.158	0.494	1.103	1.023
	(0.739)	(0.598)	(0.804)	(0.787)	(1.024)
Colony FEs:	yes	yes	yes	yes	yes
District controls:	yes	yes	yes	yes	yes
Geo. controls:	no	yes	yes	yes	yes
Ethn. controls:	no	no	yes	yes	yes
Mean DV:	13	13	13	14	11
Observations	138	138	138	118	118
Adjusted R^2	0.746	0.769	0.769	0.707	0.760

Notes: OLS linear models. Standard errors are clustered on the province level. District controls consist of their area, and their poplation in 1880 (both logged). Geographical controls consist of districts' distance to the coast, border, and the closest navigable river (all logged), the average altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and the average dependence of districts' ethnic groups on argiculture and husbandry as well as their intensity of agricultural activities. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

F.4.2 *Public infrastructure and missionary education*

With this, I am left to probe the viability of the hypothesized mechanism ex negativo, that is to test whether the results are driven by the two most prominent alternative explanations. The first explanation focuses on the provision of local public infrastructure which was crucial for incentivizing agricultural production (see Chapter 5). It might be that British colonial governments invested more into local public infrastructure in indirectly ruled areas where they could rely on effective local governments than in directly ruled areas where they had to rely on local elites which were less able to mobilize the material and human resources (often through local traditional 'labor taxes') to build the physical infrastructure. Given that extraction from indirectly ruled areas paid off less for colonial governments, they might also have had the opposite incentive to build more infrastructure where they could rule and extract directly. Differences in the density of local transport infrastructure might have then translated into differential levels of agricultural production, affecting education rates solely through a private wealth mechanisms. I capture this mechanism by adding measures on local transport infrastructure to the main baseline model, in particular the presence and density of roads in 1966, rails in 1960, and the local distance to the next port (in hrs.). All data are presented in detail in Chapter 5. Doing so amounts to adding 'bad', that is post-treatment controls, thus biasing the estimated treatment effect of the interaction of $\mathsf{PCC} \times \mathsf{CCS}$. As the results presented in Table F.10 show, doing so does not change the main results.

The second mechanism in the influence of missionaries. Besides colonial governments, missionaries were the main providers of education, in particular in British colonies (see e.g. Gifford and Weiskel, 1971; Cogneau, 2003; Cogneau and Moradi, 2014). The choices of missionaries where to build their missionary stations was hardly random (see e.g. Jedwab, zu Selhausen and Moradi, 2018) and might have been influenced by local economic potentials as well as precolonial institutions. Their settlements might therefore constitute a so far unobserved mechanism that led suitable soils to relate more strongly to education in centralized groups under British rule than elsewhere. To account for that factor, I control for DHS clusters' logged distance to the next missionary station (from Roome, 1924; Nunn, 2010) as well as a dummy for clusters

Table F.10: Indirect rule, cash crops, and colonial education: Alternative mechanisms

	Primary Education (0/100)					
	British colonies			French colonies		
	(1)	(2)	(3)	(4)	(5)	(6)
Precol. centr. (PCC)	60.539***	52.289**	51.589**	−51.933**	45.980*	−45.445**
	(21.071)	(21.330)	(21.107)	(24.051)	(23.601)	(23.121)
Cash crop suit. (CCS)	−38.051*	−37.474*	−35.990*	87.418***	25.218	79.648***
	(21.623)	(21.855)	(20.495)	(24.608)	(26.323)	(25.516)
PCC × CCS	6.846***	5.509**	5.510**	−4.151	−1.207	−4.175
	(2.332)	(2.247)	(2.225)	(3.036)	(3.092)	(3.029)
Dist. to rail 1960 < 10km	4.082*		4.357**	2.246*		2.306*
	(2.168)		(2.104)	(1.340)		(1.338)
Dist. to rail 1960 (km; log)	0.277		0.701	−1.118***		−0.738*
	(0.665)		(0.653)	(0.405)		(0.393)
Dist. to road 1966 < 10km	2.679***		2.649***	1.270***		1.440***
	(0.635)		(0.614)	(0.472)		(0.491)
Roads 1966 (log)	0.227		0.111	−0.0004		0.023
	(0.159)		(0.148)	(0.116)		(0.111)
Dist. to port 1966 (hrs, log)	−1.677		−0.685	−4.185***		−3.252**
	(3.130)		(3.066)	(1.375)		(1.390)
Dist. to Mission < 10km		−1.418	−1.832		3.374	2.839
		(1.114)	(1.126)		(2.185)	(2.431)
Dist. to Mission (km; log)		−3.604***	−3.448***		−2.867***	−1.869***
		(0.630)	(0.653)		(0.569)	(0.598)
Colony×Birthyear FE	yes	yes	yes	yes	yes	yes
Survey FE	yes	yes	yes	yes	yes	yes
Ind. controls:	yes	yes	yes	yes	yes	yes
Geo. controls:	no	yes	yes	no	yes	yes
Ethn. controls:	no	no	yes	no	no	yes
Mean DV:	49	49	49	15	17	15
Observations	184,607	184,872	184,607	138,545	147,539	138,545
Adjusted R^2	0.290	0.291	0.292	0.292	0.339	0.294

Notes: OLS linear probability models. Standard errors are clustered on ethnic groups within colonies. Individual controls consist of respondent's sex and age. Geographical controls consist of their distance to the coast, border, and the closest navigable river (all logged), the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and ethnic groups' dependence on agriculture and husbandry as well as the intensity of agriculture. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

that are within a 10 km radius to the next mission.[6] Doing so does only slightly reduce the estimate coefficient of the main interaction term $\mathsf{PCC} \times \mathsf{CCS}$, suggesting that endogenous missionary settlements do not drive the observed patterns.

F.5 CONTEMPORARY OUTCOMES

F.5.1 *Household wealth*

Table F.11: Indirect rule, cash crops, and contemporary Household Wealth (DHS Wealth Index)

	DHS Wealth Index					
	former British colonies			former French colonies		
	(1)	(2)	(3)	(4)	(5)	(6)
Precol. centr. (PCC)	−0.371***	−0.732	−0.715	0.277***	0.502	−0.157
	(0.102)	(0.726)	(0.521)	(0.091)	(0.400)	(0.301)
Cash crop suit. (CCS)	−1.335***	−0.788***	−1.315**	−0.177	−0.081	2.414***
	(0.379)	(0.233)	(0.551)	(0.494)	(0.333)	(0.738)
PCC × CCS	0.865***	0.629***	0.344***	−0.259	−0.171	0.143
	(0.196)	(0.113)	(0.086)	(0.215)	(0.255)	(0.097)
Survey FE	yes	yes	yes	yes	yes	yes
Former colony FE	yes	yes	yes	yes	yes	yes
HH controls:	yes	yes	yes	yes	yes	yes
Geo. controls:	no	yes	yes	no	yes	yes
Ethn. controls:	no	no	yes	no	no	yes
Mean DV:	0.017	-0.011	-0.011	0.039	-0.0079	-0.0079
Observations	417,567	398,863	398,863	186,771	176,396	176,396
Adjusted R^2	0.050	0.155	0.315	0.072	0.200	0.394

Notes: OLS linear models. Standard errors are clustered on ethnic groups within former colonies. Individual controls consist of the number of HH members and children, both linear and squared. Geographical controls consist of households distance to the coast, border, and the closest navigable river (all logged), the local altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and ethnic groups dependence on argiculture and husbandry as well as the intensity of agriculture. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

6 Note that recent work by Jedwab, zu Selhausen and Moradi (2018) indicates that there where many more missions in Africa than indicated on Roome's (1924) map. However, their newly collected data on missions only covers the Gold Coast and is not yet openly available.

F.5.2 *Per capita nightlight emissions*

Table F.12: Indirect rule, cash crops, and contemporary nightlight emissions

	Nightlights per capita (1992–2013; log)					
	British colonies			French colonies		
	All	Rural	Urban	All	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Precol. centr. (PCC)	−0.546	−0.216	0.134	−0.581	0.317	−2.150**
	(0.690)	(0.752)	(0.763)	(0.599)	(0.676)	(1.005)
Cash crop suit. (CCS)	0.803	2.133**	−1.726	4.346**	3.637**	−1.469
	(1.246)	(1.064)	(1.175)	(1.980)	(1.839)	(2.682)
PCC × CCS	0.552**	0.520***	0.260	−0.123	−0.353	−0.370
	(0.253)	(0.200)	(0.323)	(0.264)	(0.330)	(0.437)
Former colony FE	yes	yes	yes	yes	yes	yes
District controls:	yes	yes	yes	yes	yes	yes
Geo. controls:	yes	yes	yes	yes	yes	yes
Ethn. controls:	yes	yes	yes	yes	yes	yes
Mean DV:	-5.4	-5.6	-4.5	-4.7	-4.8	-4.6
Observations	2,107	1,918	1,031	1,767	1,687	825
Adjusted R^2	0.671	0.723	0.614	0.822	0.820	0.664

Notes: OLS linear models. Standard errors are clustered on districts' modal ethnic group. District controls consist of their area, and their rural and urban poplation (in 2000; logged). Geographical controls consist of districts' distance to the coast, border, and the closest navigable river (all logged), the average altitude and slope, mean annual temperature, precipitation and evapotranspiration, the ratio of the two, and the local suitability for agriculture. Ethnic controls consist of the local population density in 1880 (log), and ethnic groups dependence on argiculture and husbandry as well as the intensity of agriculture. Geographical controls are interacted with the level of precolonial centralization, ethnic controls with the cash crop suitability score. Significance codes: *p<0.1; **p<0.05; ***p<0.01

G

APPENDIX: STATE REACH AND DEVELOPMENT

This appendix presents additional information and analyses that supplement Chapter 7. Section G.1 contains summary statistics and Section G.2 presents the full cross-sectional analysis that is only briefly discussed in the main text. Lastly, Section G.3 presents discussions and results of all robustness checks conducted for the panel analysis.

G.1 DATA AND SUMMARY STATISTICS

Table G.1: Summary statistics: DHS education data (Personal Recode)

Statistic	N	Mean	St. Dev.	Min	Max
Primary educ. (0/100)	1893067	70.22	45.73	0	100
Female	1893067	0.53	0.50	0	1
Age	1893067	27.92	9.66	15	57
Time to nat. capital (log)	1893067	1.92	0.80	0.00	4.30
Time to reg. capital (log)	1893067	1.17	0.65	0.00	3.92

Table G.2: Summary statistics: DHS infant mortality data (Children Recode)

Statistic	N	Mean	St. Dev.	Min	Max
Infant mort. (0/100)	2644591	9.89	29.86	0	100
Female	2644591	0.49	0.50	0	1
Birth-order	2644591	3.30	2.25	1	18
Twin	2644591	0.03	0.18	0	1
Mother's age at birth	2634666	24.64	6.42	10	49
Time to nat. capital (log)	2644591	1.94	0.71	0.00	4.26
Time to reg. capital (log)	2644591	1.20	0.62	0.00	3.91

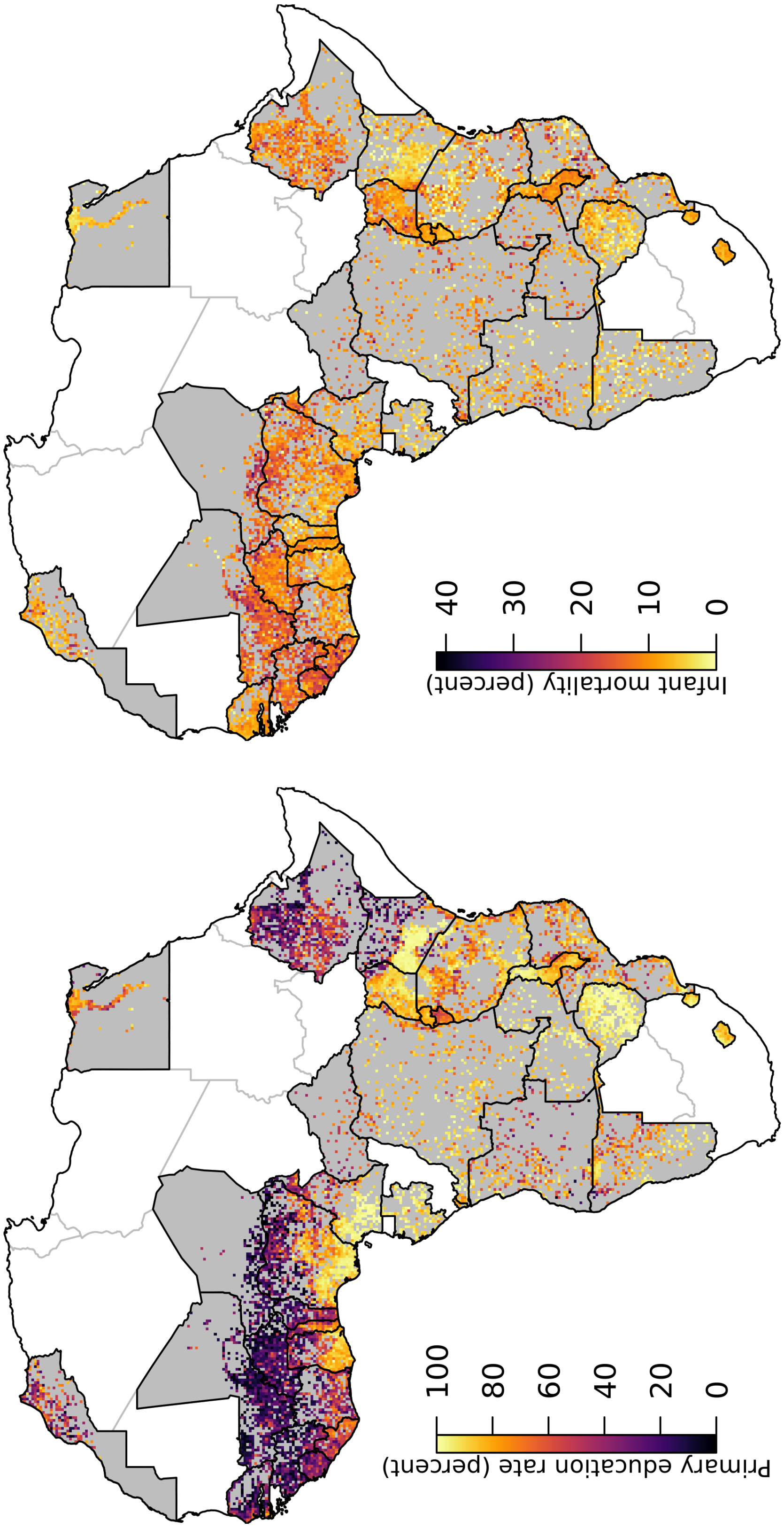
Table G.3: Summary statistics: Nightlight data (Voronoi cells, 400 km^2)

Statistic	N	Mean	St. Dev.	Min	Max
Light/capita (log)	1506991	−6.51	1.28	−6.91	10.12
Time to nat. capital (log)	1506991	2.92	0.77	0.24	4.96
Time to reg. capital (log)	1506991	2.30	0.83	0.24	4.93

Table G.4: Samples across data sources, DHS rounds and nightlight observations

Country	Adults	Children	Households	Nightlight-cells
Algeria				1992–2013
Angola	7.1	6.1, 7.1	5.1, 6.1, 7.1	1992–2013
Benin	4.1, 6.1	4.1, 6.1	6.1	1992–2013
Botswana				1992–2013
Burkina Faso	2.1, 3.1, 4.1, 6.1	2.1, 3.1, 4.1, 6.1	4.1, 6.1, 7.1	1992–2013
Burundi	6.1, 6.2, 7.1	6.1, 6.2, 7.1	6.1, 6.2, 7.1	1992–2013
Cameroon	2.1, 4.1, 6.1	2.1, 4.1, 6.1	4.1, 6.1	1992–2013
Central African Republic	3.1	3.1		1992–2013
Chad				1992–2013
Congo				1992–2013
Côte D'Ivoire	3.1, 3.2, 6.1	3.1, 3.2, 6.1	6.1	1992–2013
Djibouti				1992–2013
DR Congo	5.1, 6.1	5.1, 6.1	5.1, 6.1	1992–2013
Egypt	2.1, 3.1, 4.1, 4.2, 5.1, 5.2	2.1, 3.1, 4.1, 4.2, 5.1, 5.2	5.1, 5.2	1992–2013
Equatorial Guinea				1992–2013
Eritrea				1993–2013
Ethiopia	4.1, 5.1, 6.1, 7.1	4.1, 5.1, 6.1, 7.1	5.1, 6.1, 7.1	1992–2013
Gabon	6.1	6.1	6.1	1992–2013
Gambia				1992–2013
Ghana	3.1, 4.1, 4.2, 5.2, 7.1	3.1, 4.1, 4.2, 5.2, 7.1	4.2, 5.2, 7.1, 7.2	1992–2013
Guinea	4.1, 5.1, 6.1	4.1, 5.1, 6.1	5.1, 6.1	1992–2013
Guinea Bissau				1992–2013
Kenya	4.1, 5.1, 7.1	4.1, 5.1, 7.1	4.1, 5.1, 7.1, 7.2	1992–2013
Lesotho	4.1, 6.1, 7.1	4.1, 6.1, 7.1	4.1, 6.1, 7.1	1992–2013
Liberia	5.1, 6.2	0.1, 5.1, 5.2, 6.1, 6.2	5.1, 5.2, 6.1, 6.2, 7.1	1992–2013
Libya				1992–2013
Malawi	4.1, 4.2, 6.1, 7.2	4.1, 4.2, 6.1, 6.2, 7.2	4.2, 6.1, 6.2, 7.1, 7.2	1992–2013
Mali	3.1, 4.1, 5.1, 6.2	3.1, 4.1, 5.1, 6.2	5.1, 6.2, 7.1	1992–2013
Mauritania				1992–2013
Morocco	4.1	4.1	4.1	1992–2013
Mozambique	5.1, 6.1, 7.1	6.1, 7.1	5.1, 6.1, 7.1	1992–2013
Namibia	4.1, 5.1, 6.1	4.1, 5.1, 6.1	5.1, 6.1	1992–2013
Niger	2.1, 3.1	2.1, 3.1		1992–2013
Nigeria	2.1, 4.2, 5.1, 6.1, 6.2, 7.1	2.1, 4.2, 5.1, 6.1, 6.2	4.2, 5.1, 6.1, 6.2, 7.1	1992–2013
Rwanda	5.1, 6.1, 7.1	5.1, 5.2, 6.1, 7.1	5.1, 5.2, 6.1, 7.1	1992–2013
Senegal	2.1, 4.2, 6.1, 6.2	2.1, 3.1, 4.2, 5.2, 6.1, 6.2	4.2, 5.2, 6.1, 6.2	1992–2013
Sierra Leone	5.1, 6.1	5.1, 6.1	5.1, 6.1, 7.1	1992–2013
Somalia				1992–2013
South Africa				1992–2013
South Sudan				2011–2013
Sudan				1992–2013
Swaziland	5.1	5.1	5.1	1992–2013
Tanzania	4.1, 4.2, 5.1, 6.1, 6.2	4.1, 5.1, 6.1, 6.2	4.2, 5.1, 6.1, 6.2	1992–2013
Togo	3.1, 6.1	0.1, 3.1, 6.1	6.1	1992–2013
Tunisia				1992–2013
Uganda	4.1, 5.1, 6.1, 6.2	4.1, 5.1, 5.2, 6.1	5.1, 5.2, 6.1, 6.2	1992–2013
Zambia	5.1	5.1	5.1	1992–2013
Zimbabwe	4.1, 5.1, 6.1	4.1, 5.1, 6.1	5.1, 6.1	1992–2013

Note that the divergence between the samples used from the DHS stems from the fact that not all surveys enlist the level of education of household members, come with the Child Recode file needed to derive infant mortality rates, or include the DHS wealth index.



(a) Primary education rate in sample

(b) Infant mortality in sample

Figure G.1: Primary education and infant mortality in the DHS Data, aggregated to .25 raster cells.

(a) Travel time to national capital 2015

(b) Change in travel time to national capital 1965–2015

Figure G.2: Population-weighted distribution of state reach in Africa in 2015 and development since 1965.
Note: Brighter colors identify better outcomes. To calculate population-weighted distributions for single countries, I fix the spatial distribution of the population and the borders of countries to their status in 2015. For areas that became independent after 1965 no change can be calculated. Sources: Own calculations based on 2015 WorldPop population estimates, Michelin-based road networks, and country-borders from 2015 from Cshapes (Weidmann and Gleditsch, 2010).

G.2 CROSS-SECTIONAL ANALYSIS

This section presents the full cross-sectional analysis that the main text only briefly summaries. Subsection G.2.1 first presents evidence that the cross-sectional associations of travel times to capitals with the proxies of state-society transactions from the Afrobarometer are only in part driven by differences between capitals and other locations. Subsection G.2.2 then presents the results of the robustness checks of the cross-sectional analysis of the association of travel times to national and regional capitals with local development outcomes.

G.2.1 *Afrobarometer outcomes: Controlling for capitals*

Table G.5: Time to national/regional capital and state-society transactions

	Tax & fee idx.	Law & order idx.	Gvt. services idx.	Gvt. employee (0/1)
	(1)	(2)	(3)	(4)
Time to nat. capital (log)	−0.066	−0.010	−0.237***	0.003
	(0.056)	(0.050)	(0.054)	(0.007)
Time to reg. capital (log)	−0.158***	−0.092**	−0.305***	−0.020**
	(0.061)	(0.041)	(0.045)	(0.009)
Time to nat. cap. < 1hr	0.200*	0.131	0.267**	0.012
	(0.115)	(0.092)	(0.117)	(0.011)
Time to reg. cap < 1hr	−0.043	0.237***	0.323***	0.010
	(0.072)	(0.040)	(0.058)	(0.009)
Unit:	Individual	Enum. area	Enum. area	Individual
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	-0.055	-0.029	-0.027	0.097
Std.-dev. DV:	1.4	1.3	1.7	0.3
Observations	21,204	14,296	13,007	31,146
Adjusted R^2	0.135	0.163	0.419	0.057

Notes: OLS models. Control variables are: the local population count, local cash crop suitability, mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of an area, as well as the local agricultural suitability score and a mineral deposit dummy. In addition, I add a locations' logged distance to the coast, border, and closest navigable river. Precolonial ethnic attributes are captured through their political centralization, dependence on agriculture and husbandry, and the intensity of their agricultural activities. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

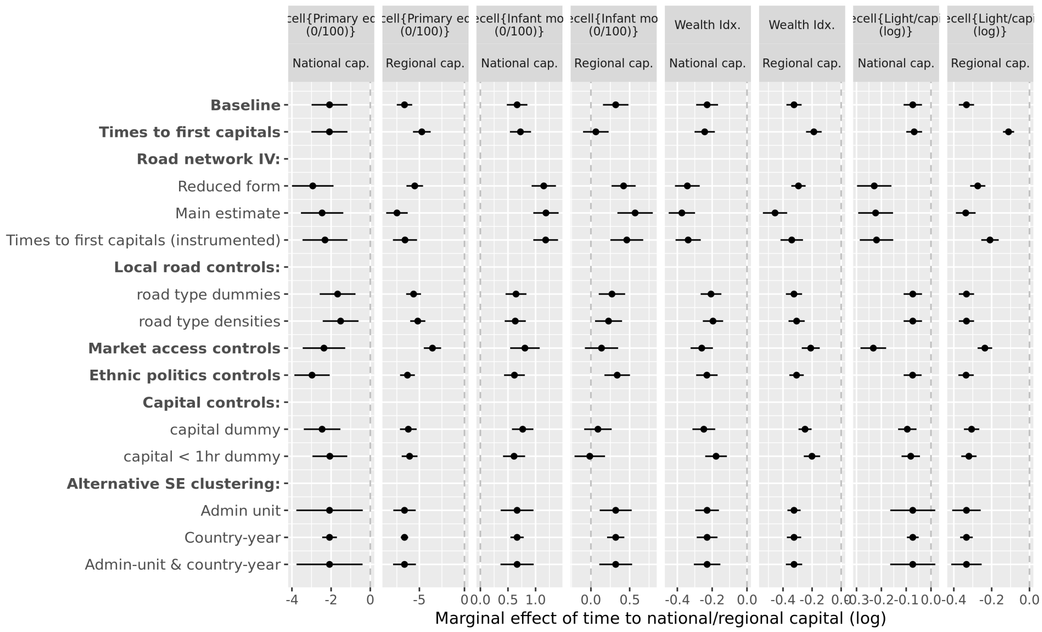


Figure G.3: Cross-sectional analysis: Summary of robustness checks.

G.2.2 *Cross-sectional development analyses: Robustness checks*

Figure G.3 summarizes the results of all robustness checks estimated for the cross-sectional analysis of the association of travel times to national and regional capitals on the four local development outcomes: primary education rates, infant mortality rates, households' wealth, and local nightlight emissions. Its first row presents the baseline results as reported in Table 7.3 in the main text. The second row reports the estimated effect of travel times to national and regional capitals observed in the first year of each country's independence. While this does not make a difference for travel times to national capitals, the effects associated with travel times to regional capitals decreases for all outcomes and reduces to zero for infant mortality rates. This is most likely due to the fact that the number of regions sharply increased over the years, thus overcoming the effect of early regional capitals. The question remains however, whether the location of the new regional capitals and borders is endogenous to local development patterns or not.

Beyond the block of results from the instrumental variable analysis, which is discussed in the next subsection, the remaining coefficients in Figure G.3 report the results from estimating the main robustness checks from the panel analysis. Their descriptions (see Section G.3) apply without exception to the cross-sectional specification. Overall, most results remain comparable to the baseline results. However exceptions to this pattern exist:

- First, as already noted, the association between travel times to regional capitals and infant mortality rates is unstable, and reduces to zero when I control for measures of international and national market access or capital dummies. This suggests that it is mainly big cities that are at the same time regional capitals that have reduced levels of infant mortality. This pattern is supported by the null-effect arising from the panel analysis.
- In general, the additional of the market access measures reduces the estimated effect of distances to regional capitals on the three other outcomes. This suggests that the respective baseline associations are to some degree driven by the effect of being close to major cities that are most often regional capitals, rather than regional capitals more specifically. Many regional capitals are not major cities and therefore not included in the set of 1243 African cities and towns used to calculate the market access measures (see Subsection G.3.4 below).
- Lastly, the clustering of standard errors on the point and country-year level leads to less conservative results than clustering errors either on the regional level, or clustering them on the regional and country-year level. However, the p-values of the estimates remain below $p < .05$ for the most part, except for those of the effect of travel times to national capitals on nightlight emissions ($p < .1$).

G.2.3 *Instrumental variable approach*

In order to gauge whether the cross-sectional baseline results are driven by endogenous road networks, I implement an instrumental variable design that closely mirrors the approach taken in Chapter 8. In Chapter 5, I have found that colonizers built roads to extract resources and that postcolonial governments did not overcome the extractive bias of their transport infrastructure. This finding raises the specter of omitted variable bias and reverse causality: places rich in resources attracted more infrastructure investments, and both correlate with local development today. Similarly, it might be that economically developed regions received more infrastructure investments, causing the cross-sectional baseline correlation to be reversely caused. The same logic of course applies also to the location of regional capitals. Their endogeneity is dealt with in the main panel analysis.

In order to account for the endogeneity of road networks, I draw on a set of simulated road networks that capture the structure of road networks observed in Africa, but are only affected by (1) the shape of countries, (2) their population distribution in 1880 (before road building took off on the continent), and (3) the type-specific road stock observed in 1966, the first year for which I have an Africa-wide Michelin map. The simulation algorithm is based on a heuristic that builds one road after the other, trying to optimally interconnect a country's population. It is described in detail in Section H.3 below. Note that, for computational reasons, the resolution of the simulations varies: 10, 20, and 40km for small, intermediate, and large countries respectively. In particular in large countries, simulated networks are thus much coarser than observed road networks (5km). This introduces (random) measurement error.

Based on the simulated road networks, I calculate each point's travel time to its regional and national capitals for every year between 1966 and 2016. I then use the travel times on the simulated road networks as instruments for the travel times measured on the observed road networks. I use these instruments in a straightforward two stage least squares setup, adding the same co-variates as at the baseline with two exceptions. First, rather than controlling for the post-treatment population count in time t, I control for the local estimated population count in 1880, which is used in the road network simulation. Second, I control for points' average travel time on the simulated road networks to the 1880 population of the region and the country they are located in. This is an effective control for the local and national population distribution, both of which affect the simulation.

Unsurprisingly and coinciding with the strong first state obtained in Chapter 8, the simulated travel times are highly predictive of observed travel times, both when I do not control for 'foot-travel times' in Table G.6. The results of the reduced form estimation of this setup are plotted in Figure G.3. All estimates point in the expected direction, with levels of local development decreasing in travel times to national and regional capitals. They also show larger effects of travel times to national capitals than estimated with the naive baseline specification. The main 2SLS estimates, plotted below in the same Figure and presented in Table G.7 mostly coincide with the reduced

Table G.6: Time to national/regional capital and local development: First stage

	Households		Cells	
	Nat. cap.	Reg. cap.	Nat. cap.	Reg. cap.
	(1)	(2)	(3)	(4)
Time to nat. capital (log; sim)	0.899***	0.006	1.007***	0.011
	(0.023)	(0.019)	(0.011)	(0.011)
Time to reg. capital (log; sim)	0.020***	0.620***	0.094***	0.750***
	(0.007)	(0.019)	(0.005)	(0.008)
Country-year FE:	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	1.8	1.1	2.9	2.3
Observations	712,212	712,212	1,378,909	1,378,909
Adjusted R^2	0.888	0.684	0.893	0.787

Notes: First-stage linear models. Control variables consist of the local population count in 1880 (log), as well as a locations' logged connectdness to the average 1880 population of the region and country based on the simulated road-network, local cash crop suitability, mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of an area, as well as the local agricultural suitability score and a mineral deposit dummy. In addition, I add a locations' logged distance to the coast, border, and closest navigable river. Precolonial ethnic attributes are captured through their political centralization, dependence on agriculture and husbandry, and the intensity of their agricultural activities. In Models 1 and 2, I furthermore control for households' size and the number of children (both linear and squared). Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

form estimates, howing large and statistically significant negative effects of travel times to capitals on local development, measured through primary education and infant mortality rates, households' wealth, as well as local nightlight emissions.

Furthermore, I implement two variations of the instrumental variable design. In the first, I control for point's 'foot travel time' to regional and national capitals. These two controls capture the geodesic distance to capitals, thus leaving only the difference that roads make by unevenly contracting physical space for the instruments to capture.[1] However, these two measures of foot travel times also control for the location of regional and national capitals, thus controlling for part of the treatment (roads × capital locations) I am studying here. The results of this conservative test (Table G.8) point in the same direction than the main IV-estimates. Standard errors are however larger, and the estimate of the impact of travel time to national capitals on households' wealth decreases to almost zero. The respective effects on education and infant mortality rates increase in absolute size, but also in imprecision, making it hard to gauge whether the effects are truly different from the main specification.

1 This is the specification used in Chapter 8.

Table G.7: Time to national/regional capital and local development: Main 2SLS-IV

	Primary educ. (0/100)	Infant mort. (0/100)	Wealth Idx.	Light/capita (log)
	(1)	(2)	(3)	(4)
Time to nat. capital (log)	-2.459^{***}	1.188^{***}	-0.374^{***}	-0.223^{***}
	(0.551)	(0.117)	(0.039)	(0.036)
Time to reg. capital (log)	-7.487^{***}	0.570^{***}	-0.455^{***}	-0.337^{***}
	(0.609)	(0.116)	(0.043)	(0.027)
Country-year FE:	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	70	9.9	-0.026	-6.5
1st stage Nat. Cap.:	7550.47	6979.31	1570.24	10938.1
1st stage Reg. Cap.:	4939.4	4379.18	1049.28	9171.71
Observations	1,659,381	2,315,018	712,212	1,378,909
Adjusted R^2	0.348	0.039	0.361	0.292

Notes: 2SLS-IV models. Control variables consist of the local population count in 1880 (log), as well as a locations' logged connectdness to the average 1880 population of the region and country based on the simulated road-network, local cash crop suitability, mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of an area, as well as the local agricultural suitability score and a mineral deposit dummy. In addition, I add a locations' logged distance to the coast, border, and closest navigable river. Precolonial ethnic attributes are captured through their political centralization, dependence on agriculture and husbandry, and the intensity of their agricultural activities. Control variables for models of primary education consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. In models of households' wealth, I control for the size and the number of children (both linear and squared). Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

These patterns suggest that regional administrative geographies drive parts of the reported cross-sectional results.

Second, I estimate the main IV-specification (without foot travel time controls) but taking travel times to regional and national capitals at the time of independence as the main explanatory and instrumental variables. The results are again plotted in Figure G.3 and largely coincide with the main estimates. This is with the exception for the effect of the time to regional capitals at independence on infant mortality and local nightlights. Both estimates are of smaller absolute size, mirroring the results of the respective naive OLS specifications plotted in the second row of the Figure G.3.

All in all, and with deviations in the size of estimates, these results support the cross-sectional baseline results. However, and as noted in Chapter 7, the instrumental strategy is not geared to address the endogeneity of capital locations. As Chapter 5 has shown, regional capitals (and borders) in particular have been placed and relocated as a response to local environments and are therefore endogenous to long-

Table G.8: Time to national/regional capital and local development: 2SLS IV, foot-travel controls

	Primary educ. (0/100)	Infant mort. (0/100)	Wealth Idx.	Light/capita (log)
	(1)	(2)	(3)	(4)
Time to nat. capital (log)	−6.421**	2.601***	−0.158	−0.358**
	(3.231)	(0.653)	(0.161)	(0.178)
Time to reg. capital (log)	−8.213***	1.163*	−0.340**	−0.488**
	(2.874)	(0.704)	(0.159)	(0.220)
Time to nat. capital (foot; log; sim)	1.889	−0.668**	−0.096	0.081
	(1.523)	(0.308)	(0.076)	(0.099)
Time to reg. capital (foot; log; sim)	0.481	−0.320	−0.058	0.107
	(1.131)	(0.277)	(0.056)	(0.124)
Country-year FE:	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	70	9.9	-0.026	-6.5
1st stage Nat. Cap.:	438.7	426.99	73.17	249.32
1st stage Reg. Cap.:	240.96	267.58	70.85	145.47
Observations	1,659,381	2,315,018	712,212	1,378,909
Adjusted R^2	0.348	0.039	0.368	0.279

Notes: 2SLS-IV models. Control variables consist of the local population count in 1880 (log), as well as a locations' logged connectdness to the average 1880 population of the region and country based on the simulated road-network, local cash crop suitability, mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of an area, as well as the local agricultural suitability score and a mineral deposit dummy. In addition, I add a locations' logged distance to the coast, border, and closest navigable river. Precolonial ethnic attributes are captured through their political centralization, dependence on agriculture and husbandry, and the intensity of their agricultural activities. Control variables for models of primary education consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. In models of househols' wealth, I control for the size and the number of children (both linear and squared). Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

run economic development which constitutes the main source of variation explained by the cross-sectional models. This concern is less threatening when it comes to the estimated effects of travel times national capitals which are less subject to relocation and were established early on in the colonial period. As argued in Section 7.5 in the main chapter, the resulting cross-sectional endogeneity is best addressed by estimating the effect of changes in travel times to regional and national capitals in a panel setup that keeps time-invariant attributes of locations constant through the use of point fixed effects.

G.3 PANEL ANALYSIS: ROBUSTNESS CHECKS

This Section describes additional analyses that probe the robustness of the analysis of the effects of changes in the distance to national and regional capitals on local development. Figure G.4 provides an overview over the various robustness checks. All additional models are described in detail below. Where Figure G.4 captures the main insights from an additional analysis, I do not report detailed results as a table. However, the reader may note that all results will be made available as tables with the replication data.

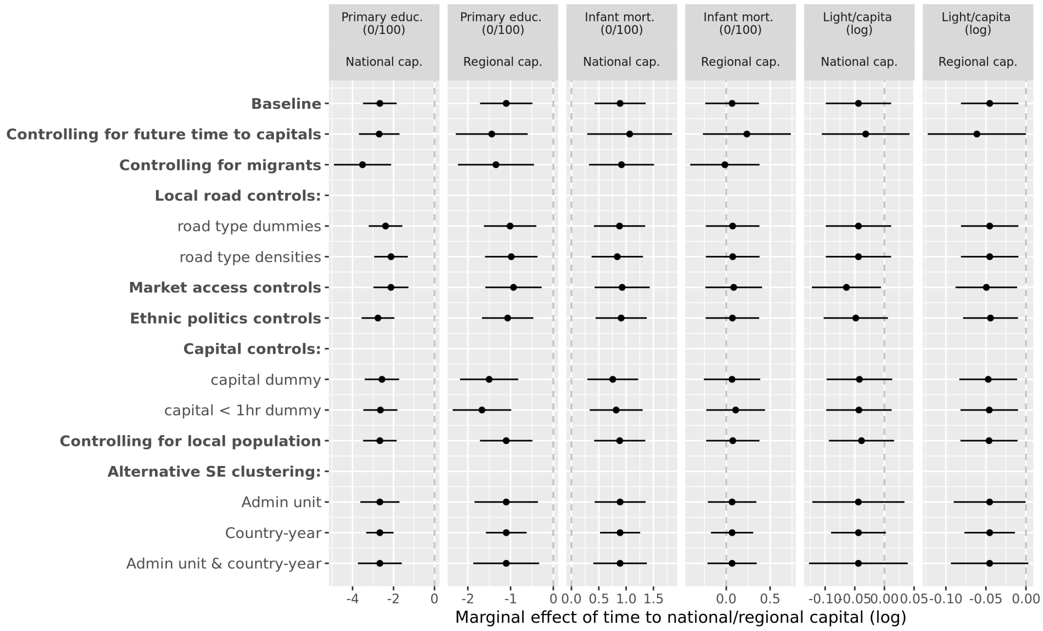


Figure G.4: Panel analysis: Summary of robustness checks.

G.3.1 *Re-locations of national capitals vs. road-network development:*

One main concern of the analysis of changes in travel times to national capitals is that we observe only very few relocations of capitals that drive the results. In the sample of DHS respondents, capital re-locations have occurred in Côte d'Ivoire (Abidjan to Yamassoukro), Nigeria (Lagos to Abuja), and Tanzania (the de jure move from Dar es Salaam to Dodoma). The data on nightlights include the secessionist cases of Eritrea and South Sudan. In order to gauge whether the baseline estiamtes are due to these relocations and secessions or whether they are driven by changes in the road networks that link national capitals to their citizens, Table G.9 presents the results of estimating the baseline panel specification on the split samples. For primary education rates we observe a larger effect in the sample of DHS respondents from countries with capital relocations, than from those without. However, also in the latter case the estimate is substantive and statistically significant, meaning that improved road connections to national capitals come with increases in local primary education rates. In the case of infant mortality rates, the results show them to be driven by capital relocations rather than road network improvements. Lastly and consistent with the baseline results, the results show no significant effect of either better road connections or new capitals on nightlight emissions. Note however, that the secessionist cases of Eritrea and South Sudan are each only observed one year pre-/post-treatment. We would hardly expect the new capitals to have such an immediate and sudden effect on nightlight emissions, in particular since both cases where riven by civil war before (and after, in the case of South Sudan) their secession.

Table G.9: Changes in time to national/regional capital and local development: Cases where national capitals changed

	Primary educ. (0/100)		Infant mort. (0/100)		Light/capita (log)	
	(1)	(2)	(3)	(4)	(5)	(6)
Time to nat. capital (log)	−1.445**	−3.400***	0.060	1.474***	−0.031	−0.056
	(0.652)	(0.522)	(0.302)	(0.320)	(0.035)	(0.037)
Time to reg. capital (log)	−1.361***	−0.689	0.003	0.948**	−0.048**	−0.044*
	(0.364)	(0.631)	(0.166)	(0.448)	(0.020)	(0.024)
Cap.-re-loc. cases:	drop	only	drop	only	drop	only
Point FE:	yes	yes	yes	yes	yes	yes
Country-year FE:	yes	yes	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes	yes	yes
Controls:	yes	yes	yes	yes	–	–
Mean DV:	70	71	9.7	11	-6.5	-6.8
Observations	1,583,837	309,230	2,247,372	387,294	1,467,083	39,908
Adjusted R^2	0.445	0.446	0.051	0.049	0.838	0.395

Notes: OLS linear models. Control variables for models with primary education as the dependent variable consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

G.3.2 *Testing for parallel trends:*

As highlighted in Section 7.5.3 of the main Chapter, one main threat to inference in the panel analysis is that the baseline estimates may be biased by non-parallel trends in local development that reversely cause the extension of state reach. I here present the full details of the empirical test that accounts for such trends. In particular, I re-estimate the baseline specification, adding measures of future travel times to capitals in $t+x$. These leads of the main treatments capture differential changes in local development that occur before changes in travel times affect localities. More specifically, for the analysis of local primary education rates, which are affected by state reach during age 6 to 11 of respondents, I estimate the effect of the average travel time to capitals during age 6-11, and add five separate controls for the travel times to capitals at age 12 to 16. For infant mortality rates, which are affected only in the year of an infants' birth, I add the time to capitals at age 2 to 6. Similarly, for local nightlight emisions, I add the respective variables for $t+1...5$.

Table G.10 and Figure G.5 show the results of these specifications. The first thing to note is that the main estimates of the effects of travel times to capitals are only marginally and insignificantly different from those obtained in the baseline specification. This is a first sign that these are not affected by non-parallel trends. Second however, and as Table G.10 shows, the sum of leads is negative and marginally significant in the estimation of the effect of travel times to national capitals on education rates. This suggests that education rates increase before capitals move closer to a location through roads or relocation. The leads in the infant mortality analysis are in sum close to zero and show few signs of non-parallel trends. The leads in the nightlight analysis have very heterogeneous estimates, but are, for the most part statistically insignificant and in sum not different from zero. Both patterns suggest that the main estimate is not affected by non-parallel trends.

One reason for the negative and marginally significant lead effect of travel times to national capitals on primary education rates consists in biased migration patterns by which individuals select in and out of treatment after having gone to school. These patterns can be picked up by the lead effects because I attribute travel times on the basis of the current location of DHS respondents. In order to test for this possibility, I re-estimate the respective specification, now interacting the treatment variables and

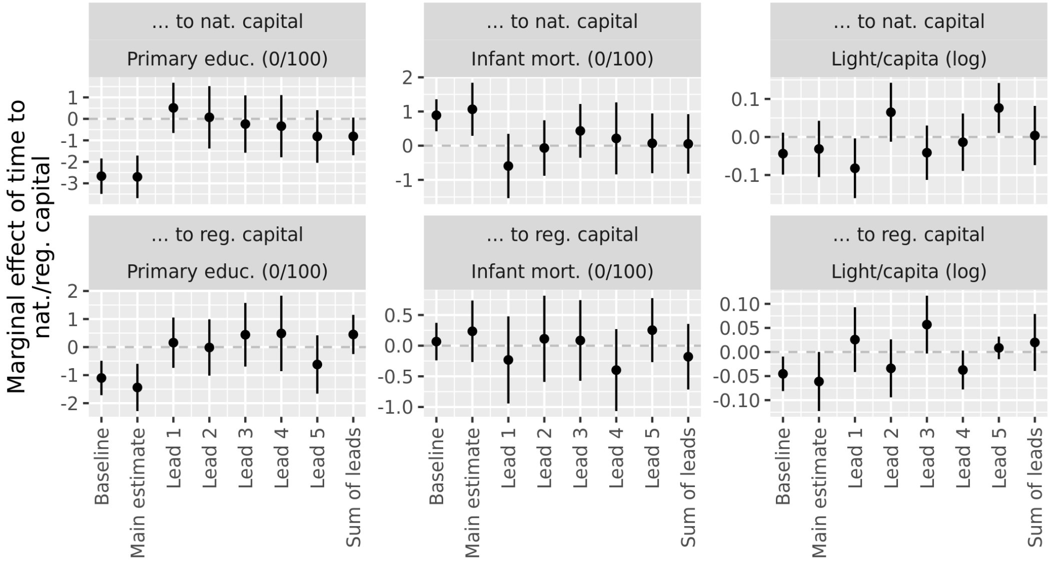


Figure G.5: All leads, based on Table G.10.

Table G.10: Changes in time to national/regional capital and local development: Controlling for leads

	Primary educ. (0/100)	Infant mort. (0/100)	Light/capita (log)
	(1)	(2)	(3)
Time to nat. capital (log)	−2.700***	1.065***	−0.032
	(0.506)	(0.396)	(0.038)
Time to reg. capital (log)	−1.440***	0.235	−0.061*
	(0.429)	(0.256)	(0.031)
Sum of leads (nat. cap.):	−0.816*	0.052	0.004
	(0.448)	(0.444)	(0.04)
Sum of leads (reg cap.):	0.45	−0.18	0.02
	(0.357)	(0.273)	(0.03)
Time to cap.$_{t+1,\ldots,t+5}$:	yes	yes	yes
Point FE:	yes	yes	yes
Country-year FE:	yes	yes	yes
Survey FE:	yes	yes	yes
Controls:	yes	yes	–
Mean DV:	70	10	-6.5
Observations	1,887,591	2,561,990	1,369,902
Adjusted R^2	0.445	0.050	0.840

Notes: OLS linear models. Control variables for models with primary education as the dependent variable consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

their leads with a dummy for migrants.[2] The respective variable is only available for the reduced sample of DHS respondents that have gone through the entire interview and is based on whether they have 'always' lived in their current place of residence.[3]

Reassuringly, the results in Table G.11 show that the lead effects are only negative for the migrants in the sample, but positive for the non-migrants. Both sums of leads are statistically insignificant, presumable due to the smaller sample size. Furthermore, the main effect associated with travel times to national and regional capitals is much larger in the non-migrant sample than in the migrant sample, which is consistent with the fact that non-migrants' primary education is affected by local state reach to greater extent than that of migrants. The results for the respective analysis of the mortality of infants of non-migrant mothers mirror those described above. The mortality of infants of migrant mothers is not affected by state reach at the time of their birth in the location of their mothers' residence at the time of the interview.

2 The baseline results for this migrant $\times$ travel times interaction without the leads is reported below in Subsection G.3.3.

3 Note that the respective question does not allow to distinguish individuals who have moved within the same neighborhood from those who have migrated from one place to another. The migrant dummy therefore overestimates migration.

Table G.11: Changes in time to national/regional capital and local development: Controlling for leads, migrants and non-migrants

	Primary educ. (0/100)	Infant mort. (0/100)
	(1)	(2)
Migrant	−4.473***	0.421*
	(1.037)	(0.223)
Non-migrants: Time to nat. capital (log)	−5.330***	1.299**
	(1.115)	(0.520)
Non-migrants: Time to reg. capital (log)	−1.594**	0.284
	(0.699)	(0.371)
Migrants: Time to nat. capital (log)	−2.989***	0.674
	(0.915)	(0.482)
Migrants: Time to reg. capital (log)	0.567	−0.249
	(0.667)	(0.401)
Non-migrant leads:		
Sum of leads (nat. cap.):	1.012	−0.122
	(0.944)	(0.558)
Sum of leads (reg. cap.):	0.167	−0.257
	(0.599)	(0.386)
Migrant leads:		
Sum of leads (nat. cap.):	−0.536	0.568
	(0.694)	(0.529)
Sum of leads (reg. cap.):	−0.355	0.015
	(0.629)	(0.413)
Time to cap.$_{t+1,\dots,t+5}$:	yes	yes
Point FE:	yes	yes
Country-year FE:	yes	yes
Survey FE:	yes	yes
Controls:	yes	yes
Mean DV:	67	11
Observations	688,712	1,604,848
Adjusted R^2	0.484	0.053

Notes: OLS linear models. Control variables for models with primary education as the dependent variable consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

G.3.3 *Migration:*

Over their lifetime, DHS respondents might have moved towards or away from changing regional (and national) capitals in a manner correlated with their level of education and wealth. Such migration patterns might bias the results. If that was the case, we should see differential effects of travel times among migrants and non-migrants. In particular, if migrants were driving the results, no effect of changes in the travel time towards capitals should be visible among non-migrants. Table G.12 demonstrates that this is not the case. The effect of travel times on education rates is significantly larger for migrants than for non-migrants. In the case of infant mortality rates, the difference between the two sample is mostly insignificant, except for Model (4), which suggest that longer travel times to regional capitals have a more *negative* effect on infant mortality rates among migrant- than non-migrant mothers. Their absolute effect is however insignificantly different from zero in both cases (see also the lead-analysis above, Table G.11).

Table G.12: Changes in time to national/regional capital and local development: Controlling for migration

	Primary educ. (0/100)		Infant mort. (0/100)	
	(1)	(2)	(3)	(4)
Time to nat. capital (log)	−3.514***	−3.275***	0.915***	0.749**
	(0.713)	(0.690)	(0.304)	(0.299)
Time to reg. capital (log)	−1.342***	−1.193***	−0.016	−0.011
	(0.454)	(0.449)	(0.202)	(0.209)
Migrant	−4.534***	−15.414***	0.427**	0.894***
	(1.014)	(2.229)	(0.214)	(0.261)
Migrant×Time to nat. capital (log)	0.872**	0.757*	0.047	0.067
	(0.416)	(0.412)	(0.125)	(0.128)
Migrant×Time to reg. capital (log)	1.593***	1.308***	−0.227	−0.248*
	(0.344)	(0.343)	(0.142)	(0.145)
Migrant × controls	no	yes	no	yes
Point FE:	yes	yes	yes	yes
Country-year FE:	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	67	67	10	10
Observations	690,657	690,657	1,646,591	1,656,444
Adjusted R^2	0.484	0.484	0.053	0.036

Notes: OLS linear models. Control variables for models with primary education as the dependent variable consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

G.3.4 *Omitted variables:*

CONTROLLING FOR ROADS: The main results might also be driven by roads that are built at the local level around specific towns and villages. Because such road building inherently lowers the distance to all capitals, it might lead to spurious results

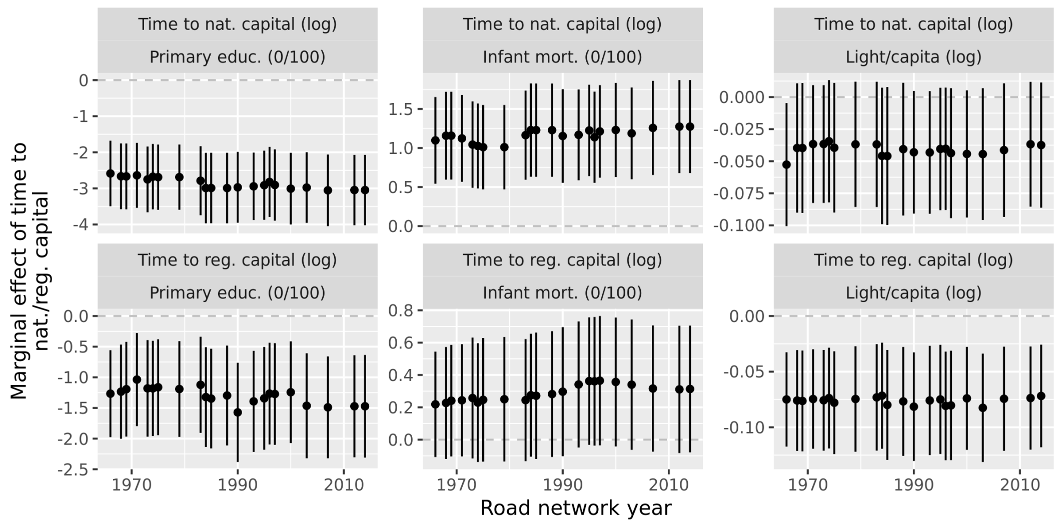


Figure G.6: Estimating the baseline models with constant road networks from varying years

if it was caused by increasing levels of development in these areas. To exclude such omitted variable bias, I employ two strategies. First, I control for the mileage of roads in the geographic neighborhood of respondents (20km) / in a Voronoi cell (see Figure G.4). This is similar to the strategy of Donaldson and Hornbeck (2016) in that the variation left stems from changes in the road network outside a particular point's neighborhood. The second and more stringent test is to re-estimate all models with non-time variant road networks. In such a setting, all variation within points stems solely from changes in the administrative design of a country – that is the location of boundaries and capitals. I do so with each Michelin network that I observe. The results, plotted in Figure G.6, are insensitive towards these changes – it appears that the magnitude of baseline results are unaffected by purging the model of temporal variance induced through changes in road networks. If at all, the absolute size of the estimates slightly increases. This can be seen as an indication that the use of the time-variant road networks in which the imperfect alignment of roads across maps introduces random noise, cause a slight attenuation bias.

CONTROLLING FOR MARKET ACCESS: Travel times to regional and national capitals might not only proxy for transaction costs between governments and citizens, but more broadly transaction costs between the participants of economic markets. Variation in economic market access might lead to spurious results, since it results in higher levels of economic activity and development (e.g. Donaldson, 2018; Donaldson and Hornbeck, 2016; Eaton and Kortum, 2002; Jedwab and Moradi, 2016; Jedwab and Storeygard, 2018). I thus follow the economic literature on the effect of market access on economic growth and calculate, for each year, the road-network based access to national and international markets. Following Donaldson (2018) and Eaton and Kortum (2002), I define the measure as:

$$MA_{p,t} = \sum_{m=1}^{M} c_{p,m,t}^{-\theta} * P_{m,t},$$

where the market access of point p in year t is the sum of the market potential P of a market m in year t multiplied by the travel time between p and m calculated on the road network and discounted by a trade elasticity θ. Because Donaldson (2018) and Eaton and Kortum (2002) estimate different trade elasticity measures ($\theta = 8.28$ and 3.2 respectively), I construct the market access measure for both parameters. Because the effects of access to national and international markets might differ, I calculate MA separately for markets inside and outside of p's country. I define markets as the 1243 most important cities and towns in Africa[4], each weighted with the population in a .1 decimal degree buffer around the cities centroid in a particular decade (from Goldewijk, Beusen and Janssen, 2010).[5] Controlling for the four resulting variables in Table G.13 indicates does not affect the magnitude of the effects associated with the measure of state reach. This indicates that a low distance towards capital cities increases local development above and beyond the effect of the economic markets they harbor. The effect of market access on education rates and nightlight emissions is slightly positive, once we take the sum of the respective coefficients. They have no discernible effect of infant mortality.

CONTROLLING FOR ETHNIC POLITICS AND WAR: Ethnic politics are an important driver of both, development and state reach. Research on administrative unit reforms has found that they can reward government allies (Green, 2010; Hassan, 2016; Gottlieb et al., 2019) or harm opponents (Resnick, 2017), strategies which may well be used in processes of ethnic accommodation or exclusion. At the same time, ethnic and regional favoritism of governments has been found to affect investments into road infrastructure (Burgess et al., 2015) and local development in general (Franck

4 From www.naturalearthdata.com.

5 Note that this approach is less precise than using the more voluminous Africapolis database released in 2019 (http://www.africapolis.org) which will be integrated into this Chapter in the future.

Table G.13: Changes in time to national/regional capital and local development: Controlling for market access

	Primary educ. (0/100)	Infant mort. (0/100)	Light/capita (log)
	(1)	(2)	(3)
Time to nat. capital (log)	-2.126^{***}	0.927^{***}	-0.064^{**}
	(0.436)	(0.257)	(0.030)
Time to reg. capital (log)	-0.931^{***}	0.085	-0.049^{**}
	(0.337)	(0.165)	(0.020)
MA, internat. (log; $\theta = 3.8$)	2.491^{***}	-0.121	-0.124^{***}
	(0.723)	(0.318)	(0.020)
MA, nat. (log; $\theta = 3.8$)	-0.664^{***}	0.122	0.058^{***}
	(0.231)	(0.101)	(0.007)
MA, internat. (log; $\theta = 8.28$)	1.038^{**}	0.140	0.007
	(0.513)	(0.283)	(0.013)
MA, nat. (log; $\theta = 8.28$)	-0.464^{**}	-0.078	0.009^{*}
	(0.190)	(0.112)	(0.005)
Point FE:	yes	yes	yes
Country-year FE:	yes	yes	yes
Survey FE:	yes	yes	yes
Controls:	yes	yes	–
Mean DV:	70	9.9	-6.5
Observations	1,889,905	2,634,607	1,506,991
Adjusted R^2	0.446	0.051	0.836

Notes: OLS linear models. Control variables for models with primary education as the dependent variable consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

and Rainer, 2012; Hodler and Raschky, 2014). To capture such dynamics, I draw on the most comprehensive and geocoded data on ethnic power dynamics in Africa since independence, the Ethnic Power Relations data set (Vogt et al., 2015).[6] I use the geodata of ethnic groups to map respondents and Voronoi cells to the database's coding of ethnic inclusion and exclusion as well as the occurrence and history of ethnic civil wars. Table G.14 shows the results from the baseline analysis with the resulting variables as additional controls. The additional controls do not affect the results. While in particular the time since the last ethnic civil war positively affects education rates, ethnic inclusion has a positive effect on infant survival rates and nightlight emissions.

6 All data can be freely downloaded from growup.ethz.ch.

Table G.14: Changes in time to national/regional capital and local development: Controlling for ethnic representation and civil war

	Primary educ. (0/100)	Infant mort. (0/100)	Light/capita (log)
	(1)	(2)	(3)
Time to nat. capital (log)	−2.760***	0.909***	−0.048*
	(0.408)	(0.239)	(0.028)
Time to reg. capital (log)	−1.068***	0.070	−0.044**
	(0.307)	(0.156)	(0.018)
Ethnic inclusion (0/1)	0.859	−0.586**	0.084***
	(0.707)	(0.231)	(0.024)
Ethnic exclusion (0/1)	0.838	−0.346	0.035*
	(0.703)	(0.219)	(0.020)
Ethnic civil war (0/1)	0.353	−0.036	0.037***
	(0.788)	(0.222)	(0.010)
Eth. war since indep. (0/1)	0.600	−0.394	−0.048***
	(0.903)	(0.312)	(0.017)
Time since eth. war	0.428***	0.009	−0.001
	(0.152)	(0.038)	(0.001)
Time since eth. war^2	−0.014***	0.001	0.00000
	(0.004)	(0.001)	(0.00002)
Point FE:	yes	yes	yes
Country-year FE:	yes	yes	yes
Survey FE:	yes	yes	yes
Controls:	yes	yes	–
Mean DV:	70	9.9	-6.5
Observations	1,893,067	2,626,737	1,506,991
Adjusted R^2	0.445	0.051	0.836

Notes: OLS linear models. Control variables for models with primary education as the dependent variable consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

CONTROLLING FOR CAPITALS: Another potential danger is that as some cities may become, for a variety of reasons, richer with time, and then benefit from a political upgrade and get their own administrative unit. In such cases, changes in state reach in that city would be endogenous to local development. In an additional robustness check I therefore include dummies for whether an interview-location was (1) in and (2) closer than 1 hour to a regional and national capital. For the Voronoi units, I create analagous measures when they either contain a capital or have an average distance to a capital of below 1 hour. The results in Table G.15 highlight that changes in distances towards capitals in locations which are not capitals themselves drive the baseline patterns. Above and beyond the effect associated with a reduction in travel times, becoming a regional capital is associated with *lower* education rates, and more mixed patterns in the other outcomes. Relocations of national capitals are associated with reductions of infant mortality rates in the new capitals.

Table G.15: Changes in time to national/regional capital and local development: Controlling for capitals

	Primary educ. (0/100)		Infant mort. (0/100)		Light/capita (log)	
	(1)	(2)	(3)	(4)	(5)	(6)
Time to nat. capital (log)	−2.566***	−2.641***	0.756***	0.817***	−0.042	−0.043
	(0.428)	(0.425)	(0.237)	(0.247)	(0.028)	(0.028)
Time to reg. capital (log)	−1.501***	−1.670***	0.066	0.107	−0.047**	−0.046**
	(0.347)	(0.350)	(0.164)	(0.171)	(0.018)	(0.018)
National capital (0/1)	−0.523		−5.838**		0.212	
	(3.293)		(2.331)		(0.272)	
Regional capital (0/1)	−2.846**		−0.084		−0.054	
	(1.126)		(0.456)		(0.053)	
Time to nat. cap. < 1hr		−0.016		−0.373		0.136*
		(0.533)		(0.273)		(0.071)
Time to reg. cap < 1hr		−1.217***		0.091		−0.026
		(0.280)		(0.138)		(0.030)
Point FE:	yes	yes	yes	yes	yes	yes
Country-year FE:	yes	yes	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes	yes	yes
Controls:	yes	yes	yes	yes	–	–
Mean DV:	70	70	9.9	9.9	-6.5	-6.5
Observations	1,893,057	1,893,067	2,634,652	2,634,666	1,506,991	1,506,991
Adjusted R^2	0.445	0.445	0.051	0.051	0.836	0.836

Notes: OLS linear models. Control variables for models with primary education as the dependent variable consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

G.3.5 *Varying education- and health-related outcomes:*

Lastly, two sets of additional analyses gauge whether the results are applicable to alternative education and health care outcomes. First, Table G.16 shows very similar effects of the travel time to regional and national capitals on (1) whether a respondent has spent any time in school, (2) on her years of schooling – logged and linear, and (3) on a simple secondary education dummy. The main deviation from the baseline model is that the distance to the national capital does not seem to impact secondary education levels. With regard to infant mortality, Table G.17 indicates that increased infant mortality in regions of low state reach can indeed be related to a lower availability of professional prenatal assistants. Similarly, changes in travel times to national capitals come with an increased chance that a child is born in a public clinic and positively relate to the receipt of professional assistance during delivery. Conversely, if children are born under low levels of state reach from the national capital, assistance is given more often in traditional manner. Consistent with the earlier results, the distance to regional capitals does not have any significant effect on the receipt of professional prenatal or birth assistance. Regarding local development measured through nightlight emissions, Table G.18 shows that the choice of outcomes – whether nightlights per capita (log), absolute nightlights (log), or a dummy for whether a Voronoi cell exhibits any nightlight emissions does not produce different conclusions. In all three cases, changes in travel times to regional capitals are associated with more nightlight, while the estimated effect of changes in travel times towards national capitals are statistically insignificant, with the exception of their effect on the simple nightlight dummy ($p < .1$).

Table G.16: Changes in time to national/regional capital and various education outcomes

	Any educ. (0/100)	Educ. years (linear)	Educ. years (log)	Sec. educ. (0/100)
	(1)	(2)	(3)	(4)
Time to nat. capital (log)	−2.654***	−0.218***	−0.053***	0.119
	(0.420)	(0.048)	(0.009)	(0.439)
Time to reg. capital (log)	−1.080***	−0.066**	−0.023***	−0.954***
	(0.317)	(0.032)	(0.007)	(0.324)
Point FE:	yes	yes	yes	yes
Country-year FE:	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	70	5.4	1.5	35
Observations	1,890,391	1,890,391	1,890,391	1,615,848
Adjusted R^2	0.443	0.459	0.483	0.356

Notes: OLS linear models. Control variables include respondents' age and age squared, as well as a female dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Table G.17: Changes in time to national/regional capital and quality of prenatal care and birth assistance (in percent)

	Prof. prenatal care	Birth in public inst.	Prof. birth assist.	Trad. birth assist.
	(1)	(2)	(3)	(4)
Time to nat. capital (log)	−7.428***	−3.440***	−6.231***	3.911**
	(2.474)	(1.287)	(2.056)	(1.537)
Time to reg. capital (log)	0.812	0.681	1.091	0.736
	(0.934)	(0.997)	(1.001)	(0.685)
Point FE:	yes	yes	yes	yes
Country-year FE:	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	81	21	54	18
Observations	419,768	579,787	578,573	573,685
Adjusted R^2	0.450	0.543	0.444	0.307

Notes: OLS linear models. Control variables include an infant's mother's age and age squared, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

Table G.18: Changes in time to national/regional capital and various nightlight measures

	Light/capita (log)	Light (log)	Any Light (0/100)
	(1)	(2)	(3)
Time to nat. capital (log)	−0.044	−0.091	−1.048*
	(0.028)	(0.075)	(0.610)
Time to reg. capital (log)	−0.045**	−0.140**	−1.097**
	(0.018)	(0.061)	(0.498)
Unit FE:	yes	yes	yes
Country-year FE:	yes	yes	yes
Survey FE:	yes	yes	yes
Controls:	–	–	–
Mean DV:	-6.5	-5.4	12
Observations	1,506,991	1,506,991	1,506,991
Adjusted R^2	0.836	0.838	0.784

Notes: OLS linear models. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

G.3.6 *Additional robustness checks:*

COUNTRY-WEIGHTS: Because the DHS has not regularly sampled all African countries, the weights each country receives in the baseline specifications vary considerably. If the effects of state capacity on development vary systematically with the number and size of survey the DHS by countries, the results would be biased towards the most-sampled set of countries. Table G.19 addresses this caveat by weighting each observation by the inverse of the number of observations from its country (Models 1 and 3) and from the respective cohort in the same country (Models 2 and 4). The latter serves to prevent that the biggest cohorts drive the results at the expense of dynamics in smaller cohorts observed in the data.[7] Though coefficients slightly change, the results remain generally consistent with those reported at the baseline. Lastly, Model 5 addresses the potential problem that until now I have treated all Voronoi cells in the same manner, thus giving equal weight to areas with many and few inhabitants. Weighting the Voronoi units by their population doubles the estimated effect of the travel time to regional capitals on nightlight emissions. This is reassuring, since it shows that the baseline models are not driven by variation in nightlight emissions in uninhabited regions.

Table G.19: Changes in time to national/regional capital and local development: Alternative weights

	Primary educ. (0/100)		Infant mort. (0/100)		Light/capita (log)
	(1)	(2)	(3)	(4)	(5)
Time to nat. capital (log)	−1.786***	−2.425***	0.567**	0.924**	−0.047
	(0.432)	(0.479)	(0.251)	(0.402)	(0.039)
Time to reg. capital (log)	−1.256***	−1.409***	0.071	0.139	−0.108***
	(0.314)	(0.330)	(0.170)	(0.355)	(0.034)
Weights	country	country cohort	country	country cohort	population
Point FE:	yes	yes	yes	yes	yes
Country-year FE:	yes	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes	yes
Mean DV:	70	70	9.9	9.9	-6.5
Observations	1,893,067	1,893,067	2,634,666	2,634,666	1,506,991
Adjusted R^2	0.427	0.422	0.054	0.155	0.949

Notes: OLS linear models. Control variables for models with primary education as the dependent variable consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01

STANDARD ERROR SPECIFICATIONS: I furthermore conduct a series of permutations to the geographic level on which standard errors are clustered. The results are robust to clustering on the level of administrative units, country-years, or the combination of both (see Figure G.4 above).

7 Because of the sampling scheme of the DHS and attrition-by-death, cohorts close to the date of the survey are bigger than those in the past.

VARYING THE SIZE OF VORONOI UNITS: This last robustness check assesses the impact of the choice of the size of Voronoi units used in the baseline analysis (400km^2). Figure G.7 plots the results of the baseline specification estimated with units with exponential increases in their size from 100 to 6400km^2. Reassuringly, the Figure shows that small units generally give rise to smaller estimated and standardized effects. The increases in the respective effects that comes with larger units might be due to the unit constant added to the nightlight measure, which has a larger effect in small units in which more units have no observed nightlight emissions. Throughout, decreases in travel times to regional capitals are associated with more nightlights. Decreases in travel times to national capitals are only associated with more nightlights in large units. This result adds to the uncertainty discussed in the main chapter whether travel times to national capitals are indeed related to local nightlight emissions or not.

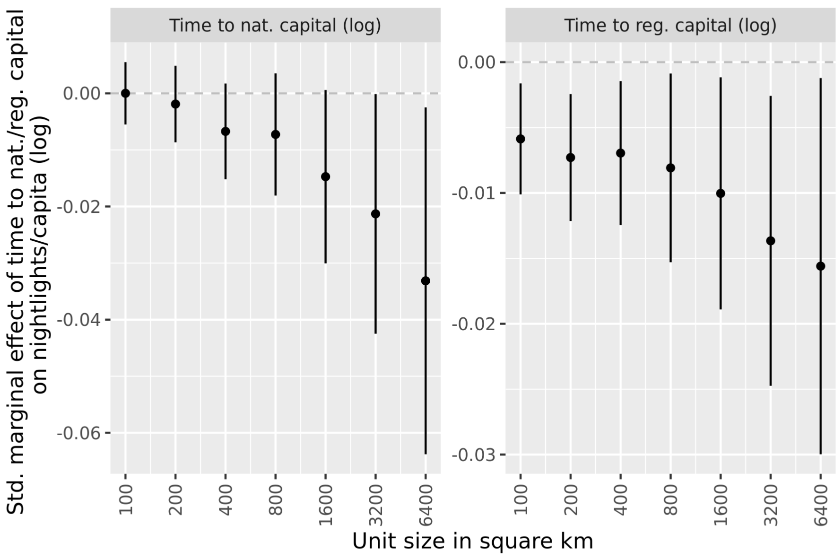


Figure G.7: Effect of travel times to national and regional capitals on nightlight emissions in Voronoi units of increasing size.
Estimated coefficients and their 95% CI are standardized by dividing them by the mean dependent variable (log(.001 + nightlight)).

H

APPENDIX: ROADS TO RULE, ROADS TO REBEL

This appendix provides information on the summary statistics of the data used in Chapter 8 (Section H.1), the results of all robustness checks (Section H.2), as well as details on the simulation of road networks (Section H.3) and the instrumental variable strategy (Section H.4).

H.1 SUMMARY STATISTICS

Table H.1: Summary statistics

Statistic	N	Mean	St. Dev.	Min	Max
Challengers	31,780	0.51	2.41	0	128
Challenger Events	31,780	1.02	13.33	0	739
State Events	31,780	0.99	12.86	0	881
RSC 1966 (log)	42,878	−1.21	0.79	−4.09	0.49
State access 1966; road (log)	42,878	−2.76	0.72	−5.06	−0.06
Internal connect. 1966; road (log)	42,878	−1.55	0.59	−4.01	0.00
State access 1966; foot (log)	42,878	−4.22	0.81	−5.94	−0.23
Internal connect. 1966; foot (log)	42,878	−1.98	0.80	−4.88	0.00
State access 1880; road (sim; log)	42,196	−2.45	0.64	−4.40	−0.11
Internal connect. 1880; road (sim; log)	42,196	−1.35	0.67	−4.04	0.00
Distance to border	42,878	103.45	106.13	0.03	590.94
Capital dummy	42,878	0.03	0.16	0	1
Median altitude	42,851	601.64	452.68	3.80	2,427.10
Median slope	42,851	3.91	1.21	1.00	9.00
Evapotranspiration	42,851	1,617.84	274.68	1,073.75	2,509.15
Precipitation	42,851	1,166.26	554.99	3.24	3,217.60
Evapotranspiration / Precipitation	42,851	4.42	1.53	1.00	8.00
Temperature	42,851	24.85	2.89	11.32	29.94
Cash crop suitability	42,851	0.36	0.15	0.00	0.80
Agricultural suitability	42,851	0.35	0.25	0.00	0.99
Distance to coast	42,878	5.83	4.08	0.0001	16.10
Distance to nav. river	42,878	1.74	2.29	0.001	20.07
Mineral deposit	42,878	0.12	0.32	0	1
Area (1000 km^2)	42,878	17.35	64.23	0.11	1,311.14
Population (1000s)	42,878	463.54	1,846.02	0.003	37,581.51
Urban population (1000s)	42,878	178.03	1,159.41	0.00	30,564.21

H.2 ROBUSTNESS CHECKS

We apply an extensive set of robustness checks to our main analysis. The following pages give an overview over the motivations, implementation, and results of each additional test. Due to space restrictions and to facilitate interpretation, most robustness checks are presented as coefficient plots, in particular and unless otherwise noted, in Figure H.1 below. Detailed reports will be available with the replication data.

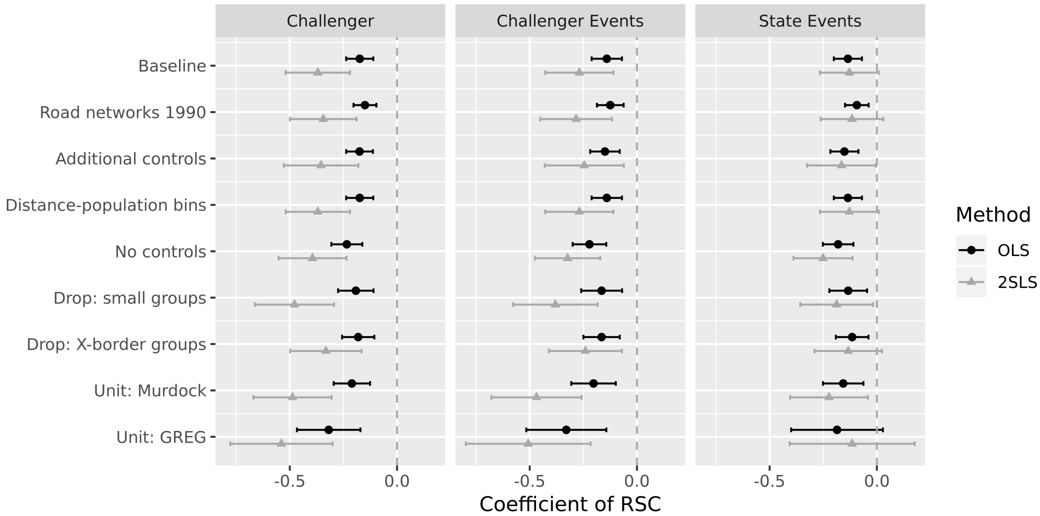


Figure H.1: Coefficients of RSC in robustness checks. Bars indicate 95% confidence intervals.

H.2.1 *Alternative dependent variables*

We heed the advice of Hegre and Sambanis (2006) and subject our main model to (1) alternative specifications of our outcomes of interest and (2) additional outcomes from various data sets on political violence. Figure H.2 plots the results for using (1) linear models of the probability that our main outcomes take a value > 0 and (2) the logged number of fatalities of the event types as the dependent variable. The respective results are consistent with those that are based on pure event counts. In addition, the Figure shows results when repeating our analysis with alternative outcome data from the (3) ACLED (Raleigh et al., 2010), (4) UCDP GED (Sundberg and Melander, 2013), and (5) SCAD data sets (Salehyan et al., 2012). The results show that low levels of relative state capacity are robustly associated with higher event counts across almost all categories of political violence.

Two classes of exceptions exist. First, the results show no evidence of an impact of RSC on remote violence, i.e. aerial bombings, as measured by ACLED. This is not too surprising, since these events are seldom and have little to do with physical accessibility. Also, We do not find effects of RSC on the number of violent incidents committed by pro-government militias taken from the SCAD data. These oftentimes happen in capitals, where RSC is by definition high. The second class of exceptions consist in that, in our instrumental variable approach, we find no evidence of an effect of RSC on (1) fatalities of state-challenger battles and (2) UCDP GED civil war events. Both exceptions relate to the generally more mixed results of the effect of RSC on fighting between challengers and state forces discussed in Section 8.5 of the main chapter.

Notwithstanding these deviating patterns, the results from analyzing the effect of RSC on alternative measures of violent events in ethnic groups highlight that challenger-related violence in the periphery of a state also brings along other forms of political violence, most importantly violence against civilians.

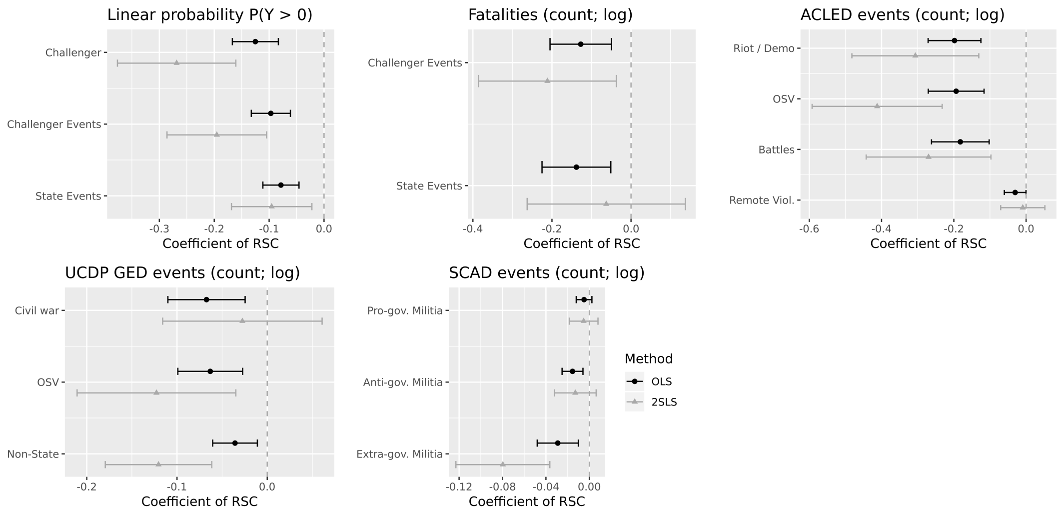


Figure H.2: Effect of RSC on alternative outcome variables. Bars indicate 95% confidence intervals.

H.2.2 *Alternative functional forms*

Although linear count models have the advantage of allowing for a very flexible specification of fixed effects, the bias introduced by the miss-specification of the distribution of the dependent variables might drive our results. Figure H.3 therefore

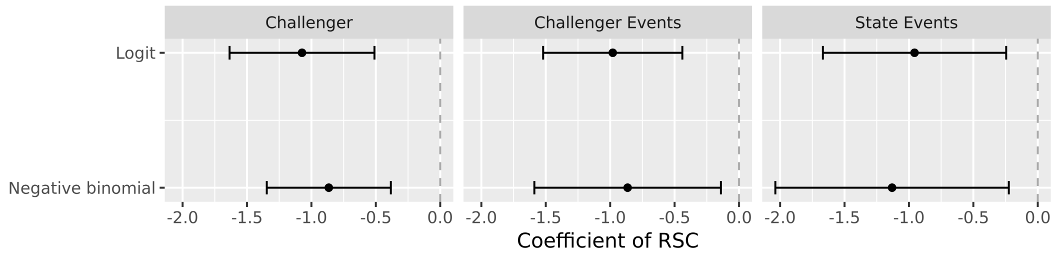


Figure H.3: Coefficients of RSC when choosing different functional forms. Bars indicate 95% confidence intervals.

presents results based on logistic and negative binomial models that use the same vector of explanatory variables as our main model.[1] Due to computational complications in estimating such models with a large number of country-year dummies, we control for country- and year-fixed effects rather than country-year fixed effects. Because our independent variables are cross-sectional in nature, this limitation has negligible effects. The results mirror the ones presented in the main paper, indicating that the latter are not due to the choice of linear count models.

H.2.3 *Measuring RSC on 1990 road networks*

In order to gauge whether the reliance on 1966 road network data significantly affects the results, we re-estimate the baseline specifications of the naive OLS and the instrumental variable analysis using data on the Michelin road network observed in 1990. For the IV-specification, this implies that we simulate a new set of road networks for each country using the road budget observed within a country's borders in 1990, and then using the simulation to instrument for RSC as measured on the 1990 road networks. The results of the resulting models are plotted in the second row of Figure H.1 and show no significant deviation from the baseline estimates, except for the IV-estimate of the effect of RSC on the number of battles between the state and its challengers which slightly decreases in precision ($p = .11$) but comes with a point estimate that is equivalent to the baseline estimate.

1 We do not estimate Poisson models here because of the significant overdispersion of the dependent variables.

H.2.4 *Adding and dropping control variables*

To account for potential omitted variable bias, we include a set of additional control variables. These are:

- *Precolonial characteristics:* Data from Murdock (Murdock, 1959, 1967) to account for precolonial characteristics of ethnic groups that might affect the extent and structure of colonial road building as well as contemporary conflict risk. In particular, we control for precolonial (1) economic practices – ethnic groups' dependence of hunting, fishing, animal husbandry, and agriculture –, (2) intensity of agriculture, and (3) political centralization.

- *Landcover:* Since landcover might affect conflict risk and the extend of transport infrastructure, we control for the percentage of ethnic groups' settlement areas that covered by (1) pasture land that is used for grazing, (2) savanna, (3) tropical woodlands, and (4) tropical forests (FAO, 2015). With average values over 10 percent, these are the most prevalent land-cover types in Africa.

- *Characteristics of the area between an ethnic group and the capital:* Lastly, the quality of the road connection between an ethnic group and the capital is likely correlated with characteristics of the area between an ethnic group and the capital. Such characteristics might also conflict risk. For this reason, we compute the geographical area that lies between ethnic groups and their countries' capitals, recompute all baseline controls for these areas,[2] and add these new variables as controls.

The third row of Figure H.1 shows that including these three vectors of control variables does not affect the baseline results.

In addition to the effect of potential omitted variables, the results reported above might be driven by non-linear effects of (1) pure geographic distances, in particular $\mathsf{state\ access}_g^{foot}$ and $\mathsf{internal\ connectedness}_g^{foot}$, and/or (2) population counts. To control for this caveat, we create yearly bins of ethnic groups that have similar values on these three dimensions. Within each country-year, we divide each of the three variables

2 I.e, these areas' distance to the coast, their local climate (mean temperature, precipitation, evapotranspiration, and the ratio of the latter two), and their average altitude and roughness. We also include their contemporaneous (urban) population (both logged), as well as the logged size of their area.

into bins of approximately 25 observations, which when all combined and interacted with make a total of 10’019 unique bins populated by observed ethnic groups. We then add one fixed effect per distance-population-country-year-bin to our baseline specification. Econometrically, this method is akin to matching observations from the same country-year on these three variables. The results of the re-estimated models are presented in the fourth row of Figure H.1. The coefficients for RSC are similar those at the baseline and statistically significant. This further suggests that the negative effect of relational state capacity on conflict risk is not due to country-specific, nonlinear effects of (the combination of) pure geographic distances and population sizes.

Lastly, the fifth row of Figure H.1 reports the results of estimating the main specifications without any control variables, except for foot-travel times to capitals and within ethnic groups. The results are not driven by the inclusion of the various controls. If at all, the specification shows that the inclusion of controls decreases the estimated effect of RSC, in particular its effect on the number of battles between state forces and challengers, which is now highly significant, both in the naive OLS and the IV-specification.

H.2.5 *Restricting the sample*

Ethnologue lists (1) many very small ethnic groups, and (2) groups that are not strictly nested in (changing) country-borders. The first caveat may lead to internal connectedness measures that are hardly influenced by any roads. The second caveat produces small ‘rump’ groups where ethnic settlement patterns are cut by country borders, leaving potentially insignificant parts of an ethnic group on one side of a border. Such ethnic groups might inherently feature different types of conflict risks (Michalopoulos and Papaioannou, 2016) which might bias our results.

To address both caveats, we restrict the sample of ethnic groups in two ways. The first is to drop ethnic groups with an area that is below the 25$^{\text{th}}$ percentile of the distribution of ethnic groups’ areas (1295 km^2). The second robustness check drops ethnic groups that are divided by a state border. The results (Figure H.1) of both tests are consistent with our main results. All coefficients are statistically indistinguishable from the baseline.

H.2.6 *Alternative units of analysis*

Next, we reevaluate our hypotheses using two alternative data sets of ethnic settlement patterns, namely GREG (Weidmann, Rød and Cederman, 2010) and Murdock's (Murdock, 1959) ethnic map. GREG is based on the Soviet Atlas Narodov Mira, and Murdock's Atlas is coded on the basis of ethnographic evidence available in the 1950s and has been digitized by Nunn and Wantchekon (2011). Ethnic groups encoded in both data sets are on average bigger than those of the Ethnoglogue data. This might reduce potential bias introduced by very small ethnic groups (see also above, Subsection H.2.5). The ensuing results, plotted in Figure H.1, are mostly equivalent to the baseline results. As noted in the main chapter, this is with the exception of the IV-estimate of the effect of RSC on the number of state-challenger battles which does not decrease in magnitude but is estimated to be insignificantly different from zero. The respective naive OLS estimate is statistically significant at $p < .1$.

H.2.7 *Country-level jackknife*

To assess the effect single countries have on our estimates, we implement a country-by-country jackknife approach, where we iteratively delete observations from each country from our sample. The estimates for the coefficient of relational state capacity RSC are plotted in Figure H.4. No single country affects the statistical significance of the effect of RSC on the number of challengers and fighting between them. Not surprisingly however, we do see lower associations of RSC with our conflict-related outcomes when we drop countries such as the DR Congo, Ethiopia, Algeria, or Kenya. In particular the first two countries host peripheral ethnic groups which are prime examples of relatively weak relational state capacity leading to violent competition over local power. With regard to the third main outcome, battles between state forces and armed groups, Figure H.4 shows that the baseline estimate of the IV specification are mainly driven by patterns of relational state capacity and conflict in the DR Congo. Although the country, in particular its Eastern part, hosts many proto-typical ethnic groups with low levels of relational state capacity and high levels of conflict, its influence on the results casts doubt on its representatives of conflict dynamics elsewhere on the continent. We discuss the implications of this finding in Section 8.5 of the main chapter.

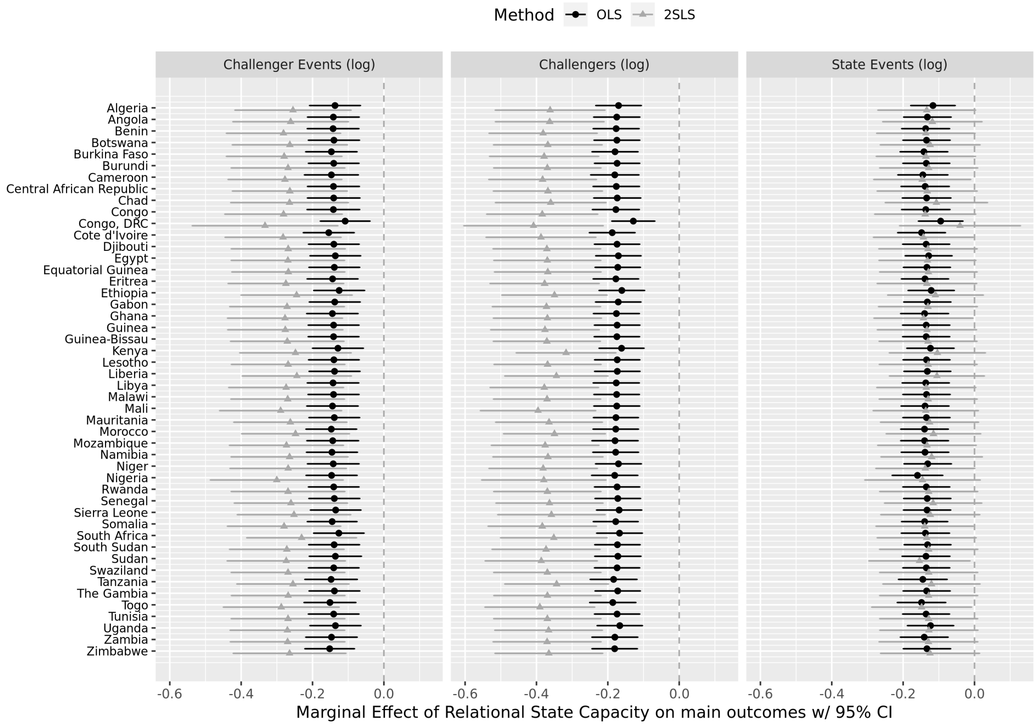


Figure H.4: Country-by-country jackknife applied to main models (Table 8.2). Bars indicate 95% confidence intervals.

H.2.8 *Cross-sectional design*

To reflect border changes in Africa after 1997,[3] we have so far analyzed panel data. Because such border changes might bias our results, we turn towards a cross-sectional analysis. The first cross-section is based on state borders and capitals observed in 1997 (the start of ACLED). The second cross-section chooses countries' borders and capitals locations at the time of their independence.[4] The dependent variables consist of the logged sums of our baseline outcomes between 1997 and 2016. Using the cross-sectional data, we estimate our main models with country-fixed effects, and base data for the population controls on the year 1966 (1990) for the first (second) cross-section.

3 Mainly the secession of South Sudan in 2011, but, where the SCAD and UCDP GED data is used, also the independence of Eritrea and Namibia.

4 Dropping ethnic groups from Eritrea, South Sudan, and Namibia from the sample, and replacing them with the groups we would have observed had these countries not become independent from Ethiopia, Sudan, and South Africa.

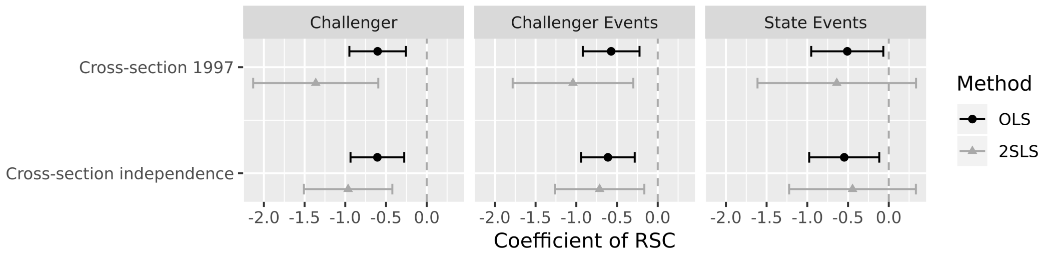


Figure H.5: Coefficients of RSC in the cross-sectional analysis. Bars indicate 95% confidence intervals.

The results of these analyses are presented in Figure H.5. They show that the insights gained from our baseline models hold irrespective of the choice of a panel- or cross-sectional design. Only the IV-estimate of the effect of RSC on violence between the state and challengers turns statistically insignificant, but is associated with the a point-estimate that is indistinguishable from the one derived from the respective naive OLS-estimate.[5] In sum, these patterns suggest that it is unlikely that the main results are due to endogenous, postcolonial changes of borders and capital locations.

H.2.9 *Alternative mechanisms*

Our theoretical argument is that *RSC* affects conflict risk through social control. However, our empirical measure of RSC, being based on travel times, might affect conflict risks through different mechanisms. In the following, we test two such alternatives.

First, roads will increase market access and thus foster local development (e.g. Donaldson and Hornbeck, 2016), which in turn increases the odds of peace. To control for this alternative causal mechanism, we control for (1) ethnic groups' nightlight emissions as a proxy for local development (Henderson, Storeygard and Weil, 2012),[6] as well as (2) the total, quality-weighted road mileage present in an ethnic groups settlement area in 1966.[7] The latter controls for the historical level of economic development. After controlling for the length of the local road network, the RSC-

5 Note however, that the size of coefficients is smaller than at the baseline, once they are standardized by the means of the dependent variables.

6 We include the logged value of average nightlight emissions in a settlement area and a dummy for whether any light is emitted in a certain group-year. Data on nightlights is available from 1992 to 2015 from National Geophysical Data Center (2014). We substitute the missing 2016 values for data from 2015.

7 This measure is simply the sum of all road kilometers multiplied by the average speed attainable on them. See above in Appendix Section B.3.

Table H.2: Alternative Mechanisms, OLS: Alternative mechanisms

	Dependent variable (logged)		
	Challengers	Challenger Events	State Events
	(1)	(2)	(3)
RSC 1966 (log)	-0.149^{***}	-0.115^{***}	-0.125^{***}
	(0.032)	(0.036)	(0.033)
State access 1966; foot (log)	0.020	-0.012	-0.016
	(0.027)	(0.033)	(0.028)
Internal connectedness 1966; foot (log)	-0.112^{***}	-0.095^{***}	-0.100^{***}
	(0.029)	(0.032)	(0.029)
External connectedness (log)	0.009	0.026	-0.011
	(0.056)	(0.061)	(0.063)
External connectedness; foot (log)	0.065	0.048	0.121
	(0.068)	(0.079)	(0.081)
Roads (km x quality)	-0.002	-0.004^{**}	-0.002
	(0.002)	(0.002)	(0.002)
Nightlights (log)	0.043^{***}	0.047^{***}	0.025^{***}
	(0.006)	(0.007)	(0.006)
Nightlights >0	-0.176^{***}	-0.208^{***}	-0.087^{***}
	(0.031)	(0.040)	(0.031)
Country-year FE:	yes	yes	yes
Controls:	yes	yes	yes
Mean DV	0.21	0.17	0.15
F-Stat:	24.1	21.42	16.49
Observations	31,740	31,740	31,740
Adjusted R^2	0.408	0.379	0.316

Notes: OLS models. Control variables consist of the total and urban population (log), groups' area (log), the mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of a group's settlement area, its cash crop and agricultural suitability, a mineral deposit dummy, as well as groups' logged distance to the coast, navigable river, and border. Two-way clustered standard errors in parentheses (ethnic group and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01.

measure picks up effects only due to the structure but not the local length of the road network. It therefore is a conservative test.

Second, ethnic groups that are well connected to the capital might also be well connected to entirety of the country's population. Such physical integration might promote socio-economic integration and increased trans-ethnic interactions, both of which may foster peace. To avoid picking up effects that are due to the general level of connectedness between an ethnic group and its country, we include a measure for the extent of 'external connectedness' of an ethnic group.[8]

Using the additional covariates to control for these two alternative causal mechanisms, we re-estimate the baseline models (Table H.2).[9] Of the additional variables, only our proxy for local development is consistently associated with challengers to state power and conflict. In particular, local nightlight emissions have a negative association with conflict at the extensive, and a positive one at the intensive margin. External connectedness has no discernible effect on conflict patterns. Importantly however, the estimate of the effect of relational state capacity remains almost unchanged from the baseline.

H.3 INSTRUMENTAL VARIABLE APPROACH: SIMULATING ROAD NETWORKS

To simulate realistic, yet simplified road networks based only on exogenous information, we assume that the road-builder aims to minimize the following objective function:

$$LOSS = \frac{1}{I^2} * \sum_{j=0}^{I} \sum_{i=0}^{I} time_{j,i}, \tag{H.1}$$

where $i, j \in I$ are the inhabitants of the territory on which roads are built. They are separated by a distance of travel time $time_{i,j}$. The road builder seeks to minimize

8 Specifically and similar to the computation of the measure *internal.connectedness* (Equation 8.2), we compute

$$external.connectedness_g = \left(\frac{1}{I_g * K_g} * \sum_{k=0}^{K_g} \sum_{i=0}^{I_g} time_{k,i} \right)^{-1},$$

where $i \in I_g$ denotes the inhabitants of the settlement area of group g that live at a distance of travel time $time_{k,i}$ from their compatriots $k \in K_g$ that live in all other ethnic settlement areas. To differentiate effects of pure geography from the effects of the road network, we again compute the measure $\text{external connectedness}_g^{foot}$ on the foot-path network and include it as a covariate.

9 We do not implement this robustness check for the IV-analysis because the contemporary data (population and nightlights) would introduce post-treatment bias.

the average travel time between any two inhabitants of the territory.[10] Population data are obtained from Goldewijk, Beusen and Janssen (2010). Since we want to avoid using data on population distributions that are affected by modern transport infrastructure, we use estimates of the African population distribution in 1880.[11] Road building is constrained by the road budget:

$$B_q = \sum_{k=q}^{Q} length_k^{observed}, \tag{H.2}$$

which consists of different qualities $q \in Q$ of roads, each of which corresponds to the observed Michelin road types (see B.3). For each road type, the road-builder receives the road mileage of this type of road and all superior types of roads observed in the Michelin map of 1966.

Roads are 'built' on a pre-determined network of footpaths.[12] Given computational constraints in the repeated computation of the loss function (Eq. H.1), we adjust the resolution of our baseline network to countries' size (see Table H.3). As detailed by the following description of our road-building algorithm, the road-builder builds one type of road after the other, by upgrading existing roads sequentially. The budget constraint ensures that the total road mileage per type of road on the simulated network corresponds to the one in the observed network.

Table H.3: Mean of input values to road network simulation

Resolution	Countries	Example	Population	Vertices	B_1	B_2	B_3	B_4	B_5
0.083	11	Togo	352,680.9	450	1.5	1.5	1.0	0.8	0.2
0.167	33	Nigeria	3,306,608.0	1,547.2	11.6	11.6	8.2	7.4	2.9
0.25	22	DR Congo	3,827,680.0	1,984.9	21.5	21.5	15.9	14.6	7.7

Networks' resolution is measured in decimal degrees and road budgets B_q in 1000 kilometers.

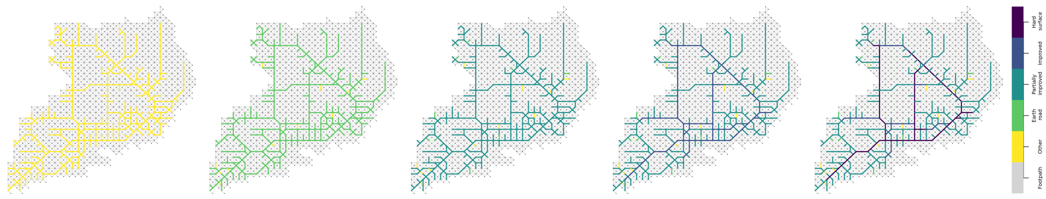
ALGORITHM:

1. Round; $q = 1$

10 Note that Burgess et al. (2015) add a distance penalty to each pair of citizens to the loss function for their simulation of 'optimal' road investment. In our IV-setup, this approach would introduce an additional parameter (distance) and further complicate the control for omitted variables.

11 Note however that this estimate is based on a combination of current disaggregated population data and historical census data which is extrapolated to the past.

12 These footpath-networks are of the same kind as the one used to transport the Michelin maps into networks. In particular, the networks consist of (1) vertices distributed in a grid-like manner in space, and (2) 8-connected 'foot-path'-edges.



(a) $q = 1$ (b) $q = 2$ (c) $q = 3$ (d) $q = 4$ (e) $q = 5$

Figure H.6: Simulation of the Ugandan road network in 1966, using population data from 1880.

a) Draw 10 seed edges, upgrade to q

b) Select neighboring edges $q_e < q$ of all edges with quality $q_e = q$, evaluate and keep 10 most promising edges as E_p.

c) Upgrade edge $e \in E_p$ that minimizes $LOSS$. Select neighboring edges $q_e < q$ of e and add to E_p. Update $B_q = B_q - length_e$.

d) Repeat step (c), and, in every 10^{th} round, step (b), until budget B_q is spent.

2.-5. Round; $q \in [2, 3, 4, 5]$

a) Select all edges with quality $q_e = q - 1$, evaluate and keep 10 most promising edges as E_p.

b) Upgrade edge $e \in E_p$ that minimizes $LOSS$. Select neighboring edges $q_e = q - 1$ of e and add to E_p.

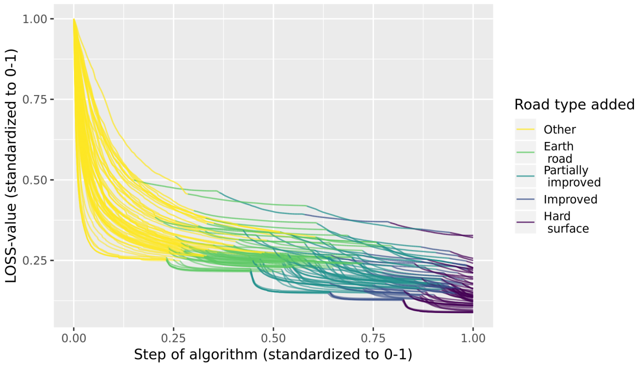
c) Repeat step (b), and, in every 10^{th} round, step (a), until budget B_q is spent.

d) Proceed to the next higher road quality and start again at (a).

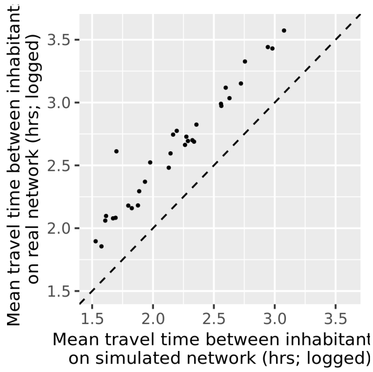
The algorithm generates realistic road networks (Figure H.6) for all country-periods in Africa since independence. To speed up computation, we make use of a 90 CPU high performance computing cluster. While small countries can be simulated in minutes, bigger countries require up to 48 hours of run time, simply because each time we recalculate the loss value we have to compute up to 3415^2 shortest paths.[13]

Over subsequent iterations of edge-by-edge road building, the within-country connectedness increases and the LOSS improves (Figure H.7a), but with decreasing

13 We make use of a path updating algorithm with $\mathcal{O}(|V^2|)$ efficiency.



(a) Improvement of loss during application of road-building algorithm



(b) Comparison of loss on observed and optimized networks (both with population data for 1960).

Figure H.7

marginal returns due to substantive scale effects of roads. Figure H.7b compares the LOSS values achieved by the simulated networks and observed networks. The simulated networks are consistently better in connecting countries' populations.

H.4 INSTRUMENTAL VARIABLE APPROACH: ROBUSTNESS CHECKS

In addition to the sensitivity analyses presented above, this section discusses robustness checks tailored specifically to the IV-approach.

H.4.1 *First stage by size of country and ethnic group*

Because we vary the resolution of the road simulations depending on countries' size (see Table H.3), our first stage results are driven by intermediate and large countries. In contrast, small countries do not feature enough variation in our instruments. Splitting up the sample along the three levels of resolution used for simulating road networks and estimating the first stage separately illustrates this pattern (Table H.4). Our two instruments possess no significant explanatory power in the 11 very small countries (think of Burundi or Lesotho) in our sample. However, in the remaining bulk of countries, our instruments behave as expected.

In a similar vein, it might be argued that only very large ethnic groups drive the first stage. We therefore split the sample along the quartiles of the total geographic area of

Table H.4: First stage estimation across resolutions of optimized networks

	Dependent variable: RSC 1990 (log)		
	(1)	(2)	(3)
State access 1880 (sim; log)	−0.039	0.479***	0.914***
	(0.365)	(0.070)	(0.109)
Internal connectedness 1880 (sim; log)	−0.241	−0.276***	−0.258***
	(0.179)	(0.040)	(0.047)
Resolution (dec. degrees):	0.083	0.167	0.25
Country-year FE:	yes	yes	yes
Controls:	yes	yes	yes
Mean DV	-0.68	-1	-1.38
F-Stat:	84.23	329.3	375.02
Observations	1,740	19,045	10,495
Adjusted R^2	0.919	0.897	0.902

Notes: OLS models. Control variables consist of the total and urban population (log), groups' area (log), the mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of a group's settlement area, its cash crop and agricultural suitability, a mineral deposit dummy, as well as groups' logged distance to the coast, navigable river, and border. Two-way clustered standard errors in parentheses (ethnic group and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01.

Table H.5: First stage estimation across geographic size of ethnic groups

	Dependent variable: RSC 1990 (log)			
	(1)	(2)	(3)	(4)
State access 1880 (sim; log)	0.539***	0.530***	0.771***	0.925***
	(0.107)	(0.097)	(0.116)	(0.121)
Internal connectedness 1880 (sim; log)	0.019	−0.186***	−0.463***	−0.367***
	(0.072)	(0.051)	(0.066)	(0.074)
Group area quartile:	1	2	3	4
Country-year FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV	-1.58	-1.29	-0.98	-0.59
F-Stat:	48.82	79.05	98.48	95.11
Observations	7,485	7,910	7,950	7,935
Adjusted R^2	0.814	0.885	0.901	0.916

Notes: OLS models. Control variables consist of the total and urban population (log), groups' area (log), the mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of a group's settlement area, its cash crop and agricultural suitability, a mineral deposit dummy, as well as groups' logged distance to the coast, navigable river, and border. Two-way clustered standard errors in parentheses (ethnic group and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01.

ethnic groups and re-estimate the first stage regression. Table H.5 shows that the first stage results are not driven only by large ethnic groups. Indeed, although the coefficient size of our first instrument, state access^{sim} increases with group size, its precision does not. For the smallest quartile of ethnic groups, where we expect least variation in the internal connectedness, our second instrument, internal connectedness^{sim} does not have explanatory leverage over RSC. However, it does have such power for the three remaining quartiles. These results show that our first stage estimation is not driven by the biggest ethnic groups although, as one would expect, the first stage is strongest there.

A finial finding worth discussing is that across country and group sizes, the coefficient for the state access instrument is larger than that of the internal connectedness instrument. This difference is due to the manner in which we construct our road graph, in which the observed road networks are superimposed on the 8-connected 'foot-path' network. This superposition implies that to travel on the road network, each traveler has to first 'walk' the next road network vertex. This walk inflates within-group travel times proportionally more than travel towards capitals. Because this extra 'walk' is not necessary on the simulated networks, the coefficient for the internal connectedness instrument is pushed towards zero.

H.4.2 *Disaggregated relational state capacity*

To test whether it is indeed the combination of ethnic groups' internal connectedness and state access, coined RSC, that drives the results, we conduct a robustness check where we instrument for both constituents of RSC separately. Table H.6 shows that both instruments are valid for the respective endogenous variable. The instrumented measures of state access and internal connectedness are consistently related to the number of challengers and violence among them. In these two models, we can also not reject the null that the coefficients of state access and internal connectedness are of equal absolute size. This supports the use of our aggregated measure of RSC. Mirroring the results of the reduced form estimates reported in the main chapter in Table 8.3, the results show that the number of battles between state forces and armed group decreases with state access to ethnic groups ($p = .057$), but does not increase with their internal connectedness. The coefficient of internal connectedness is half the size of that of state access, and associated with a large standard error. See Section 8.5 in the main chapter for further discussion of the implications of this pattern.

Table H.6: Effect of the components of RSC, 2SLS

	Dependent variable (logged)				
	Stage 1		Stage 2		
	State access	Internal connectedness	Challengers	Challenger events	State
	(1)	(2)	(3)	(4)	(5)
State access 1880 (sim; log)	0.665***	0.020			
	(0.051)	(0.060)			
Int. connect. 1880 (sim; log)	−0.077***	0.185***			
	(0.024)	(0.033)			
β_3: State access 1990; (log)			−0.348***	−0.246***	−0.137*
			(0.084)	(0.088)	(0.072)
β_4: Int. connect. 1990; (log)			0.517***	0.428***	0.065
			(0.154)	(0.159)	(0.143)
State access 1880; foot (log)	0.273***	0.012	0.151***	0.074	0.007
	(0.032)	(0.038)	(0.056)	(0.061)	(0.050)
Int. connect. 1880; foot (log)	0.053***	0.262***	−0.171***	−0.132**	−0.012
	(0.017)	(0.031)	(0.064)	(0.066)	(0.058)
$\beta_3 + \beta_4$			0.17	0.18	-0.07
			(0.16)	(0.16)	(0.14)
Country-year FE:	yes	yes	yes	yes	yes
Controls:	yes	yes	yes	yes	yes
Mean DV	-2.6	-1.49	0.21	0.17	0.15
F-Stat:	445.97	120.14	20.56	19	15.46
F-Stat Stage 1 (state):			185.93	185.93	185.93
F-Stat Stage 1 (internal):			39.81	39.81	39.81
Observations	31,280	31,280	31,280	31,280	31,280
Adjusted R^2	0.932	0.786	0.352	0.343	0.307

Notes: 2SLS-IV models. Control variables consist of the total population in 1880 (log), groups' area (log), the mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, and the mean altitude and slope of a group's settlement area, its cash crop and agricultural suitability, a mineral deposit dummy, as well as groups' logged distance to the coast, navigable river, and border. Two-way clustered standard errors in parentheses (ethnic group and country-year clusters). Significance codes: *p<0.1; **p<0.05; ***p<0.01.

I

APPENDIX: COUNTERFACTUAL BORDERS AND CAPITALS

This appendix presents technical details and additional results that complement the discussion of the simulation of counterfactual African borders in the excursus in Chapter 9. I begin by explaining the simulation procedure in Section I.1. Section I.3 provides the results of the analysis of differences in development and conflict outcomes that would result in the simulated world, and Section I.2 analyzes whether it is indeed true that the simulated improvements in state reach primarily occur in areas that today are less developed and peaceful.

I.1 SIMULATION ALGORITHM

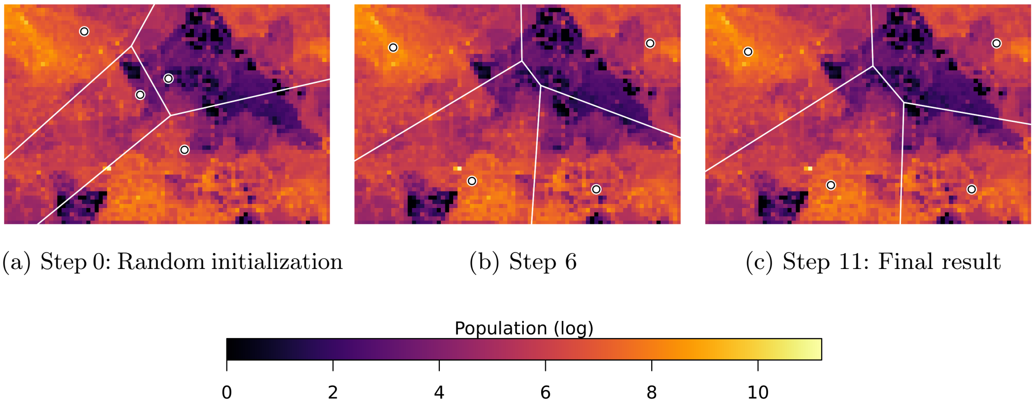
To find the optimal set of spatial units $\mathbf{U} = (U_1, ..., U_k)$ each with a capital C_u, I apply a variant of the k-means algorithm (Lloyd, 1982) to raster cells weighted with their population estimate for 1880 (from Goldewijk, Beusen and Janssen, 2010).

The simulation maximizes states' reach through an objective function that aims to minimize the average distance between states capitals and their citizens:

$$\arg\min_{\mathbf{U}} \sum_{i=1}^{k} \sum_{x \in X_i} w_x * d(C_i, x) \tag{I.1}$$

where each unit U_i is associated with a distance between its capital C_i and the raster cells $x \in X_i$ which it governs, each of which is associated with a population weight w_x. With this goal in mind, the simulation algorithm proceeds as follows:

1. Randomly place a set of 48 capitals, corresponding in number to the number of states observed today.
2. Generate the optimal tesselation around these capitals, so that the centroid of every raster cell is attributed to the closest capital. This is equivalent to a Voronoi tesselation.



(a) Step 0: Random initialization (b) Step 6 (c) Step 11: Final result

Figure I.1: Visualization of the algorithm simulating four units.

3. Find the optimal capitals for each unit of the current tesselation: The optimal capital of a unit is that point that minimizes the weighted sum of the capital's distance to all cells in the unit.

→ Repeat steps (2) and (3) until the tesselation and set of capitals does not change anymore.

As can be seen from the above formulation, each step of the algorithm is deterministic and strictly shortens average distances between the population and the simulated capitals. Being heuristic, the algorithm's drawback is that it can find only an approximation to the optimal solution. Finding the global optimum is an NP-hard problem that the k-means algorithm cannot guarantee to solve. Because the random initialization of the clustering algorithm in step (1) leads to varying results over subsequent runs, I simulate 100 sets of counterfactual captials and borders.

As explained in the main text of Chapter 9, I use the simulated geographies to calculate the geodesic distance from every point in space to its simulated capital. To arrive at *travel times* to capitals, the main measure of state reach used throughout the dissertation, I transform these geodesic distances using the non-parametric relationship between geodesic distances and travel times to national capitals observed in the real data. Figure I.2 plots this relationship in two flavors, once population-weighted, and once without weights. Since the entire simulation exercise focuses on per-capita outcomes, I chose the population-weighted version, noting that replacing it with the non-weighted one does not make much of a difference.

Figure I.2: Non-parametric LOESS-estimate of the relationship between geodesic distances and road travel times to observed national capitals. The population-weighted version is used for the transformation of the geodesic distances to simulated capitals.

I.2 COUNTERFACTUAL DEVELOPMENT AND CONFLICT OUTCOMES

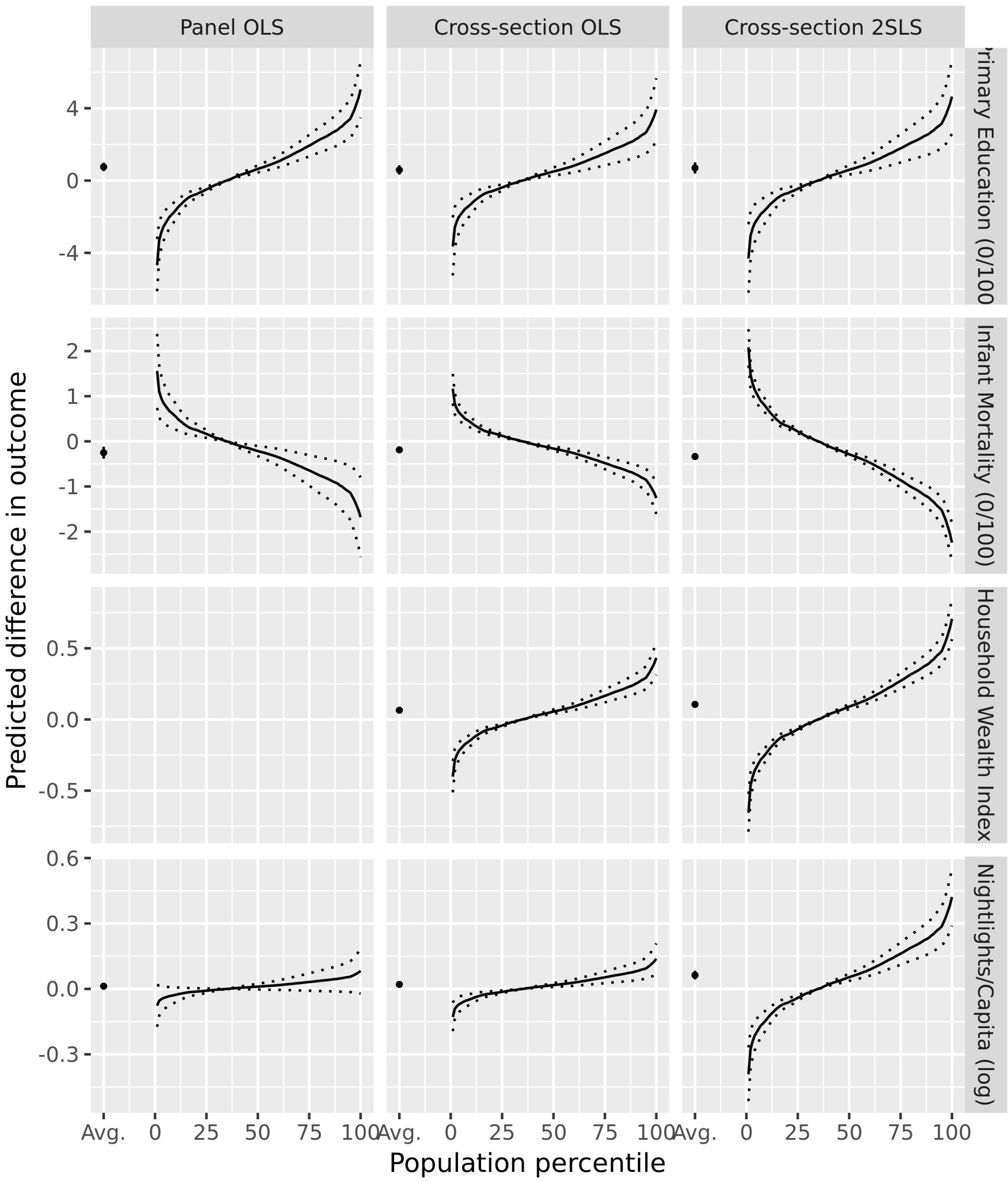


Figure I.3: Counterfactual changes of developmental outcomes, by population quantile, and varying specifications of the analysis of the effect of travel times to capitals and local development.

Note: Calculated on the basis of the results from the panel (Table 7.5), the cross-sectional (Table 7.5), and the cross-sectional IV analyses (Table 7.5) in Chapter 7.

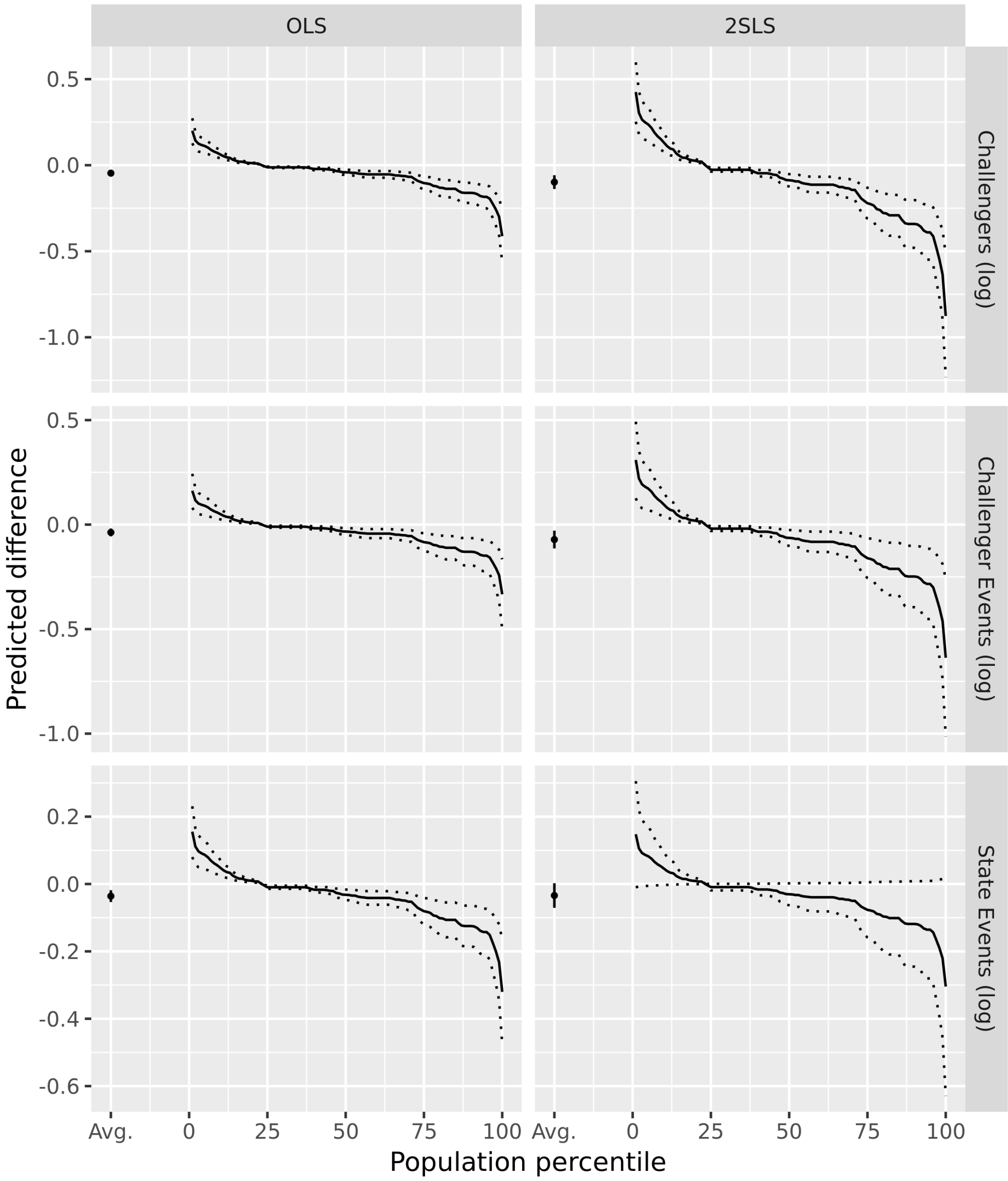


Figure I.4: Counterfactual changes in conflict incidence in ethnic groups, by population quantile, and varying specifications of the analysis of the effect of relational state capacity (RSC) and conflict in ethnic groups.
Note: Based on the results from the simple OLS (Table 8.2) and instrumental variables analysis (Table 8.4) in Chapter 8.

I.3 CORRELATION OF SIMULATED IMPROVEMENTS WITH OBSERVED OUTCOMES

Beyond being able to improve national geographies over those created by the colonizers, Herbst's (2000) hypothesis requires that the simulated improvements correlate with low levels of development and more conflict today. If, instead, the regions that would have been better off in the simulated world are actually more developed or peaceful than the average continent, the claim that suboptimal colonial borders caused low levels of local development by creating difficult geographies would be difficult to uphold.[1]

Based on the data from Chapters 7 and 8, Tables I.1 and I.2 show that improvements in state reach resulting from the simulations exhibit the expected correlation with local development outcomes and conflict in ethnic groups. Simulated 'improvements' of the status quo produce negative differences between simulated and observed travel times to national capitals. These correlate strongly with currently low levels of education and infant survival rates, less household wealth, and fewer nightlight emissions per capita. Similarly, regions with large simulated improvements have experienced substantially more violence involving militias and rebel groups since 1997. In combination with the previous results, these correlational patterns support Herbst's arguments. Both suggest that some of the poorest and most conflict-ridden areas of the continent would have been better off if colonization had resulted in better-placed national borders and capitals.

1 Note that these borders would still cause aggregate levels of development to be lower than under the simulated scenarios.

Table I.1: Correlation between difference in simulated and observed travel times to national capitals and local development

	Primary educ. (0/100)	Infant mort. (0/100)	Wealth Idx.	Light/capita (log)
	(1)	(2)	(3)	(4)
$\log(1+\text{time}_{sim}) - \log(1+\text{time}_{obs})$	3.918^{***}	-1.010^{***}	0.250^{***}	0.042^{**}
	(0.381)	(0.082)	(0.028)	(0.017)
Country-year FE:	yes	yes	yes	yes
Survey FE:	yes	yes	yes	yes
Controls:	yes	yes	yes	yes
Mean DV:	67	9.9	-0.026	-6.5
Observations	1,864,537	2,372,066	726,082	1,384,627
Adjusted R^2	0.358	0.039	0.348	0.256

Notes: OLS models. Control variables consist of the local population count (log), local cash crop suitability, mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, and the mean altitude and slope of an area, as well as the local agricultural suitability score and a mineral deposit dummy. In addition, I add a locations' logged distance to the coast, border, and closest navigable river. Precolonial ethnic attributes are captured through their political centralization, dependence on agriculture and husbandry, and the intensity of their agricultural activities. Control variables for models of primary education consist of responents' age and age squared, as well as a female dummy. Where infant mortality is the dependent variable, models include an infant's mother's age at birth and its square, the birthorder and its square, as well as a female and twin dummy. In models of househols' wealth, I control for the size and the number of children (both linear and squared). Two-way clustered standard errors in parentheses (point and country-year clusters). Significance codes: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table I.2: Correlation between difference in simulated and observed travel times to national capitals and conflict outcomes

	Dependent variable (logged)		
	Challengers	Challenger Events	State Events
	(1)	(2)	(3)
$\log(1+\text{time}_{sim}) - \log(1+\text{time}_{obs})$	-0.084^{***}	-0.100^{***}	-0.088^{***}
	(0.018)	(0.021)	(0.019)
Country-year FE:	yes	yes	yes
Controls:	yes	yes	yes
Mean DV	0.21	0.17	0.15
F-Stat:	22.41	20.1	15.71
Observations	31,445	31,445	31,445
Adjusted R^2	0.395	0.369	0.310

Notes: OLS models. Control variables consist of the total and urban population (log), groups' area (log), the mean annual temperature, precipitation, evaporation, the ratio of precipitation and evaporation, the mean altitude and slope of a group, its cash crop and agricultural suitability, a mineral deposit dummy, as well as groups' logged distance to the coast, navigable river, and border. Two-way clustered standard errors in parentheses (ethnic group and country-year clusters). Significance codes: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

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ACADEMIC POSITIONS

ETH Zurich, Switzerland — July 2019–
Postdoctoral Researcher, International Conflict Research Group

Harvard University, Cambridge MA, USA — February 2020– August 2021
Postdoctoral Visiting Researcher, Government Department

EDUCATION

ETH Zurich, Switzerland — Apr. 2015 – May 2019
PhD in Political Science
State Building, Development and Conflict in 20th Century Africa
Committee: Lars-Erik Cederman (main supervisor), Simon Hug, Horacio Larreguy Arbesú

Harvard University, Cambridge MA, USA — Feb. 2017 – Dec. 2017
Predoctoral Visiting Fellow

ETH Zurich and University of Zurich, Switzerland — Mar. 2015
MA in Comparative and International Studies (MACIS)

University of St. Gallen, Switzerland — July 2012
BA in Economics & BA in International Affairs

Universidad de los Andes, Bogotá, Colombia — Aug. 2011 – Feb. 2012
Exchange semester

PUBLICATIONS AND PAPERS

Peer Reviewed

Müller-Crepon, Carl and Philipp Hunziker. 2018. *New Spatial Data on Ethnicity: Introducing SIDE*. Journal of Peace Research, 55(5). *JPR Best Visualization Award 2018.*

Dissertation Papers

Carl Müller-Crepon. *Continuity or Change: (In)direct rule in British and French Colonial Africa.* Revise & resubmit International Organization.

Carl Müller-Crepon. *Roads to Extraction: Natural Resources and the Physical Foundations of State Capacity in Africa.*

Carl Müller-Crepon. *State Reach and Development in Africa, 1965–2015.*

Carl Müller-Crepon. *Indirect Colonial Rule and the Provision of Public Services.*

Hunziker, Philipp, Carl Müller-Crepon, and Lars-Erik Cederman. *Roads to Rule, Roads to Rebellion: Relational State Capacity and Conflict in Africa*. Under Review.

Other Working Papers

Carl Müller-Crepon. *Sowing Hatred: Local Ethno-Political Competition and Pre-Election Violence in Majoritarian Elections.* Under Review.

Beiser, Janina, Carl Müller-Crepon, and Yannick Pengl. *Who Benefits? How Local Ethnic Demography Shapes Political Favoritism in Africa.* Revise & resubmit British Journal of Political Science.

Carl Müller-Crepon, Yannick Pengl, and Nils-Christian Bormann. *Matching Ethnic Groups in Africa.*

AWARDS, GRANTS AND FELLOWSHIPS

Doc.CH Fellowship Mar. 2016 – Sep. 2019
PhD scholarship of the Swiss National Science Foundation (CHF 208'301)

Doc.Mobility Fellowship Feb. 2017 – Dec. 2017
Mobility scholarship of the Swiss National Science Foundation (CHF 12'760)

ETH Medal Dec. 2015
Awarded for MA thesis entitled *Ethno-Political Competition and Pre-Election Violence in Africa*.

Scholarship of the German National Academic Foundation Feb. 2010 – Mar. 2015

ETH Zurich Master Scholarship Sep. 2013 – Mar. 2015

Carlo-Schmid Fellowship Sep. 2012 – Feb. 2013
Fellowship for work at the United Nation's Department for Economic and Social Affairs

Arthur Rich Award Mai 2013
Awarded for excellent BA thesis on ethical issues at the University of St. Gallen

TEACHING

Political Economy of Development and Conflict in Africa Fall 2019
ETH Zurich Master of Comparaitve and International Studies elective (with Yannick Pengl)

Ethnic conflict from a political science perspective Summer 2017
Undergraduate summer school course, German National Academic Foundation (with Yannick Pengl)

Teaching Assistant Quantitative Methods Fall 2015
Taught by Liam McGrath and Jørgen Bølstad, MA Comparative and International Studies ETH Zurich

Teaching Assistant Introduction to Political Economy Fall 2014, 2015
Taught by Prof. Katharina Michaelowa, BA Political Science University of Zurich

WORK EXPERIENCE

Research Assistant Mar. 2014 – Apr. 2015
With Prof. Lars-Erik Cederman and Prof. Katharina Michaelowa

Kiel Institute for the World Economy Mar. 2013 – July 2013
Research-internship on migration and development

UN DESA Sep. 2012 – Feb. 2013
Carlo-Schmid Fellow at the United Nations Department for Economic and Social Affairs, Office for Economic and Social Council (ECOSOC) Support and Coordination, Policy Coordination Branch

Coordination Régionale des Artisans de Ségou, Mali Aug. 2008 – July 2009
Voluntary service through the *weltwärts*-program of the German Development Service

INVITED TALKS

2019 Graduate Institute Geneva

CONFERENCE PRESENTATIONS

APSA Annual Meeting	2017, 2018, 2019
MPSA Annual Conference	2017
ISA Annual Convention	2016
European Network of Conflict Research	2015, 2016

TECHNICAL SKILLS

Statistical Software	R, Stata
Computer Languages	Python
Databases	PostgreSQL, PostGIS
Tools	SVN/Git, ArcGIS, QGIS