Identification Politics: Information, Distribution, and the State in Sub-Saharan Africa

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Identification Politics: Information, Distribution, and the State in Sub-Saharan Africa

A dissertation presented

by

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Abstract

A distinguished literature in political science conceives of states as seeking to impose uniform control throughout their territories but being constrained by the high resource costs of doing so. Accordingly, the inability of many developing countries to evenly administer policy is often construed to be a problem of limited state capacity. This dissertation considers the reverse: how even nominally universal state-building schemes can exacerbate inequalities in the state's coverage. Because such efforts have stratified effects, they induce distributive conflicts which undermine states' incentives to expand their reach. As a result, the challenge is less resource constraints than a set of perverse and self-reinforcing political incentives which restrict the state's uniform expansion.

Substantively, I focus on the development of states' informational capacities through citizen identification and registration schemes. First, drawing on evidence from Tanzania, I consider why we observe such enduring economic inequalities in citizens' legibility to the state. Leveraging a targeted policy reform in the early post-independence period, novel causal estimates highlight the selective incentives faced by wealthier citizens to comply with the state's informational demands. Second, drawing on evidence from Ghana, I study the challenge of disrupting this truncated status quo. Exploiting a discontinuity in the spatial assignment of identity registries, I demonstrate how the more even incidence of the state's capacities risks exposing it to demands it struggles to meet. Third, in the context of modern Uganda, I study how citizens interpret signals of the state's expanding reach. Utilizing the fortuitous timing of a social survey administered during its intensive biometric identification

rollout, I show how complex signals of capacity can raise citizens' expectations of future service provision in ways which prompt disappointment and disillusionment ex post.

Together, these country cases and historical moments underscore how efforts to develop the state's informational capacities have distributive consequences for *who* the state covers and *how*. These consequences jar both with policy narratives regarding such modernizing initiatives and a broader academic literature on state-building that is often quiet on questions of distribution.

Contents

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1 | Introduction

In his manifesto decrying the intrusions of the nineteenth century French state, the anarchist philosopher Pierre-Joseph Proudhon declared that "To be governed is to be noted, registered, enumerated, accounted for, stamped, measured, classified, audited, [...] in every operation, every transaction, every movement." If being governed is equivalent to these epistemological acts—of the state seeing, and learning about, its citizens—then a substantial portion of the world's population remains ungoverned. More than a billion people lack any state-issued documentation of their identity; half a billion of whom are in sub-Saharan Africa. Scholars in public policy have gone so far as to call this the "single most critical failure of development over the past 30 years" (Horton 2007) representing a "scandal of invisibility, which renders most of the world's poor as unseen, uncountable, and hence uncounted" (Setel et al. 2007).

This failure is surprising for two reasons. First, what states know about their citizens is integral to core functions of governance. Minimalist definitions of state capacity center on the ability of the state to implement policies. Accordingly, the state's stock of information about its citizens is at the core of our conceptions of state capacity (Brambor et al. 2020). In his seminal analysis of *legibility*,[1] Scott (1998) argues that "An illegible society is a hindrance to any effective intervention by the state, whether the purpose of that intervention is plunder or public welfare" (p.78). Recent evidence underscores how what the state knows about its

[1]Scott's notion of *legibility* is slightly broader than what more recent authors refer to as informational capacity. Legibility encompasses a broad set of activities which seek to simplify societal complexities into simple abstractions in order to facilitate centralized governance. The sense in which I use 'informational capacity' is closer to Brambor et al. (2020) and Berwick and Christia (2018), defined as the stock of information possessed by state institutions about their populations.

citizens affects its ability to extract taxation (Lee and Zhang 2017; Stasavage 2020), deliver public goods (D'Arcy and Nistotskaya 2017; Muralidharan, Niehaus, and Sukhtankar 2016), and aggregate the preferences of its citizens (Piccolino 2016).

Second, classic accounts of the state are premised on the notion that state actors seek control over society (Giddens 1986; Mann 1984). In Foucault (1991)'s classic work, for example, the state is constituted by an all-encompassing need to monitor and regulate its citizens, while Migdal (1988) describes the persistent efforts of weak states to penetrate localized societal structures. In the world of Proudhon and Scott, the state does not seek to extract information from its citizens for the instrumental reasons of taxation and service delivery, but because states *intrinsically* seek control. Further, the technologies by which states develop the capacities necessary for exerting this control are well-known: technologies of enumeration and registration have long histories both in now-developed countries (Caplan and Torpey 2001) as well as in developing countries (Breckenridge and Szreter 2012). Work dating back to the modernization theorists posited that the gradual expansion of such technologies would build citizens and make nations (Smelser and Lipset 1966).

Together, the demonstrated *benefits* of the state's informational capacities for governance; and the posited *incentives* of political leaders to seek control over society through developing this capacity, should lead us to question why we observe such enduringly, and stubbornly, low levels of capacity—both informational and otherwise—across many developing countries.

Prior literature points to two sets of explanations for why so many states seemingly know so little about their citizens. First, classic studies of state development point to the constraints imposed by the high material costs of expanding the state apparatus. Since state-building is expensive, it requires strong motivating forces—most famously, the external threat of war (Gennaioli and Voth 2015; Tilly 1985). In the absence of these motivating forces, or in the presence of internal conflicts (Centeno 2002), state actors face prohibitively high administrative costs of expanding control across their territory fully. In Herbst (2000)'s classic analysis, for example, the low population density of many African states renders the

cost of building a state apparatus prohibitively high. Second, citizens might actively oppose the development of a stronger state for fear of its despotic tendencies (Mann 1984). In the context of Southeast Asia, for example, Scott (2009) illustrates how peasants defy the efforts of the state to render them governable, while Migdal (1988) shows how local societal structures are able to push back against the intrusions of the central state. Particularly when mistrust towards institutions has deep historical roots (Breckenridge 2014; Nunn and Wantchekon 2011), then, states' capacities might remain weak because citizens' demand is limited.

1.1 Dissertation overview

This dissertation considers an alternative, largely overlooked, challenge—neither the administrative cost of expansion nor the subterfuge of citizens, but rather a set of political incentives facing state-building actors which inhibit their "supply-side" incentives to invest in the uniform expansion of their reach. The problem is less that citizens seek to avoid being seen by the state, but rather that the state faces only selective incentives to see them. To advance this claim, I suggest that fully understanding the enduringly limited *levels* of the state's capacities requires better understanding its *incidence* across societal groups. Empirically, throughout the dissertation I leverage research designs which enable me to hold fixed, or at least to benchmark, explanatory variables relating to resource costs or citizens' resistance.

As this dissertation demonstrates, even seemingly administrative functions of the state can generate strikingly uneven effects on patterns of access, exclusion, and extraction. Because these effects are stratified across groups, state-led investments in capacity then create (either absolute or relative) winners and losers. Identifying the relative losers from state-building efforts depends on the status quo of distribution: applied to schemes which expand the state's informational capacities, efforts to begin to solicit information from citizens, from a low baseline, are likely to have distinct distributive implications from efforts which

broaden, from a higher baseline, what the state knows about its citizens. Because of the stratified implications of capacity, plus the importance of citizens' compliance for furthering the state's objectives, the extent to which states are sensitive to the *losers* of a stronger state conditions the strength of its incentives to invest in broadening its reach. As I discuss below, these incentives are structured by forces relating to competition and coercion.

More broadly, I suggest that state-building processes should be conceptualized as distributive challenges of resource allocation rather than challenges of enhancing the efficiency of the state's functioning. Because state-building efforts are rarely Pareto-improving, the conflicts that result from a state investing in its capacity impose constraints on their effective expansion.

I focus on one core technology constituting the state's informational capacities, that of citizen registration and identification schemes, and demonstrate how these technologies facilitate unmediated state-citizen flows: both enabling the state to extract taxation from its citizens as well as enabling citizens to make claims on public resources. When a state has only limited ability to coerce its citizens, citizens electing to comply with the state's informational demands to register then trade off these expected benefits (access) against its potential costs (extraction). The extent to which citizens incur benefits from registering with the state depends on the state's bureaucratic ability to regulate access to public resources on the basis of registration status. Put in other words, whether obtaining documentation through identity registration schemes benefits citizens' access to resources is contingent on the state's ability and incentives to exclude citizens *lacking* such documentation.

From a status quo in which the state knows very little about its population, wealthy citizens stand to lose more than the poor from the downstream consequences of supplying information to the state through registration schemes. Precisely because expanding the state's informational capacities facilitates its extraction of taxation, the state seeks to enroll the rich but faces the challenge of inducing their compliance through the provision of sufficient compensatory benefits. As I show in Chapter 2, the narrow targeting of the benefits of registration generates variation in access to resources of particular relevance for them. This

narrow targeting is undergirded by the state's inability to generate broad-based demand—for example, it is more bureaucratically taxing to regulate access to primary schools on the basis of registration status than secondary schools. The extent to which these net benefits are *credible* depends ultimately on the extent of political competition: when competition is high, the rich fear the redistribution of their resources to the poor and so are less willing to comply; when competition is low, the state more willingly excludes citizens from making claims on resources absent documentation and hence the returns to compliance are higher.

Structural conditions of limited coercion and weak political competition therefore imply that efforts to initially expand such schemes can only succeed when they have economically *regressive* effects on state-citizen flows. These regressive effects create a bifurcation: while the wealthy enjoy increased access to public resources in exchange for their increased exposure to taxation, the poor—even ignoring their higher material costs of being seen by the state—remain governed only indirectly and make claims on public resources via mediated brokerage.

While economic inequalities in registration, access, and taxation are widely observed across developing countries, the initial dynamics of building the state's informational capacities entail that inequalities in registration can *propagate* other inequalities in state-citizen interaction. This dynamic connection between state capacity and economic inequality resembles the first half of Kuznets (1955)'s famous conjecture: that, from a low baseline, increases in economic development go hand in hand with increases in inequality.

I then consider whether such truncated, bifurcated systems of distribution might constitute a stable equilibrium rather than a transitory midpoint on the road to modernization (Ferguson 1999). This regressive status quo is incentive-compatible from the state's perspective, and hence stable, to the extent that the state (1) faces sufficiently weak political incentives to more efficiently deliver services, and redistribute resources towards, the poor; (2) is sufficiently unable to broadly coerce its population either to comply with its demands for registration or taxation.

Beginning from this status quo, I consider how shifts along either of these dimensions—

competition or coercion—induce efforts to register citizens to have countervailing effects on governance outcomes which dampen states' incentives to do so. In Chapter 3, I consider how structural changes in political competition, holding fixed the state's limited ability to coerce, imply that efforts to expand the state's informational capacities have *redistributive* effects. I show how, starting from a regressive status quo, democratic political incentives might motivate the state to uses its informational capacities in order to administer policy directly to poorer citizens. By building linkages directly between state and citizen, such efforts additionally undermine the authority and influence of local intermediaries otherwise responsible for brokering access and exclusion. But, because broadening the state's informational coverage renders citizens' access to public resources more progressive, it threatens the privileged access previously enjoyed by relatively wealthier citizens. As a result, threatened by the redistributive signal of a stronger state, wealthier citizens seek—and because of the state's limited ability to coerce, can—withdraw their political and fiscal support. Ex ante incentives to invest in the expansion of the state's informational capacities, therefore, are conditioned by the state's sensitivity to the distributive conflicts it engenders.

In Chapter 4, I consider how structural changes in the state's ability to coercively register citizens affects citizens' attitudes, expectations, and allegiances. I explore how state-led interventions which, ex post, have highly extractive consequences for policy implementation can, ex ante, send ambiguous and highly uncertain signals of future service provision to citizens. As a result, common conceptions of citizens seeking to hide from an intrusive state rest importantly on the assumption that citizens' construe such attempts as intrusive at all. But, because the ex post consequences of state-building efforts for policy implementation are often divorced from citizens' ex ante expectations, over time this prompts disappointment and disillusionment with the realized capacity of the state to fulfil its promises of modernized service provision.[2] As a result, the extent to which states are sensitive to the potential disappointment of its citizens, likely determined by the extent of political competition,

[2]As I note in the chapter, to an extent this resembles the Tocquevillian logic of modernizing reforms provoking political contestation (Healy, Kosec, and Mo 2017; Huntington 1968).

shape the strength of its incentives to invest in expanding its informational capacities.

Chapters 3 and 4 then demonstrate how breaking out of truncated, regressive systems of state-citizen distribution risk either inducing distributive conflict or citizens' disappointment and disillusionment. As a result, evidence on the *aggregate* benefits of investments in the state's informational capacities tends to mask the distributive political and economic effects of these shifts. These distributive effects, implied largely by the temporal sequencing of how states build, impose important constraints on the state's ability to do so effectively. Together, it suggests that state development is more a supply-side challenge undermined by the conflicting *incentives* facing political leaders than a demand-side challenge of citizens evading the state's grasp.

To draw out the political challenges created by the distributive implications of state-building, I draw on a related set of historical moments and country case studies in sub-Saharan Africa. I consider a set of three countries—Tanzania, Ghana, and Uganda—which share a common colonial history and inherited similar initial conditions upon independence with regard to their limited informational capacities (Kuczynski 1948).[3] As argued by Cooper (2002), the absence of colonial efforts to solicit information from their subjects meant that "Far from an oppressive and ubiquitous will to know, African states were built in an informational void." Each country inherited little systematic information about their populations and a citizenry often acutely aware of the potentially extractive consequences of registering with the state (Breckenridge and Szreter 2012; Ittmann, Cordell, and Maddox 2010). The cases vary along two dimensions: first, the extent of the state's coercive capabilities; second, the extent of political competition.[4]

In Chapter 2, I study the efforts of the early post-independence state in Tanzania to induce the registration of its citizens. As a case where the state struggled to induce broad-based demand and limited competition inhibited the threat of redistribution, I demonstrate

[3]See Stasavage (2020) for a recent discussion of the importance of initial conditions in explaining subsequent patterns of state development over time.

[4]Importantly, this variation is *across-case* rather than *within-case*, which remains an objective for future work.

how state-building efforts to register citizens either failed entirely or, when they had any effect, regressively shifted the coverage of the state by restricting access to narrow-based public resources. Empirically, I demonstrate how quasi-random variation in citizens' registration status provides narrow, but substantially large, benefits, and show how citizens' decisions whether to comply in the first place are conditioned by the local incidence of benefits they expect to accrue.

In Chapter 3, I study how patterns of access and compliance are affected by variation in the state's informational capacities in Ghana, a setting characterized as weakly coercive but politically competitive. Leveraging a historical population-based discontinuity used to assign identity registries across localities, I show how—as the state begins to use its informational capacities to administer resources towards the poor—the compliance of the rich with demands for taxation is crowded out.

In Chapter 4, I study how citizens update in response to a potentially ambiguous signal of state capacity in Uganda, a setting characterized as noncompetitive but relatively coercive. Facilitated by a technological shock to the ease of controlling access to resources, in the form of novel biometric registers, I show how citizens update strongly about the benefits of future economic welfare but only weakly about the costs of a stronger, more extractive state. Ex post, as these costs are realized more than the benefits, citizens' optimism turns to disillusionment.

Across the cases, I draw out how variation in what the state knows about its citizens affects *who* the state covers and *how* and underscore the political challenges posed by the distributive consequences of state-building processes. These consequences jar both with policy narratives regarding such modernizing initiatives and a broader academic literature on state-building that is often quiet on questions of distribution.

1.2 Relationship to prior literature

In studying the distributive implications of state-building efforts, this dissertation makes two important analytical decisions worth discussing for the benefits they present and the limitations they impose. First, I depart from an expansive literature positing the centrality of ethnic cleavages, more than economic cleavages, as structuring political competition and resource distribution across African states (Posner 2005). Some part of the reason for my focus on the *economically* stratified effects of capacity surely owes to case selection—Tanzania, for example, is a setting where ethnicity is perhaps unusually non-salient (Miguel 2004). More broadly, however, there are theoretical reasons to pay increasing attention to the politics of economic inequality across sub-Saharan Africa. For one, a number of recent studies suggest that the ethnic basis of distribution is often reducible to latent economic class-based forms of competition, such that what scholars often *characterize* as purely ethnic competition is predicated on the existence of overlapping intergroup economic differences (Alesina, Michalopoulos, and Papaioannou 2016; Baldwin and Huber 2010; Boone and Simson 2019; Huber and Suryanarayan 2016).[5]

That these cleavages empirically overlap is demonstrated in Chapter 2, where I document at the cross-national level a strong correlation (particularly so in sub-Saharan Africa) between patterns of access and exclusion on the basis of ethnic versus economic status. Related work underscores how class-based, versus identity-based, forms of competition and distribution are tightly linked to the capacity of the state: as the state's ability to extract taxation increases, citizens' allegiances to economic categories shifts (Bates 1983; Kasara and Suryanarayan 2020). Last, recent evidence from highly ethnicized settings underscores how the predictive power of ethnic status in explaining welfare outcomes is being modified by structural changes in the distribution of population over space (Harris and Posner 2019; Nathan 2019).

[5]Studying India, a canonical case of caste-based distribution, Huber and Suryanarayan (2016) find "a strong class component to ethnic politics in India, underscoring the possibility that what scholars often view as identity politics can have an element of class politics in disguise."

Together, while my focus on the incidence of state capacity across economic, rather than ethnic, groups therefore limits its relevance for settings where economic status only weakly predicts patterns of access and exclusion, such cases are likely to be relatively rare (and increasingly so). By comparison, this project suggests an important, relatively understudied interaction between the politics of state development and economic inequality in sub-Saharan Africa.

Second, this dissertation is ultimately concerned with the (potentially countervailing) incentives facing states to expand their reach, but it generally empirically *infers* these incentives rather than observing them directly. While this somewhat consequentialist approach is often assumed in the literature, it is worth justifying. Methodologically, a focus on research designs which afford opportunities for drawing credible causal inferences perhaps naturally leads one to study the effects of specific, theoretically relevant, policy reforms. Rigorously evaluating the consequences of such reforms then provides empirical evidence of how citizens respond to the efforts of states to render them legible. Linking citizens' response functions back to the observed behavior of states is rendered particularly challenging because these responses often help to explain why states did *not* try to expand their informational capacities: what is frequently striking is *how little* states have invested in seeing their citizens. As a result, for example, Chapter 3 leverages the unintended consequence of a prior policy reform to explain persistent underinvestments in technologies to identify citizens.

Further, while a rich literature characterizes the state as a unitary actor somewhat separated from society—as Hyden (1983) famously called the African state, "suspended balloon-like in mid-air"—recent scholarship demonstrates the limits of this simplification (Wang 2021). Particularly because many of these incentives are structural rather than affecting specific parts of the government, disentangling some of the concrete channels through which they affect government policymaking remains a task in need of additional qualitative work. Further, some of the dynamic patterns proposed in the dissertation could usefully be tested using panel data at the national and subnational levels to assess the drivers of state-led behavior on the "left-hand side."

2 | Identifying the Rich: Registration, Taxation, and Access to the State in Sub-Saharan Africa[1]

2.1 Introduction

Classic accounts of the state posit that its capacity is constituted by what it knows about its population (Foucault 1991; Giddens 1986; Mann 1984; Scott 1998). As Scott (1998) writes in his seminal analysis of *legibility*, "An illegible society is a hindrance to any effective intervention by the state, whether the purpose of that intervention is plunder or public welfare" (p.78). Recent studies, accordingly, have demonstrated important variation in this informational capacity, as measured through censuses, cadasters, and registries, and validated its independent effects on the ability of states to tax and deliver public goods (Brambor et al. 2020; Christensen and Garfias 2021; D'Arcy and Nistotskaya 2017; Lee and Zhang 2017). Much less is known, however, about how such capacity is built: how do states obtain information from their citizens, and who are they able to learn about?

I argue that distributive politics conditions how the state's capacity develops. When coercion is prohibitively expensive, the state must rely on the instrumental compliance of citizens with state-building schemes (Levi 1988; Migdal 1988). Since the state's informational capacity affects its ability to tax its citizens (Kiser and Sacks 2009; Stasavage 2020),

[1]With thanks to the Kennedy Memorial Trust and Vogelheim Fund for funding. I thank Anne Degrave, Torben Iversen, Kimuli Kasara, Imke Harbers, Horacio Larreguy, Melissa Lee, Evan Lieberman, Lucy Martin, Constantine Manda, Baruani Mshale, Michael Olson, Pia Raffler, Joan Ricart-Huguet, Abhilasha Sahay, Ken Shepsle and Joe Wong for thoughtful comments, and participants at APCG Colloquium, APSA 2019, ASA 2019, Harvard, MIT Political Behavior of Development Conference 2018, MPSA 2019, NEUDC 2018, Twaweza Tanzania, and the Weatherhead Center for International Affairs for their feedback. Thanks also to representatives of the Tanzania National Archives, UNICEF Tanzania and the United Nations Archives who provided valuable assistance and insights.

therefore, building this capacity relies on structuring the parameters of *informational transactions* with citizens that induce them to comply in spite of the costs. In this paper I develop a theory of how these parameters are set, articulate its implications for the state's coverage, and take advantage of a rich empirical setting in post-independence Tanzania to provide quasi-experimental evidence in support of the argument.

I focus on the case of identity registration schemes, perhaps the most ubiquitous mechanism through which states solicit information from their citizens.[2] In complying with state demands for information, citizens evaluate its expected consequences: increased access to public resources (through formal documentation) against their potentially increased exposure to taxation and redistribution (Breckenridge and Szreter 2012; Caplan and Torpey 2001). When bureaucratic capacity is moderate and political competition is weak, such schemes have especially uneven effects. Moderate bureaucratic capacity renders the state more able to regulate access to *narrow-based* public resources on the basis of registration status than broad-based ones. Limited competition, by weakening political incentives to redistribute fiscal resources, dampens the concerns of the rich about registering with the state. Together, the benefits of compliance become non-uniform even absent variation in citizens' logistical and financial costs of enrollment. This incidence, further, is incentive-compatible to the extent that the state gains more from inducing the taxation of the rich than it does the poor.

In such cases,[3] seemingly administrative state-building schemes have particularly regressive implications: rather than universalizing access, registration technologies then undergird and *propagate* economic inequalities in state-citizen interaction. The first empirical prediction of the theoretical framework is that supplying information to the state through

[2]Survey data, for example, suggests that more than a third of all citizens in sub-Saharan Africa had attempted to obtain identity documents from the state in the year prior to enumeration (Afrobarometer, 2016).

[3]Aside from bureaucratic capacity and limited political competition, I specify that the argument rests on two additional assumptions: (1) that economic status meaningfully predicts access to public resources; (2) that the state is sufficiently incentivized to tax its population due to the absence of alternative revenue streams.

registration should facilitate access to narrow-based public resources of particular value to the rich, in exchange for increasing exposure to taxation. The second is that citizens' decisions whether to register in the first place ought to be conditioned by the relative balance of these returns. Registration, however, is not randomly assigned and so any analysis of its effects must account for major confounders, such as rurality, income, and education, which also independently affect access to the state. Consequently, no causally identified evidence appears to exist on its consequences in any context.[4]

I study civil registration in Tanzania in the state-building period following independence in 1961, which offers an appropriate but nontrivial case. The state's moderate bureaucratic capacity rendered it selectively capable of regulating access to public resources but, in spite of limited political competition, the regime's socialist orientation posed a strong redistributive threat to wealthier citizens supplying information to the state. Descriptive evidence underscores the strong link between registration and taxation during this period (Due 1963; Lee 1965), the extent to which citizens were aware of the tradeoffs they faced in registering with the state (Harris 1965; Kjekshus 1974), and how these were especially salient for the rich due to the progressive incidence of taxation (Huang 1976).

To evaluate the theory's empirical implications, I exploit a set of legal reforms that sought to induce registration in a set of districts in the mid 1960s.[5] Using rich historical and administrative data sources, in a difference-in-differences setup I first compare individuals in cohorts born shortly after, versus before, the reform in these districts relative to a set of control districts where the reform was not extended. Then, leveraging exposure to the policy reform as an instrument for individuals' registration status—and therefore possession of identity documents—I provide causally identified evidence on its consequences for access

[4]Prior work looks at the correlates of functioning registration systems at the national-level (Phillips et al. 2015). Those authors claim that "difficulties in identification of valid measurement instruments rule out strong analytic designs for causal inference, such as quasi-experimental approaches or instrumental variables" (p.1389).

[5]I focus on civil registration, through which individuals obtain birth certificates: even if other forms of identity documents exist, the possession of a birth certificate is generally viewed as the most important single document facilitating access to the state (AbouZahr et al. 2015).

to public resources and exposure to taxation.

Consistent with the theoretical framework, the results point to effects on access to resources of particular relevance for the wealthy, such as higher education and state employment. In turn, registration increases direct tax payment by citizens. There is no evidence of effects on access to broad-based public goods, such as primary education. These results point to a relatively narrow set of resources where registration affects access. For the compliers induced to be registered by the reforms, the magnitude of the estimates suggests that registration creates substantial variation in access to these resources. Then, to demonstrate that the relative balance of these costs and benefits conditions citizens' decisions, I examine the determinants of compliance with the reform and establish three results. First, fewer citizens complied with the reform in localities where overall levels of tax collection were higher, *or* where the rich faced particularly high tax rates relative to the poor. Second, the local presence of narrow-based public goods (secondary schools) positively predicts compliance much more so than the presence of broad-based public goods (primary schools). Third, consistent with citizens evaluating these returns, I show that individuals induced to register were differentially those born into the elite. I demonstrate the robustness of the results across specifications, datasets, and estimation strategies.

In so doing, this paper speaks to two literatures. First, it relates to work on the informational dimensions of state capacity. As a recent literature shows, the state's informational capacity lies at the core of common conceptions of state strength (Berwick and Christia 2018; Hanson and Sigman 2021; Lindvall and Teorell 2016; Soifer 2013), and its aggregate variation affects a wide range of societal outcomes (Brambor et al. 2020; D'Arcy and Nistotskaya 2017; Lee and Zhang 2017). If this capacity has such wide-ranging *effects*, then, it is an important—but mostly overlooked—question to ask how it is *built*.[6] This paper suggests that the development of this capacity, rather than being a purely administrative challenge,

[6]Some recent work considers the endogenous determinants of investments in capacity more broadly. For example, Sánchez-Talanquer (2020) points to the strategic decision of elites to register their land; Christensen and Garfias (2021) and Gottlieb (2019) point to the electoral costs of implementing capacity-building interventions; and Garfias and Sellars (2021) points to the ways in which centralizing investments are disincentivized by their effects on weakening local elites.

is deeply political because it has distributive implications. As a result, the striking variation that exists in states' abilities to see their citizens represents a *political* outcome which implies that the benefits of increases in informational capacity are unlikely to be evenly distributed across societal groups.

Second, it relates to work on the distributive consequences of state development. Prior work on the politics of civil registration has studied how the expansion of individually-targeted social welfare programs has driven registration among previously excluded groups (Harbers 2020; Hunter and Brill 2016; Lund 2008). In contexts where welfare systems are heavily 'truncated,' however, citizen compliance with state demands often regressively benefits the rich at the expense of the poor (Bastagli 2009; Ferguson 1999). The results here show how the constraints facing weakly institutionalized states, combined with variation in compliance decisions of citizens, may generate a regressive distribution of returns even to schemes which might appear nominally progressive. This is substantively important because a billion people, half of them in sub-Saharan Africa, lack proof of legal identity in spite of sustained policy efforts (Gelb and Metz 2018; World Bank 2018b). Understanding the returns to, and determinants of, registering with the state is a necessary component in evaluating these persistent failures to generate broad-based coverage.

2.2 Registration and the state

The informational capacity of the state is typically tied to the core challenge of revenue generation: states need information about their populations to tax them (Lee and Zhang 2017; Scott 1998; Stasavage 2020). Beyond taxation, theoretical models show how reductions in the extent of asymmetric information about citizens ameliorate a wide array of governance dysfunctions (Banerjee 1997; Ting 2017).[7] Perhaps the most common method for states to collect information about their citizens is through identification and registra-

[7]With respect to specific policy domains, Slater (2008) points to the role of electoral administration in driving the mass registration of citizens in Southeast Asia, while Hunter and Brill (2016) show how the spread of redistributive programmes has driven progress towards universal birth registration, and Doyle (2006) shows how fears of demographic collapse drove civil registration in Uganda.

tion schemes (Breckenridge and Szreter 2012; Caplan and Torpey 2001).[8] I conceptualize citizens' enrollment in such schemes as an *informational transaction*: citizens provide information about themselves, or their child, to the state, and in exchange the state provides them with documentary evidence of their identity. Registration, therefore, reflects an instrumental decision rather than a coercive imposition (Cohn and Dirks 1988). I discuss the parameters structuring this decision before articulating their implications for the state's informational coverage.

2.2.1 Citizens' decisions to supply information to the state

Citizens weigh the costs and benefits of registering with the state before deciding whether to enroll. These costs are both direct and indirect. Direct costs represent the short-run financial and time costs of registration, which are often significant in countries with large rural populations (Makannah 1985). Further, even when registration carries low *de jure* financial costs, high informal costs necessitated by bribery and brokerage impose particular burdens on the poor (Gupta 2012; Kruks-Wisner 2018).

The indirect costs of registration comprise the often-uncertain downstream consequences of becoming *legible* to the state (Scott 1998). Following Scott, recent studies validate the strong aggregate link between citizens' supply of information to the state and their exposure to taxation (Brambor et al. 2020; Lee and Zhang 2017; Sánchez-Talanquer 2020). Several potential channels undergird this connection.[9] For example, the administration of centralized direct taxes often impose high informational demands on the state which are naturally fulfilled by registration systems (Fjeldstad and Therkildsen 2008; Kiser and Sacks 2009). Alternatively, because identity registration systems often undergird databases used across a range of government agencies, supplying information to the state for one purpose may not

[8] This is especially the case in settings where coercion is prohibitively expensive—take, for example, the cost and complexity of census enumerations (Soifer 2013).

[9] As Breckenridge and Szreter (2012) write of the colonial period, "identity registration was typically an instrument of nasty exactions—of taxes, rents, conscription and confession" (p.359).

preclude its future use for extraction (Longman 2001; Phillips et al. 2015).[10]

The returns to registration are defined by how compliance affects access to resources, whether in the short or the long-term. International organizations point to an wide array of sectors and services whereby access potentially depends on registration with the state, including education, labor contracts, property rights, inheritance, and healthcare (UNICEF 2013). How binding these restrictions are, in practice, is often uncertain.[11]

2.2.2 Distributive incidence of the returns to registration

The structure of these informational transactions implies that the state's capacities develop in an economically regressive, truncated fashion. This regressivity is conditioned by the extent of bureaucratic capacity and political competition together shaping citizens' expectations about the consequences of supplying information to the state. Crucially, it holds *even ignoring* the fact that, somewhat mechanically, wealthier citizens are likely to face lower direct costs of registration.

Bureaucratic capacity

Akin to gatekeeping (Cooper 2002), states shape citizens' expected instrumental benefits of registration through the targeting and enforcement of exclusionary eligibility requirements in accessing particular goods, sectors, and services.[12] The extent of the state's bureaucratic

[10]Recent examples discussed by Clark (2018), including from India, Pakistan, and Uganda, illustrate how foundational identity system data is often shared across government agencies for purposes of tax collection. In turn, historical examples underscore the ways in which registration systems can become repurposed over time for the targeting of coercion and violence by the state (Longman 2001). More indirectly, if citizens instrumentally benefit from registration schemes they may become more willing to comply with demands for taxation in return (Levi 1988; Timmons 2005).

[11]A review by DLA Piper (2016) regarding whether birth certificates are legally required to access different services reveals the ambiguity of these requirements. Across 18 African countries the overwhelming majority have ambiguous, or contradictory, legal requirements for the use of identity documents.

[12]Cooper (2002) argues that the primary exercise of state power in Africa has been through selectively withholding access to internal markets. Applied to the logic of formalizing the relationship between citizen and state, governments induce citizen enrollment by excluding access to particular public goods and services in their absence.

capacity constrains its ability to generate this demand for registration.[13] Where such capacity is negligible, states are unable to regulate access to limited public resources and hence few citizens are induced to comply (Powell 1981).[14] At high levels of capacity, states generate broad-based demand by conditioning access to a wide set of public resources on the basis of registration status.[15]

In intermediate cases, states can only selectively regulate access to public resources. Controlling access to *broad-based* areas of service delivery imposes high bureaucratic costs of enforcement which may be untenable. Imposing a restriction on access to primary education in the absence of identity documents, for example, requires significant monitoring of compliance by the bureaucrats responsible for assessing eligibility. By comparison, it is less bureaucratically taxing to regulate access to more *narrow-based* areas of service delivery as a function of registration status, which are also likely to be in localities where the state possesses preexisting bureaucratic infrastructure. Limitations in this capacity, then, imply that the instrumental benefits of registration are more easily centered on areas of service delivery relevant for a narrow set of economic elites than a broad set of citizens.

Political competition

However, wealthier citizens face more pronounced indirect costs of supplying information to the state due to its potential consequences for taxation. High levels of political competition, under conditions of moderate bureaucratic capacity, render their registration decision particularly fraught. Because political leaders then have not just some capacity to tax enrolled citizens but also incentives to redistribute fiscal resources, they cannot commit *against*

[13] A rich literature characterizes dimensions of state capacity, and definitions overlap. 'Bureaucratic capacity' aligns approximately with what Hanson and Sigman (2021) call 'administrative capacity' and what Berwick and Christia (2018) call 'compliance capacity.'

[14] Breckenridge (2014), in his account of biometric identity systems in South Africa, additionally underscores the tremendous administrative costs of identity registration systems in settings where citizens face incentives to avoid the state's extractive reach.

[15] Gelb and Metz (2018), for example, describe the striking difficulties in accessing public resources faced by unregistered citizens in developed countries.

redistributing the extracted taxes of wealthier citizens towards the poorer majority (Kasara and Suryanarayan 2015). Further, dampening the instrumental benefits of compliance, political competition might disincentivize political leaders, in pursuit of citizens' support, from enforcing policies regarding access to public resources (Gottlieb 2019; Holland 2017).[16]

2.2.3 Who complies and who benefits?

As a result, efforts to develop the state's informational reach have the most striking distributive consequences when bureaucratic capacity is moderate and political competition is weak. From the perspective of wealthier citizens, in spite of the increased exposure to taxation they expect from complying with the state's demands, weak competition limits the threat of the wholesale redistribution of their resources. In turn, the state's limited extent of bureaucratic capacity renders the expected benefits of registration more *credible* with respect to narrow-based public resources than for broad-based ones. Poorer citizens, on the other hand, accrue only diffuse and uncertain benefits from compliance even ignoring the higher direct costs they face to do so.

The theoretical framework implies that states initially seeking to develop their informational capacities only structure an informational transaction with the rich to induce their compliance. Further, from the perspective of the state, this incidence is incentive-compatible to the extent that it fiscally benefits from inducing the registration of the rich relative to the poor.

We should only expect these regressive implications in settings where economic status meaningfully predicts access to, and exclusion from, public resources. Importantly, this does not preclude its relevance for contexts where distribution is often characterized as centering on non-economic divides. While a large literature is premised upon the the *ethnic* basis of distribution across many developing country settings, recent work underscores how ethnic distribution is frequently equivalent to latent forms of economic class-based distribu-

[16]To the extent that high levels of competition imply frequent political turnover, in highly stratified settings this might amplify citizens' uncertainty regarding the longer-term consequences of formalizing their identity with the state (Padró i Miquel 2007).

tion (Alesina, Michalopoulos, and Papaioannou 2016; Baldwin and Huber 2010; Huber and Suryanarayan 2016).[17]

Concordantly, Figure A.1, using cross-national data, demonstrates that exclusion from public resources according to ethnic status very strongly correlates with exclusion according to economic status.[18] As a result, while ethnic status can exert theoretically distinct effects on state-citizen interaction,[19] the distributive consequences of such state-building schemes should continue to hold in ethnically stratified settings to the—empirically widespread—extent that these cleavages overlap. Finally, the framework rests on the assumption that states are incentivized to tax their citizens and use information towards this end. In settings where these incentives are sufficiently absent, such as in rentier states, we should expect that such efforts have more minimal effects on determining access to public resources.

2.2.4 Implications of the theoretical framework

Cross-national evidence in Figure 2.1a documents the overall positive relationship between levels of economic development and civil registration rates. Figure 2.1b, however, illustrates how *levels* of registration mask important nonlinearities in the *distribution* of registrants. Inequality in citizens' registration status expands as states initially develop their capacities, before shrinking as the full measure of citizens becomes registered by the state.[20] While suggestively consistent with the selective benefits of these instruments inducing uneven distributions of citizens to enroll, it remains confounded. Most obviously, holding fixed its benefits and indirect costs, a similar pattern would be generated if wealthier citizens

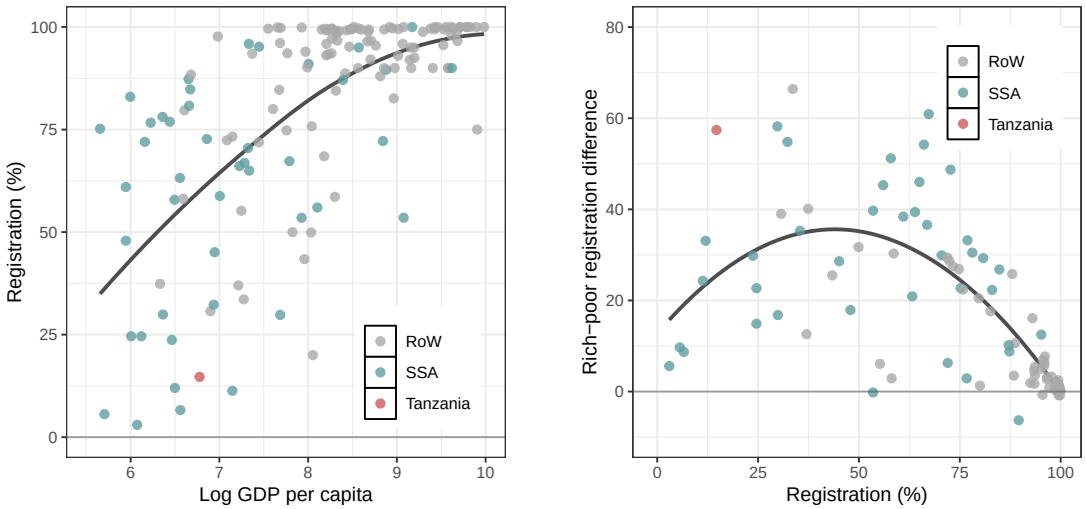
[17]Studying India, a canonical case of caste-based distribution, Huber and Suryanarayan (2016) find "a strong class component to ethnic politics in India, underscoring the possibility that what scholars often view as identity politics can have an element of class politics in disguise."

[18]Notably, across most countries in sub-Saharan Africa, exclusion by economic group is more acute than by social group.

[19]For example, ethnic status is likely to predict embeddedness in local networks useful for accessing resources, which might then dampen the incentives to register with the state (Kasara 2007). We might additionally expect that the salience of ethnic divides, by shaping their beliefs about downstream extraction, increase citizens' indirect costs of registration.

[20]Indicative of the extent of their correlation, Figure A.2 replicates this inverted U-shaped relationship with respect to either levels of economic development or state capacity on the X-axis.

Figure 2.1: Income, registration, and inequality



(a) Income and registration levels

(b) Registration levels and inequality

Sources: World Bank, UNICEF. *Registration* is the share of citizens possessing birth certificates. *Rich-poor registration difference* is the percentage point difference in the registration rate of highest income quintile citizens relative to lowest quintile.

simply faced lower direct costs of registering with the state.

Economic inequality in registration, access, and taxation is observed across a wide set of countries. The theoretical framework implies that, moderated by capacity and competition, inequality in citizens' registration is not simply ephiphenomenal to these other inequalities in state-citizen interaction, but plays a constitutive role in propagating them. There are two key empirical implications of the theoretical framework which underlie this unevenness in the state's informational coverage. First, citizens face only selective incentives to register with the state since doing so particularly facilitates access to *narrow-based* public resources while increasing exposure to taxation. Second, citizens' local decisions whether to supply information to the state through registration schemes ought to depend on the balance of the benefits, in terms of access, and costs, in terms of taxation and redistribution, of doing so.

2.3 Identifying citizens in Tanzania

The early post-independence period in Tanzania offers a useful case in which to test these broader implications. In this section I first draw out the features of the Tanzanian case salient to the scope conditions of the theoretical framework. Given these, I provide qualitative evidence on the state's efforts to register its citizens and citizens' decisions whether to comply.

2.3.1 Salient features of the Tanzanian case

First, with regard to its capacity, Figure A.3 plots the latent measure of state capacity from Hanson and Sigman (2021). This suggests that Tanzania's capacity, in the period following independence in 1961, was slightly above average within sub-Saharan Africa and slightly below the global average.[21] This classification of moderate capacity is supported by the state's partial regulation of access to public resources. With regard to education, the Tanganyika African National Union (TANU) regime was able to exert significant control over higher levels of education (Resnick 1968) while efforts to expand the provision of primary education were only weakly regulated and overseen (Court 1976). With regard to social security, the government exerted tight control over access to the formal sector, such as state jobs and pensions (Tripp 1989).

Further, scholars underscore the state's initial inability to effectively coerce its population: as Hyden (1980) notes, the TANU regime initially "relied more on exhortation and persuasion than on compulsion" (p.76) to pursue its objectives. While the state became more coercive over time, the success of state-building schemes continued to rely on inducing the instrumental compliance of citizens with the state's aims. For example, it was only due to the state's failure to induce rural citizens to relocate into organized villages that more coercive methods were attempted to resettle citizens (Barkan 1984). Other efforts to coerce

[21] This measure reflects important temporal variation in capacity, which was relatively low in the immediate post-independence period but significantly developed over the subsequent decades.

citizens—such as to formalize urban workers (Diouf and Fredericks 2014; Tripp 1989) or employ tax field units (Bienen 1970)—were strongly resisted by the population and often rendered unsuccessful.

Second, the Tanzanian case is defined by persistently low levels of political competition. Following TANU's overwhelming electoral victory in 1962, the regime introduced an element of within-party competition while banning opposition parties (Bienen 1970). This led scholars to label Tanzania a "one-party democracy" (Cliffe 1967), with limited competition at the legislative level but no electoral competition at the presidential level.[22] Importantly, in spite of this lack of meaningful political competition, the socialist orientation of the TANU regime rendered it highly redistributive from the rich to the poor in its pursuit of a rural agrarian mode of development. Huang (1976), for example, demonstrates that the incidence of taxation was highly progressive during this period.

Third, economic status has been enduringly predictive of access to public resources. Inheriting a relatively fragmented distribution of ethnic groups upon its independence, with no disproportionately powerful groups, TANU was able to implement nation-building initiatives which further reduced the salience of ethnicity as a cleavage structuring distribution (Miguel 2004). In its place, economic status became more strongly associated with distribution, with much of the country's wealth concentrated in very few districts (Tordoff 1967). Figure A.1 highlights that socio-economic cleavages are *relatively* more important for state access relative to social cleavages, but that neither of these measures are outliers either regionally or globally. Last, necessitated by its lack of natural resources and low inflows from international donors, the state had clear incentives to extract taxation from its citizens (Due 1963).

Together, these features suggest Tanzania is an appropriate, but nontrivial, case in which to examine the stratified consequences of states' efforts to register their citizens. On the one hand, its moderate bureaucratic capacity afforded it some control over access to public

[22]These low levels of political competition have persisted, with TANU and its successor party CCM remaining the dominant party since independence.

resources, and policy implementation was conditioned by citizens' willingness to comply. On the other, in spite of limited political competition, the redistributive orientation of the TANU regime posed a particular threat to economic elites.

2.3.2 Evidence on citizens' calculus to register with the state

Government efforts to register the population in Tanzania stretch back over a century, with ordinances to register non-native populations beginning in 1894 (Kuczynski 1948). Scholarly accounts make clear the persistent difficulties the state has faced in expanding registration schemes, in spite of their stated benefits—even today, under a fifth of the population possesses a birth certificate.[23] These challenges are not unique to Tanzania: as I note in Appendix A.1.1, a wide number of countries across sub-Saharan Africa exerted significant effort to register and identify their populations in the early post-independence period but were broadly unsuccessful in doing so.

A key reason for this failure, beyond the administrative challenge of reaching a highly rural population, is that citizens were acutely aware of the trade-offs they faced in registering with the state: as Kjekshus (1974) concludes, "the Tanzanian citizen responds to a series of rational calculations of benefits and sacrifices connected with registration" (p.133). The key sacrifice associated with registration during this period was the risk of taxation. The strength of the registration-taxation link inhibited birth registration in the colonial period; as Walters (2016) suggests, "officials pointed to the conflation in people's minds of the registration of birth and the collection of taxes" (p.69), while Wood (2016) notes that the same officials were often responsible for civil registration and taxation. This link persisted: as studies of the 1965 election remark, "the most important reason, by far, for low registration was the spectre of taxation" (Harris 1965). Citizens' fears were sufficiently extreme that the Vice President was forced to publicly declare that voter registration would *not* induce taxation,

[23] As one newspaper wrote, "A census is a herculean exercise that entails long-term planning and fairly high financial and manpower resources; hence its "once-after-so-many-years" feature." Therefore, registration is a "cheap, and administratively convenient means" of "ensuring that we have a ready source of reference all the time, to set our priorities right and conduct our social and economic affairs intelligently." (Daily News, 31 August, 1982).

but low registration totals suggest his promise was considered non-credible (Bienen 1970; Kjekshus 1974).

The salience of this registration-taxation link was grounded in the primacy of individually-targeted taxes inherited by the state upon independence in 1961 (Kiser and Sacks 2009). Due (1963) shows that personal poll taxes, levied upon all adults, accounted for a substantial share of direct tax revenue collection in Tanzania, while Fjeldstad and Therkildsen (2008) report that such individual poll taxes accounted for nearly all of Tanzania's local tax revenues in 1962. Undermining the administration of such taxes, Lee (1965) notes that "when a male passes his eighteenth year, and thus becomes subject to local rates, cannot readily be determined, for no vital statistics are maintained" (p.39). An inability to observe the eligibility of citizens particularly inhibited tax collection in more urban localities which contained the majority of the country's taxable wealth (Jensen and Mkama 1968).

Qualitative evidence supports that citizens weighed these indirect costs against their expected benefits of registration. Hunter (2015), for example, shows how registering and obtaining documentation "was understood to bestow particular rights and the capacity for claim making on its bearer" (p.134). These benefits, as noted by Powell (1981), were often diffuse especially for poorer citizens. Ethnographic evidence from Wood (2016) contrasts the benefits that registration with the state is *supposed* to generate for citizens—"Parents attribute many powers to birth certificates" (p.56)—with the reality that even after complying with state demands to register "birth certificates and the benefits they are supposed to provide remain largely aspirational" (p.49).

Accordingly, sources point to a relatively limited set of services where access depends on proof of registering with the state. Access to higher levels of education is claimed to strictly depend on possession of a birth certificate,[24] while certificates are reported as being necessary for national health insurance schemes but not for more general access to healthcare (UNICEF 2013). Obtaining a passport officially requires the possession of a birth certificate, as does

[24]One reason for this relates to the necessity of verifying the identity of individuals taking formal examinations (Gelb and Clark 2013).

access to more recently developed national identity cards (ITU 2015). An additional set of potential uses relates to access to economic formality. Accordingly, certificates are officially needed for applying to government jobs, for proof of citizenship for many private sector jobs and employment contracts, and for access to government pensions (Registrar General's Office, 2005). However, how binding these uses are in practice remains an empirical question.

In order to examine the regressive implications of states' efforts to develop their informational capacities through registration schemes, early post-independence Tanzania is therefore rendered a useful case by its moderate bureaucratic capacity, weakly competitive (but highly redistributive) politics, and salience of economic cleavages. In turn, qualitative evidence underscores that citizens carefully considered the trade-offs they faced in registering with the state, beyond the high direct costs they often faced, which contrasted the diffuse benefits of compliance with the concrete threat of taxation. Further, descriptive evidence is consistent with the targeting and enforcement of access to resources particularly relevant for the rich on the basis of registration status. Together this implies the two key empirical predictions to be tested: that, given its structural conditions, registering with the state generates narrow benefits in access to public resources in exchange for increased exposure for taxation; and that citizens' decisions to register are accordingly conditioned by the local balance of these benefits and costs.

2.4 Research design

In this section I describe the data sources and empirical strategy employed to test these expectations. In short, I leverage a policy reform to evaluate the *consequences* of registering with the state; then, I examine the *determinants* of registration by evaluating heterogeneity in citizens' decision to comply with the reform.

2.4.1 Data

I employ two primary sources of data. First, I use data from an extract of the 2012 Population and Housing Census, which provides data from over four million citizens. Second, I use data from the National Panel Survey (NPS). There are four rounds of NPS data in total, where the first three use the same set of respondents and the fourth uses a new sample. I employ the samples from round 2 (2010-11) and round 4 (2014-15).[25] This generates an overall sample of over 30,000 respondents. I draw outcomes from the census sample where possible, due to its much larger sample size, and only use the NPS sample as necessary. Descriptive statistics for the baseline census sample are provided in Table A.1 and for the NPS sample in Table A.2.

Dependent variables. The main outcomes for evaluating the consequences of registration relate to access to public goods and services. I focus on access to education and social security since prior scholarship on Tanzania makes clear the extent to which the central government exerted partial control over each sector (Court 1976; Resnick 1968; Tripp 1989). Using the census sample, I divide access to education into indicators for individuals having any primary education, any secondary education, and having university education. I divide access to social security into indicators for individuals having access to public health insurance, having access to a private sector pension, and having access to a government pension.

The theoretical framework requires classifying outcomes, within these sectors, based on their particular utility for the rich. To do this, for a given outcome I compute the partial correlation coefficient ρ(Income, DV) between a wealth index and the relevant dependent variable.[26] ρ is reported in all tables. Consistent with intuition and the qualitative evidence discussed above, this exercise suggests that we should expect effects of registration on access

[25]Round 1, administered in 2008-09, does not include a question on registration status, and round 3, administered in 2012-13, comprises an identical sample to round 2.

[26]More formally, I regress the standardized outcome variable onto a standardized income measure and the controls employed in Equation (2.1). For the census sample, the wealth measure comes from an index of asset ownership. For the NPS sample, the measure comes from household consumption expenditure.

to higher levels of education more than basic education, and effects on access to formal sector social security relatively more than access to redistributive health insurance funds.

Next, due to the absence of relevant variables in the census, I examine effects on tax payment drawing on the NPS sample. Using reported household expenditures, the primary outcome is an aggregate indicator for whether individuals paid any money to the government over the prior year. This is comprised of individual indicators for the payment of fees, local council rates, and taxes on income and property to the central government. Theoretically, the link between citizens' formalization and their payment of taxes is likely to be stronger for centrally-administered taxes, where states face more acute informational challenges, relative to locally-administered taxes (Kiser and Sacks 2009). Consistently, a similar exercise to compute the ρ coefficient suggests that we should expect effects on the payment of central taxes more so than on the payment of local taxes or fees.

Independent variables. In each dataset I observe whether individuals are registered, and hence possess a birth certificate, which is the primary independent variable of interest for examining its effects. Additionally, I observe the district and year of birth of each individual, which enables me to assign their exposure to the reform described below.

To investigate the determinants of registration, in line with second implication of the theoretical framework, I examine compliance with these reforms along a number of dimensions. First, I consider local variation in taxation around the time of the reform. I draw on data in Jensen and Mkama (1968), which provides district-level data on levels of taxation in the early post-independence period, as well as Lee (1965), which provides data on the relative *incidence* of taxation on the rich and poor for a subset of districts.[27] Second, I consider local variation in the presence of public goods during this time period using geolocated administrative data for primary and secondary schools which were founded prior to the mid-'60s. Third, I consider individual-level covariates including gender and the educational

[27]Lee (1965) only reports exact tax rates for around a third of all districts. To maintain reasonable coverage across the clusters, and since within-region differences in tax rates are much smaller than across-region differences, I assign these tax rates to other districts in the same region when data is provided for at least one district in a given region.

attainment of respondents' parents to proxy for familial wealth.

2.4.2 Estimation

To gain intuition for the empirical strategy that isolates the consequences of registration for access to public resources, consider the following 'naive' estimating equation:

$$y_{itd} = \beta^{OLS}\text{Registered}_{itd} + \eta_d + \mu_t + \gamma X_i + \epsilon_{itd}, \tag{2.1}$$

where y_{itd} is an outcome for individual i born in year t in district d; Registered_{itd} is an indicator for whether i is registered, and hence possesses a birth certificate; η_d are district of birth fixed effects; μ_t are year of birth fixed effects; X_i are individual-level covariates like gender,[28] and standard errors are clustered at the district of birth level. The fixed effects control for time-invariant characteristics of individuals born *within* a district, and for temporal changes that affect individuals born *across* districts equally in a given year.

For β^{OLS} to identify the causal effect of registration on outcomes, we must believe that Registered_{itd} is assigned as-if randomly conditional on the controls. This is unlikely to be the case: even within a district, the privileged might have easier access to registration *and* other public resources, or might be more easily coerced both to register *and* pay taxes. Alternatively, indicative of reverse causality, citizens already enjoying preferential access to resources might then be more willing to comply with the state's demands to register. In such cases, estimates of β^{OLS} are likely to be biased.

Compulsory birth registration reforms. To overcome this inferential challenge, I leverage variation in exposure to a set of legal reforms relating to birth registration. Under Section 27 of The Births and Deaths Registration Act, first passed in 1920, the Tanzanian government can render birth registration 'compulsory' for individuals born after a certain date in

[28] Given that citizens, in principle, might be registered by the state very close to their birth, the set of individual potential pre-treatment covariates is limited.

a given locality.[29]

This legal reform was first applied to Tanzanian citizens in 1966 to make birth registration obligatory for citizens born after a given date in a set of localities containing the "major towns" (Wood 1971). These towns equate modern districts in Tanzania and hence I refer to them as districts for consistency. The reform, publicized in newspapers throughout the year, was accompanied by changes to the price structure of registration to generate financial incentives to register births promptly in the targeted districts, and increased threats of punishment for non-compliance. Appendix A.1.1 provides additional information on the reform, while Appendix A.1.2 discusses the subsequent, largely ineffective, expansion of compulsory registration orders in the following decades.[30]

This reform, intended as a 'demand-side' shock for registration with the state, was targeted at districts which contained just 5% of the Tanzanian population (Tordoff 1967) but a far higher share of its directly taxable wealth (Jensen and Mkama 1968). The administrative councils in these areas were responsible for a greater share of their own tax revenue than more rural district councils but their ability to generate revenues significantly underperformed (Dryden 1968). Similarly targeted reforms, as I discuss in Appendix A.1.1, took place in a number of other countries in sub-Saharan Africa during this period.

Identification strategy. I leverage these reforms as a source of exogenous variation in individuals' registration status in an instrumented difference-in-differences design. The geographically selective application of the reform generates potential *spatial* variation in registration: individuals born in a 'treated' district should be more likely to be registered than those born in a 'control' district. By itself, this comparison is confounded by the general differences between treated districts and the rest of the country described above. As such,

[29]I exclude Zanzibar throughout and focus on mainland Tanzania, since Zanzibar has had an autonomous system of civil registration for over a century (Kuczynski 1948).

[30]Citizens during this initial period were not registered in hospitals, but rather had to travel to administrative centers to register. A final change, made shortly afterwards, made it extremely onerous to obtain a certificate more than five years after birth in these districts: through a long process of examination by district magistrates and the local Branch Executive Committee and involving multiple trips to the capital.

I combine this spatial variation with *temporal* variation: individuals born after the reform in one of the treated districts should also be more likely to be registered than those born before.

The identification strategy, ultimately seeking to instrument for individuals' registration status, comprises two steps. The first step is a difference-in-differences design where I compare individuals born after (versus before) the reform in districts which were (versus were not) treated by the reform. If the reform induced increased registration as intended, we should observe a larger difference in registration among cohorts born after versus before the reform in treated districts than in control districts. The baseline equation I estimate is the following:

$$\text{Registered}_{itd} = \beta^{FS}\text{Reform}_{itd} + \eta_d + \mu_t + \gamma X_i + \epsilon_{itd}, \tag{2.2}$$

where exposure to the reform, Reform_{itd}, is an indicator variable for whether i was born after the reform in one of the treated districts, and the other variables are as defined above for Equation (2.1). β^{FS} in Equation (2.2) estimates the effect of exposure to the reform on registration.

In the second step, I leverage this variation as a source of exogenous variation in registration to examine its causal effects in an instrumental variables setup. This implies a specification as follows, where I use Equation (2.2) as a first stage to predict registration, $\widehat{\text{Registered}}_{itd}$:

$$y_{itd} = \beta^{IV}\widehat{\text{Registered}}_{itd} + \eta_d + \mu_t + \gamma X_i + \epsilon_{itd}, \tag{2.3}$$

where y_{itd} is a given outcome variable and the fixed effects and clustering are as per Equations (2.1) and (2.2). β^{IV} in Equation (2.3) estimates the local average treatment effect: the causal effect of registration among compliers induced to be registered by the reform.[31] Then, to

[31]The temporality of the data means that the empirical strategy identifies the overall effects of registration, which accrue over time, rather than just pertaining to outcomes realized close to the time of survey

probe the *determinants* of registration, I examine heterogeneity in the first stage relationship. I do this by interacting the reform indicator in Equation (2.2) with district-level covariates to evaluate how the local balance of these benefits and costs of registration conditioned citizens' compliance decisions.

Identification assumptions. β^{IV} in Equation (2.3) is the primary coefficient of interest, which estimates a causal quantity if a set of identification assumptions are met. First, β^{FS} in Equation (2.2) must identify the effect of exposure to the reform on registration status. This primarily rests upon the assumption common to such difference-in-differences designs requiring that, *absent* the reform, rates of registration would have followed parallel trends over time in treated districts (where the reform was applied) relative to control (where it was not).[32]

I maximize the plausibility of this assumption in two ways. First, through sample restrictions. In the baseline specification I restrict the sample to only comprise cohorts born within ten years of the reform and restrict the set of control districts. Control districts are restricted to be those which shared the same 'parent district' as treated districts as defined prior to the reform, and are therefore the most geographically proximate districts to the treated districts.[33] Figure A.4 plots the location of these districts and Table A.3 maps relevant districts from the 1960s to 2012. Second, it could be objected that other time-varying district-level characteristics are confounding the identification of β^{FS}. Therefore, in additional specifications I add either region, or district, of birth-year of birth linear time trends to Equation (2.2). These time trends absorb either region-specific, or district-specific, general trends in registration over time.

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enumeration.

[32]Additionally, the identification of β^{FS} rests on the stable unit treatment value assignment (SUTVA) assumption that individual i's exposure to the reform does not affect j's registration status. The first stage robustness tests I provide below which include *household-level* fixed effects suggest that this no interference assumption is plausibly satisfied.

[33]For example, Arusha town was treated with the reform and was nested within the larger Arusha district. As observed in the census, Arusha town today corresponds to Arusha Urban district, while the *control* districts correspond to Arusha Rural and Meru districts.

Supportive of the plausibility of the identifying assumption, Figure A.5 provides visual evidence of parallel registration rates in the period leading up to the reform. To more formally test this, in the analysis below I include specifications where I add treatment lead terms to Equation (2.2), which reflect the treatment status of individual i in year $t+1$, $t+2$, and so on. I find no effects on these lead terms, which would otherwise provide evidence against the parallel trends assumption by suggesting non-parallel pre-trends.

If β^{FS} identifies the effect of exposure to the legal reform then, under additional assumptions, this suggests the availability of a source of exogenous variation in registration status. First, relevance requires a strong first stage coefficient β^{FS}, which I demonstrate empirically. Second, the exogeneity assumption requires that assignment to the instrument is as-if random, which I support using placebo tests in Table A.7. Third, monotonicity requires the absence of individuals who *would have* been registered absent the reform but *were not* registered due to the reform. This assumption appears reasonable in this setting.

Finally, the exclusion restriction requires that an individual being born shortly after, versus shortly before, the reform in a treated district compared to in a control district must only affect relevant outcomes through the increased probability of their registration at birth. A threat to this assumption would be the existence of other policies affecting treated districts (but not control districts), during the same narrow time period, which independently affect the later-life outcomes of individuals born during this period through different channels. Appendix A.1.3 provides supporting evidence towards the plausibility of this. In brief, a quantitative analysis of legislation during this period provides no evidence of other policies targeted specifically at the same set of districts. Further, while the *Arusha Declaration* heralded Tanzania's transition to a rural model of development, (1) the sample restrictions exclude the most rural districts; (2) the immediate implications of *Arusha* were limited; (3) as detailed in Appendix A.1.3, it is hard to account for *Arusha* affecting outcomes in ways consistent with the results.

2.5 Results

2.5.1 First stage

Table 3.1 estimates the first stage: the effect of variation in exposure to the reform on the probability of individuals being registered, and hence possessing a birth certificate, using Equation (2.2). In columns 1-3 I use the baseline specification of Equation (2.2), while in columns 4-6 I add five years of treatment lead terms to examine pre-trends, as discussed in the previous section. I linearly add region of birth-year of birth time trends (columns 2 and 5) and district of birth-year of birth linear time trends (columns 3 and 6) for more demanding tests.

Table 2.1: Effect of reform on registration

	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.06***	0.05***	0.04***	0.06***	0.05***	0.04**
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
$Reform_{t+1}$				0.02	0.02	0.01
				(0.01)	(0.01)	(0.01)
$Reform_{t+2}$				-0.02	-0.01	-0.01
				(0.01)	(0.01)	(0.01)
$Reform_{t+3}$				-0.00	-0.01	-0.01
				(0.01)	(0.01)	(0.01)
$Reform_{t+4}$				-0.00	-0.00	-0.01
				(0.01)	(0.01)	(0.01)
$Reform_{t+5}$				0.00	0.00	-0.01
				(0.01)	(0.01)	(0.01)
Time trends	None	Region	District	None	Region	District
F-statistic	50.7	138.2	14.3	18.3	19.5	6.5
Outcome mean	0.14	0.14	0.14	0.14	0.14	0.14
Observations	182635	182635	182635	182635	182635	182635

DV: respondent has a birth certificate. Data source: 20% extract of 2012 census. Sample restricted to cohorts born within ten years of reform in *treated* or *control* districts.
Specifications estimated using OLS including district of birth and year of birth fixed effects and control for gender. Exposure to reform is an indicator for being born after reform in a *treated* district. SEs clustered at the district of birth-level in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

The estimate of β^{FS} in column 1 indicates that the reform led to a 6 percentage point (pp) increase in the probability that an individual is registered. This represents an effect size

of nearly 50% compared to the outcome mean of 14%. The treatment effect is significant at the 1% level and associated with an F statistic above 50, which suggests it can be considered a strong instrument. The addition of substantially more demanding time trends only marginally decreases the coefficient estimate. There is no evidence, in Columns 4-6, of non-parallel pre-trends.

The β^{FS} coefficient implies that 6% of the sample in the baseline estimation are compliers who were induced to be registered by the reform. I assess the robustness of these estimates in three ways. First, Table B.11 permutes the sample and estimation by: (A) varying the number of cohorts included in the analysis; (B) excluding individuals born in particular years; (C) adding additional controls; (D) modifying the set of control districts; (E) estimating the same first stage relationship instead using the NPS dataset. Second, Table A.6 provides alternative estimation strategies, either (A) using a local linear regression or (B) through the use of household-level fixed effects. Third, in Figure A.6 I estimate a district-level jackknife of the first stage coefficient to test for the presence of outlier districts driving the first stage relationship. β^{FS} remains stably estimated across these different tests.

2.5.2 Consequences of registration

Under the plausibility of the identifying assumptions discussed above, I leverage this first stage relationship as a source of exogenous variation in registration status. Across the outcome tables, the first panel reports the 'naive' β^{OLS} obtained by estimating Equation (2.1) while the second panel reports β^{IV}, the primary coefficient of interest, obtained by estimating Equation (2.3).

Access to the state

In Table 2.2 I report the effects of registration on access to the state. Panel I reports outcomes relating to access to education, while Panel II reports outcomes relating to access to social security. In the table footers I report ρ(Income, DV), a standardized measure of the correlation between a given dependent variable and an index of asset ownership, to guide

interpretation.

Table 2.2: Effects on access to the state

	I. Education			II. Social security		
	Pri. (1)	Sec. (2)	Uni. (3)	HI (4)	Priv. (5)	State (6)
Registered (β^{OLS})	0.11***	0.31***	0.09***	0.11***	0.10***	0.11***
	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
$\widehat{\text{Registered}}$ (β^{IV})	-0.06	0.81***	0.16***	0.12	0.14**	0.15**
	(0.26)	(0.09)	(0.05)	(0.07)	(0.06)	(0.06)
DV Mean	0.79	0.14	0.03	0.08	0.05	0.04
DV SD	0.41	0.34	0.16	0.27	0.22	0.20
FS F-statistic	50.7	50.7	50.7	50.7	50.7	50.7
ρ(Income, DV)	0.14	0.33	0.24	0.18	0.26	0.22
Observations	182635	182635	182635	182635	182635	182635

DVs are all indicators. (1) has any primary education; (2) has any secondary education; (3) has any university education; (4) in a household accessing National Health Insurance Fund; (5) in a household accessing private pension; (6) in a household accessing state pension. ρ(Income, DV) is coefficient from regressing std. DV onto std. measure of asset ownership. Sample restricted to cohorts born within ten years of reform in either *treated* or *control* districts.
β^{OLS} estimated using Equation (2.1); β^{IV} estimated using Equation (2.3). All specifications include district of birth and year of birth fixed effects and control for gender. SEs clustered at the district of birth-level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

First, considering effects on access to education, the β^{OLS} estimates show broad differences in access between those who are registered and those who are not: registered individuals are 11 percentage points (pp) more likely to possess primary education, 31 pp more likely to possess secondary education, and 9 pp more likely to have university education. Examining the ρ vector shows that income correlates most strongly with access to post-primary education. The causal β^{IV} estimates, however, point to more selective benefits of registration on access to the state. The IV estimates show that registration causes no differences in access to basic education for the compliers induced to be registered by the reform. Registration does, however, cause striking increases in access to post-primary education, whether for secondary education or university, with effect sizes even larger than the β^{OLS}

estimates.[34]

Figure A.7 provides estimates of β^{IV} for access to education by specific *grade* of schooling. The figure, consistent with Table 2.2, shows null effects of registration on access to all levels of primary education (P1 to P7), but significantly positive coefficients on access to secondary education (S1 to S4), advanced secondary education (S5 to S6), and university education.[35]

Second, considering effects on access to social security, the β^{OLS} estimates again show broad differences: registered individuals are 11 pp more likely to have access to the National Health Insurance Fund (NHIF), 10 pp more likely to have access to a private sector pension, and 11 pp more likely to have access to a state pension. Examining the ρ vector suggests less pronounced differences compared to access to education, but that income correlates with access to pensions more strongly than with access to health insurance. In turn, the β^{IV} estimates provide stronger evidence of the causal effect of registration on access to pensions for the compliers, whether from the government or private sector, than on access to health insurance. While the point estimates are similar to each other and the β^{OLS} estimates, the estimate on access to health insurance is substantially noisier than for the other two outcomes.[36]

Table A.9 provides estimates relating to every category of social security access. The estimates show that effects on access to private pensions is driven by increased access to the National Social Security Fund (NSSF), which provides social security funds for individuals primarily with formal sector employment; effects on access to state pensions is driven by increased access to the Public Service Pension Fund (PSPF) and Government Employee Provident Fund (GEPF), each of which indicate employment by the central government.

[34]Figure A.8 plots raw trends in access to education across treated and control districts.

[35]Table A.8 provides estimates relating to literacy to support the effects on access to education. The estimates suggest no effect on literacy in Kiswahili, the language of instruction for primary education since shortly post-independence (Miguel 2004), but strongly positive effects on literacy in English, which is typically taught at higher levels of education.

[36]Figure A.9 plots raw trends in access to social security across treated and control districts.

Taxation

In Table 2.3, using the baseline NPS sample, I examine the extent to which registration induces increased exposure to taxation among the compliers induced to be registered by the reform. The β^{OLS} estimates suggest that registration is associated with substantially higher incidence of taxation, where registration is associated with a 13 pp increase in the probability of making payments to the state. The ρ vector suggests that income more strongly predicts the payment of central rather than local taxes or fees to the government.

Table 2.3: Effects on exposure to taxation

	All	Fees	Local	Central
	(1)	(2)	(3)	(4)
Registered (β^{OLS})	0.13***	-0.01	0.05***	0.13***
	(0.02)	(0.01)	(0.01)	(0.02)
$\widehat{\text{Registered}}$ (β^{IV})	0.53*	0.38*	0.00	0.39
	(0.30)	(0.20)	(0.16)	(0.26)
DV Mean	0.19	0.05	0.05	0.13
DV SD	0.39	0.22	0.21	0.34
FS F-statistic	13.9	13.9	13.9	13.9
Observations	1467	1467	1467	1467
ρ(Income, DV)	0.28	0.03	0.18	0.31
Observations	4429	4429	4429	4429

DVs are all indicators. (1) in a household which has paid any tax in the last year; (2) in a household which paid fees in the last year; (3) in a household which paid council rates in the last year; (4) in a household which paid taxes to the central government in the last year. NPS sample restricted to cohorts born within ten years of reform in either *treated* or *control* districts. β^{OLS} estimated using Equation (2.1); β^{IV} estimated using Equation (2.3). All specifications include district of birth and year of birth fixed effects and control for gender. SEs clustered at the district of birth-level in parentheses. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

While somewhat noisily estimated given the more limited sample size of the NPS sample compared to the census, the β^{IV} estimates suggest a substantively large effect of registration on tax payment overall (column 1), which is particularly driven by increased payment of fees

to the government (Column 2) and formal taxes to the central government (column 4).[37] In Table A.10 I estimate effects without restricting the set of cohorts included in the sample, which provides more precise, but very similar, point estimates.

2.5.3 Determinants of reform compliance

Consistent with the first implication of the theoretical framework, therefore, the results suggest that registration does not generate broad-based increases in access to public resources, but rather conditions access to public resources particularly relevant for the rich. The magnitude of the β^{IV} coefficients implies that, for individuals induced to be registered by the reform, these targeted benefits in access are substantively large. In turn, registration increases the incidence of tax payment. The second empirical prediction of the theoretical framework posits that citizens, given the relative incidence of these returns, face a strategic decision in electing to register with the state in the first place. To provide evidence towards this, in the context of the reforms I leverage for the instrumental variables estimates, I probe local and individual-level heterogeneity in citizens' compliance.

First, I examine how compliance with demands to register was conditioned by district-level variation in the threat of taxation at the time of the reform. I use two standardized measures of the local *levels* of taxation from Jensen and Mkama (1968): the share of individuals paying taxes, which captures the extensive margin of taxation; and the amount of tax collected per citizen, which captures the intensive margin. In addition, to proxy for local redistributive pressures, I use a standardized measure of the *incidence* of taxation drawn from Lee (1965), $(\tau^{\text{Max}} - \tau^{\text{Min}})$, defined as the difference in the district's tax rate faced by individuals in the highest tax bracket compared to those in the lowest tax bracket.

In Panel I of Table 2.4 I estimate the baseline first stage while interacting the reform indicator with these district-level variables.[38] The results show that a one standard deviation

[37] Figure A.10 plots raw trends in tax payment across treated and control districts.

[38] In all these analyses I control for local income per capita, population density and the interaction of each of these with the reform indicator. Doing so reduces concerns that the interaction terms I examine are simply picking up on overall local levels of development.

increase in each of these measures of local taxation significantly reduce the extent of citizen compliance with the reform. This holds irrespective of whether each interaction is estimated independently (columns 2-4), or simultaneously in the same specification (column 5). That is, citizens' compliance with demands to register is lower when either the overall level of local taxation is higher, *or* when the incidence of taxation particularly affects the wealthy. Conversely, the increase in citizens' registration induced by the reform is driven by changes in localities where citizens, and particularly the rich, faced relatively lower threat of taxation.

Table 2.4: Heterogeneity in compliance with reforms

		I. HE: Taxation				II. HE: Public goods		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform	0.063***	0.056***	0.054***	0.054***	0.056***	0.054***	0.044***	0.044***
	(0.009)	(0.004)	(0.005)	(0.005)	(0.004)	(0.010)	(0.008)	(0.008)
Reform × Share paying tax		-0.008***			-0.012**			
		(0.003)			(0.005)			
Reform × Tax per capita			-0.008***		-0.012***			
			(0.002)		(0.002)			
Reform × $(\tau^{Max} - \tau^{Min})$				-0.007**	-0.005**			
				(0.003)	(0.003)			
Reform × Primary schools						-0.001		0.001
						(0.003)		(0.004)
Reform × Secondary schools							0.021**	0.024*
							(0.010)	(0.013)
Outcome mean	0.14	0.14	0.14	0.15	0.15	0.14	0.14	0.14
Observations	182635	182635	182635	149575	149575	182635	182635	182635

DV: respondent has a birth certificate. Panel I examines heterogeneity using local measures of taxation; Panel II using local measures of public goods. Data source: 20% extract of 2012 census. Sample restricted to cohorts born within ten years of reform in *treated* or *control* districts. District-level measures of tax levels from Jensen and Mkama (1968); tax incidence from Lee (1965); local public goods from administrative data. Regressions control for interaction of *Reform* with standardized district income per capita and population density. All specifications estimated using OLS including district of birth and year of birth fixed effects and control for gender. SEs clustered at the district of birth-level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Second, to provide evidence on how variation in the expected benefits of registration affected enrollment, in Panel II of Table 2.4 I examine similar heterogeneity using local variation in the presence of particular public goods existing prior to the reform. Using administrative data and guided by the instrumental variable estimates, I construct a measure of the local presence of both *narrow-based* public goods in a district, using secondary schools, and *broad-based* public goods, using primary schools.[39] The estimates suggest that compliance with the reform is unrelated to the local presence of primary schools (column 6), and significantly *positively* related to the local presence of secondary schools (column 7). These effects continue to hold when I simultaneously include both interactions in the final column.

Third, I descriptively characterize the complier subpopulation who were induced to be registered by the reform, and for whom the instrumental variables estimates represent the causal effects of registration. In line with the prior results, we should expect that the individuals who complied with the reforms are those for whom these returns were positive in expectation. Following Abadie (2003) and Angrist and Fernandez-Val (2013), in Table 2.5 I assess descriptive characteristics of these compliers compared to the overall sample.[40] I consider gender and the educational attainment of individuals' *parents*, which offers a proxy for the extent to which a given individual is born into a wealthy family.

This exercise suggests that the reform induced the registration of boys more than girls: among compliers, 59% are male compared to 47% of the overall sample.[41] Comparisons using parental educational attainment show striking differences: compliers are only 11% more likely to have parents with primary education, but over twice as likely to have parents with secondary education, and three times as likely to have parents with university educa-

[39] I use an indicator for whether a given district had any secondary schools, since only a handful of districts had more than one secondary school during this period, and a continuous measure of the number of primary schools, since these were far more widespread.

[40] Marbach and Hangartner (2020) provide a recent application of these approaches to political science.

[41] Prior work on the gendered dimensions of civil registration demonstrates variation in the timing of registration (Harbers 2020) but provides relatively limited evidence on other gender biases in registration decisions (Bhatia et al. 2019).

Table 2.5: Complier characteristics

Variable	Sample mean (1)	Complier mean (2)	Ratio (3)
Male	0.47	0.59	1.26
Parent has primary education	0.41	0.46	1.11
Parent has secondary education	0.04	0.08	2.03
Parent has university education	0.01	0.03	3.06

Table computes covariate means for full baseline sample (column 1) and compliers (column 2). Column (3) is column (2) divided by column (1).

tion. The complier characteristics are consistent with the reform inducing individuals born into relatively wealthier families, for whom the returns to enrollment were positive, to be registered.

Overall, then, these results provide evidence consistent with the theoretical framework. While the instrumental variables estimates demonstrate that registration has distinctive *effects* on access to public goods and exposure to taxation, the analysis of citizens' compliance confirms that variation in the relative incidence of these costs and benefits, particularly relevant for the wealthy, condition citizens' decisions whether to supply information to the state.

2.6 Conclusion

A growing literature highlights the informational foundations of state capacity. In this paper I have examined a channel through which such capacity is initially built: through transactions whereby citizens trade information for increased access to the state. In contexts where citizens face high costs of registration, either due to the inaccessibility of infrastructure or through increased exposure to taxation, inducing compliance therefore relies upon the provision of expected benefits. These benefits, moderated by bureaucratic capacity and political competition, bias towards areas of particular utility for wealthier citizens. In turn, the rich are induced to comply and they accrue increased access—particularly to narrow-

based public goods—while increasing their exposure to taxation.

Testing the implications of the argument in Tanzania, a series of reforms in the post-independence period highlight the nexus between registration and taxation and help to explain striking failures of the state to solicit information from its population over time. Leveraging variation in citizens' exposure to these reforms in an instrumented difference-in-differences design, I provide novel estimates of the effects of registration. For the compliers induced to be registered by the reform, the benefits are substantial but targeted, with effects on access to narrow-based public goods in exchange for increases in the incidence of tax payment. Supporting citizens' underlying calculus in electing to supply information to the state, the local incidence of benefits and costs conditioned citizens' willingness to comply with the reforms.

While governments may reap rewards from the broad expansion of such schemes, therefore, their failure to expand broad coverage implies that schemes which seek to build informational capacity can have regressive distributional effects on the coverage of the state. This argument runs against policy narratives surrounding the expansion of such schemes, which typically assume that registration has *progressive* effects on access. Further, while the expansion of social welfare programs has been shown to drive broad enrollment, the argument implies that registration is unlikely to have progressive effects on access in contexts where the welfare system is truncated and the state is weak. Indeed, the initially regressive coverage of the state might create dynamic obstacles to expanding the state's reach fully.

Both theory and empirics suggest that persistent failures to develop the state's informational capacity through broad enrollment in such schemes are underwritten by a state-building equilibrium that limits the equalizing, progressive potential of these instruments to empower citizens to make demands upon the state. As a result, these failures are just as much *political* in origin as they are administrative or bureaucratic. Such technologies, instead, risk exacerbating pre-existing inequalities in access to the state when compliance, and its benefits, cannot be taken for granted. While limited to a particular institutional context, they suggest a focus on the distributional incidence of the returns to formal identi-

fication is needed to understand the aggregate consequences of the ongoing, rapid expansion of biometric identification systems across much of the developing world. An increased focus on these distributive dynamics is crucial for understanding the political economic impact of efforts to register the world's poor.

3 | The Limits of Legibility: How Distributive Conflicts Constrain State-Building[1]

3.1 Introduction

"*To be governed is to be noted, registered, enumerated, accounted for, stamped, measured, classified, audited, [...] in every operation, every transaction, every movement.*"
Proudhon (1851)

A distinguished literature conceives of states as seeking to impose uniform control throughout their territories but being constrained by the high costs of doing so (Herbst 2000; Migdal 1988; Scott 1998). Accordingly, the failure of many developing countries to successfully deliver services, extract taxation, and consolidate political support is often thought to be a problem of a latent 'weakness' in their capacity. As scholars increasingly note, however, subnational variation in measures of state capacity and in these governance outcomes confounds explanations premised on binding resource constraints alone (Christensen and Garfias 2021; Gottlieb 2019; Sánchez-Talanquer 2020; Soifer 2008). This paper argues that the problem is not limited resources, but rather a set of perverse and self-reinforcing political incentives which constrain the full expansion of the state's reach.

I focus on one dimension of states' capacity: *legibility*, the extent of systematized infor-

[1]With thanks to Sarah Balakrishnan, Paige Bollen, Peter Buisseret, Jeff Frieden, Jessica Gottlieb, Gabe Koehler-Derrick, Ben Marx, Torben Iversen, Horacio Larreguy, Melissa Lee, Evan Lieberman, Carl Müller-Crepon, Shelley Liu, Mike Olson, Pia Raffler, and Elsa Voytas for comments, and audiences at APSA 2020, EuroWEPS II, Harvard, Institute for Advanced Study in Toulouse, Konstanz, NEWEPS-15, Oxford Blavatnik, UCL, UCLA Politics of Order & Development Lab, the Weatherhead Center for International Affairs, and Yale. With thanks to the Ghana Statistical Services, Paige Bollen, Martin Guenther and Philomena Nyarko for assistance with the data, and to staff at the United Nations Archives.

mation that central states possess about their citizens (Brambor et al. 2020; Scott 1998). Harking back to the informational core of classic conceptions of the state (Giddens 1986; Mann 1984), recent work has articulated how epistemological technologies of registration, enumeration, and classification cut across the functional capabilities of the state (Berwick and Christia 2018; Hanson and Sigman 2021; Lindvall and Teorell 2016). In turn, scholars have highlighted legibility's contemporary relevance for governance, as developing countries increasingly administer social policies (Hunter and Brill 2016), develop their tax bases (Lee and Zhang 2017), and run elections (Piccolino 2016). Just as Proudhon equates these technologies with governance itself, theory and evidence suggest that states might broadly benefit from the expansion of legibility to the full population. That a billion people, overwhelmingly in the world's poorest countries, are "invisible" to their governments is sometimes therefore attributed to the prohibitively high costs of inducing them to be seen (Setel et al. 2007).

Because the state-led development of new capacities creates relative gains and losses across economic strata, I argue that conflicts which arise from the distributive incidence of this capacity undermine and disincentivize states' efforts to expand their reach fully. The argument proceeds in three steps. First, because legibility reduces state-citizen transaction costs, its broadening facilitates unmediated access to the state among previously excluded, typically relatively poorer, groups. Second, the comparative illegibility of poorer citizens otherwise facilitates preferential access to the state among wealthier citizens. In exchange for their privileged access to resources, wealthier citizens 'quasi-voluntarily' comply with the state's demands for resources because they expect redistribution to be narrow rather than broad. Third, states' efforts to disrupt this regressive status quo therefore have countervailing effects. Such efforts can promote the delivery of services to relatively poorer citizens and politically mobilize them, but might also simultaneously represent a threat to wealthier citizens. In settings characteristic of many developing countries, where the coercive enforcement of taxation is limited, their reduced compliance might lead wealthier citizens to *reduce* their fiscal and political support for the state over time—just as the expectations and demands of poorer citizens increase.

The argument implies that the state-led expansion of legibility has the potential to lessen economic inequalities but may also foment distributive conflict, political division, and disappointment. These conflicts, akin to the destabilizing effects of modernization (Huntington 1968), limit the incentives states face to invest in developing their capacity. Important scope conditions affect the generalizability of the argument. First, it is contingent on the existence of policies, such as social assistance programs, for which administration relies on dense state-citizen interaction. Second, it depends on the way that states elect to develop their capacity over time. Particularly when capacity builds gradually, rather than in sudden coordinated shifts, societal groups have time to recognize and respond to disadvantageous shifts in its distributive incidence.

I study Ghana, an ideal context to test implications of the theory: the state has persistently underinvested in the legibility of its population; its coverage remains regressive in spite of democratization and the expansion of various social policies over time; and wealthier citizens' higher rates of tax compliance are conditioned by the limited extent of redistribution towards the poor. The theoretical framework implies a number of empirical challenges to isolate the distributive consequences of state-led investments in legibility—not least, that states do not typically randomly decide where to build. To overcome these challenges, I leverage a natural experiment arising from the use of a population-based threshold used to allocate identity registries across localities in 1975. This generates quasi-random variation in the extent of the state's efforts to render its population legible, since localities just above the threshold were 'treated' with an identity registry where citizens could register with the state and obtain documentation. In turn, as the state begins to administer a widening set of policies which rely on dense state-citizen interaction, the increased possession of identity documents facilitates access to resources absent the intermediation of informal local authorities.

To execute the empirical design, I digitize and geolocate locality-level data from the 1970 census which was used to define the population threshold. Validating the applicability of a regression discontinuity design, I show that treated localities just above the threshold

appear statistically indistinguishable from control localities just below. This enables me to hold fixed a wide array of potentially confounding variables including population density, ethnic heterogeneity, and the presence of local public goods.[2] I evaluate effects mostly drawing on six geolocated waves of the Ghana Living Standards Surveys (GLSS), a nationally representative household survey spanning from 1987 to 2017. Employing this data source permits an assessment of how the discontinuous assignment of registries has medium-term effects on (1) the *levels* of outcomes across localities; (2) the *distribution* of outcomes, leveraging heterogeneity based on respondents' income, either observed or predicted using machine learning approaches to obviate post-treatment bias concerns; and (3) how these effects evolve *temporally*, as societal groups gain and lose from variation in legibility over time.

In line with theoretical expectations, I establish four results. First, to show that treated localities are quasi-randomly more legible than control localities, I show that the probability of individuals possessing proof of identity increases by 21 percentage points (pp) at the threshold, from 47% in control localities to 68% in treated localities. Second, especially relatively poorer citizens have increased access to social policies in treated localities compared to control over time: respondents are more likely to receive transfers from the government, to be covered by social health insurance, and report finding their interactions with state agencies to be more efficient.[3] Descriptively, access to the state is *regressive* is control localities but *progressive* in treated localities.

Third, in response, I show that relatively wealthier citizens are significantly less likely to pay taxes in localities just above the threshold. I demonstrate this is driven primarily by a reduction in their compliance rather than by changes in enforcement. Consistent with

[2]Further, I demonstrate that (1) the density of localities does not discontinuously change at the threshold, which would indicate sorting, (2) the population threshold does not predict differences in the local presence of the state aside from that of identity registries, which would otherwise represent a compound treatment concern, and (3) the composition of respondents in the outcome data is indistinguishable at the threshold, which would represent a differential migration concern.

[3]There are no equivalent differences in access to local public goods, such as relating to local education and healthcare, for which access tends to be more decentralized.

compliance being affected by temporal changes in the distribution of access to the state, I show that the reduction only occurs in survey waves enumerated since the expansion of relevant social policies. In turn, relatively wealthier citizens exhibit lower levels of tax morale, and weaker norms of compliance, just above the threshold. Finally, I show that although measures of political engagement increase, citizens overall (and irrespective of income) are notably more disapproving of the quality of their governance in treated localities compared to control.[4] Taken together, the results underscore how citizens' responses to changes in local state capacity condition its effects and imply 'supply-side' disincentives against universalizing the state's reach.

This paper speaks to two literatures. First, it contributes to work on the causes and consequences of redistributive welfare policies. Scholars tie the long-run expansion of welfare policies with the progressive expansion of the bureaucracy, the decline of clientelism, and the broadening of citizenship entitlements to previously excluded groups (Caplan and Torpey 2001; Bustikova and Corduneanu-Huci 2017; Harbers 2020; Hunter and Brill 2016). On the other hand, such policies are often implemented in low-information settings where barriers to access remain high, citizens' access is truncated, and inequality seems to persist (Barrientos 2013; Breckenridge and Szreter 2012; Ferguson 1999; Gupta 2012; Holland 2018). In line with recent work considering the political disincentives that states face to invest in their capacity (Christensen and Garfias 2021; Gottlieb 2019), this paper provides one rationale for the short-run persistence of these barriers. When local capacity develops gradually, then shocks which facilitate equitable access to the state might otherwise leave it vulnerable to the imposition of demands it struggles to meet.

Second, an established literature examines the positive relationship between state capacity and taxation: stronger states tax more, and more tax strengthens states (Bates and Lien 1985; Besley and Persson 2013; Huntington 1991; Tilly 1985). Studies of legibility, accordingly, have typically focused on how it facilitates taxation and extraction by bringing

[4]This is consistent with other work finding a negative effect of access to services on political approval across sub-Saharan Africa (Brinkerhoff, Wetterberg, and Wibbels 2018; De Kadt and Lieberman 2017).

citizens into the state's fiscal net (Christensen and Garfias 2021; Lee and Zhang 2017; Scott 1998). In contrast, this paper finds null effects on overall levels of taxation but large shifts in its *incidence*.[5] While positive effects among relatively poorer citizens are consistent with this prior work, negative effects among relatively wealthier citizens are not. Instead, these reductions speak to the importance of quasi-voluntary compliance in settings where tax enforcement remains weak (Besley 2020; Levi 1988; Luttmer and Singhal 2014; Torgler 2007) and how increases in capacity can trigger political responses (Kasara and Suryanarayan 2015; Weigel 2020). Because such compliance is affected by how funds are redistributed across social groups (Alesina, Baqir, and Easterly 1999; Lieberman 2003), this paper shows that legibility has spillover effects: *who* is being rendered legible matters for its overall effects on governance outcomes.

3.2 Theoretical framework

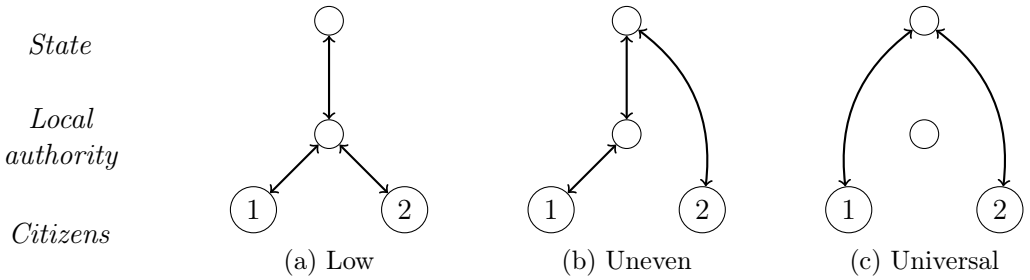
Seminal works on the political economy of development argue that states can elect to govern directly or indirectly (Boone 2003; Mamdani 1996). Direct modes, administered by agents of the central state, permit the closer monitoring of policy implementation; indirect modes, through which authority is delegated to local (often informal) authorities, inhibit monitoring but may provide superior outcomes at lower resource costs to the central state (Baldwin 2016). One important reason for these potentially improved outcomes relates to the informational advantages of locally-embedded authorities relative to the state's agents (Scott 1998).

In this section, I first provide a set of three stylized settings to illustrate how variation in state-citizen informational linkages affects governance. Second, I show how state-led efforts to dynamically transition between these different settings, through investments in their informational capacity, might incur distributive conflict. Third, given these effects, I

[5] A recent metastudy of six coordinated randomized controlled trials to encourage citizens' formalization also finds no effects on tax compliance, although this null effect is largely driven by the failure of the various interventions to affect citizens' formalization decisions in the first place (De La O et al. 2020).

consider the balance of benefits and costs states incur from broadening their capacity and what these imply for their incentives to do so.

Figure 3.1: State-citizen informational linkages and governance



(a) Low (b) Uneven (c) Universal

First, consider a context where the state's low informational reach renders state-citizen transaction costs high. Taking the example of administering welfare transfers, which relies on the revelation of information that citizens face incentives to misreport, governments often delegate discretion to select recipient households to local authorities because they possess informational advantages (Alderman 2002; Alatas et al. 2019; Bardhan and Mookherjee 2005).[6] Or, consider eligibility for voting. In many of Africa's early post-independence elections, because state agents had little way of identifying eligible voters, traditional authorities were given full discretion over who could access electoral participation (Mackenzie and Robinson 1960). Last, consider taxation. Examples of tax administration being delegated to local elites due to their informational advantages feature prominently in state-building accounts (Levi 1988). Each of these examples correspond to case (a) in Figure 3.1.

Second, consider a context where the state's universal informational reach lowers state-citizen transaction costs. For example, where social registries document individuals' eligibility for transfers (Gelb and Metz 2018), biometric identification permits the direct identification and payment of recipients (Muralidharan, Niehaus, and Sukhtankar 2016), permanent voter registers document eligibility for voting (Piccolino 2016), or cadasters document tax liability (D'Arcy and Nistotskaya 2017). In these cases, corresponding to case (c) in Figure

[6] As Barrientos (2013) notes, "The crucial role of information and information systems in the effective implementation of social transfer schemes is often underestimated" (p.143).

3.1, the central state is more easily able to govern through unmediated exchange between state and citizen rather than through the intermediation of local authorities.

Third, however, across much of the developing world we often observe the uneven case (b). Here, some citizens (denoted "1" in the figure) are effectively indirectly governed, because they face high barriers to direct state-citizen access, while others (denoted "2") access resources through unmediated interactions with the state (Berenschot 2019; Magaloni, Diaz-Cayeros, and Estévez 2007).[7] Take, again, access to electoral participation. While legal identity documents such as birth certificates are nearly universally accepted as sufficient evidence to go on the voter rolls, many countries simultaneously permit the testimony of local authorities as sufficient. Such informal allowances vary by country, but are present in at least 20 African democracies. In Liberia, the testimony of a village elder remains sufficient to vote; in Uganda, the LC1 chairperson; in Gambia, the alkalo chief (Carter Center 2013).[8]

3.2.1 Economic stratification in state-citizen interaction

Variation in legibility consequently has important implications for the feasibility of different modes of governance. In this paper I focus on the dynamic challenge of state-led efforts to shift from case (b) to case (c), facilitated by the expansion of legibility from a narrow to a broad set of the population. We should expect that case (b) is typically economically stratified: relatively poorer citizens, who are less legible to the state, must rely on mediated access to resources while relatively wealthier citizens, who are more legible, enjoy direct access. The reasons to expect such a stratification are motivated fully in Chapter 2, which posits that the transition from case (a) to case (b) rests on the incentive-compatible

[7] In the literature on welfare targeting, the incidence of such programs is often defined based on type I errors of exclusion (eligible individuals fail to access a program) and type II errors of inclusion (otherwise ineligible individuals access a program). As Muralidharan, Niehaus, and Sukhtankar (2020) show, attempting to enforce strict identity requirements in a context where many citizens face high costs of interacting with the state leads to large type I errors. As a result, even welfare-maximizing governments face incentives to permit the simultaneous existence of direct and indirect channels of access.

[8] Suggestively consistent with how the power of local authorities might limit the expansion of schemes to build state-citizen informational linkages, these informal provisions are evident in twice as many former British colonies, where traditional authorities were generally entrenched during the colonial period, compared to former French colonies, where they were generally diminished.

structuring of bargains to induce the rich to comply with legibility-increasing schemes which increase their exposure to taxation.[9]

As a result, citizen "2" in Figure 3.1 is typically wealthier than citizen "1."[10] This has three implications. First, it implies that relatively poorer citizens are more likely to be reliant on relationships with local leaders to make claims on public resources. Facing sufficiently high barriers to accessing the state, the poor are either entirely excluded from access or otherwise must often rely on mediated clientelist brokerage (Berenschot 2019; Rizzo 2019). The presence of such barriers, in turn, dampens their expectations of state service provision (Holland 2018; Singer 2009). Second, it implies that access to the state is likely to be *regressive*, as the wealthy enjoy preferential access to the state. Prior literature consistently finds only a weak relationship between citizens' income levels and their access to redistributive welfare transfers in developing countries (Bastagli 2009; Ferguson 1999; Tschirgi 2000). Such truncated access to welfare is partially explained by variation in the costs faced by citizens of different income levels seeking to make claims on public resources (Currie 2004; Kleven and Kopczuk 2011).

Third, crucially, we might expect that relatively wealthier citizens are more likely to supply taxes to the state both due to increased enforcement *and* increased compliance. As a large literature on fiscal bargaining contends, citizens' willingness to comply with the state's demands for taxation is affected by their expectations of how their funds will be used (Levi 1988; Timmons 2005). This is particularly important in low-enforcement environments, where the supply of these instrumental benefits helps to sustain tax morale and norms of compliance (Luttmer and Singhal 2014; Torgler 2007). Such 'quasi-voluntary' compliance is sustained by expectations about how fiscal resources are redistributed across societal

[9]The poor, additionally, face higher costs to enrollment in schemes that broaden state-citizen informational linkages. As Ferguson (2012) notes, "the burdens of obtaining and retaining documents fall hardest on the poorest ... who often fail for this reason to qualify for schemes ostensibly meant to benefit them" (p.513).

[10]For one illustrative example, Figure B.7 plots the probability of possessing a national ID card by income quintile across different non-OECD regions. This demonstrates a strikingly positive relationship in sub-Saharan Africa particularly.

groups: when citizens expect that broad redistribution is unlikely, they are more likely to comply (Alesina, Baqir, and Easterly 1999; Hollenbach and Silva 2019; Lieberman 2003). As such, the differential legibility of the wealthy might both facilitate the enforcement of direct taxation by the state (Kiser and Sacks 2009) but *also* encourage them to supply funds to the state in the expectation that these resources will be redistributed narrowly rather than broadly.

3.2.2 Distributive consequences of broadening legibility

I next consider the distributive consequences of state-led efforts to expand legibility to the full population, given this regressive status quo, and use these effects to speak to the tradeoffs faced by the state in electing to do so. First, states benefit through the increased supply of welfare to relatively poorer citizens, which is likely to induce their political support and facilitate their downstream taxation in return. This is because, as scholars have argued, indirect modes of governance risk distortion and manipulation by local authorities who can often only be weakly monitored by the central state (Kasara 2007; Tordoff 1994). In turn, empirical studies have found that more direct modes tend to be associated with reduced leakages, rent extraction, and more efficiently targeted resources (Alatas et al. 2019; Balan et al. 2020; Banerjee et al. 2018; Muralidharan, Niehaus, and Sukhtankar 2016).

The magnitude of this gain in effective service delivery is conditioned by the set of policies available to implement. While such distortions affect many dimensions of governance, they are particularly pernicious for policies where citizens' eligibility is defined at the individual-level, rather than the locality-level, such as for a wide range of social policies (Gupta 2012; Szreter and Breckenridge 2012).[11] Such policies have spread rapidly across developing countries in recent years, at least partially due to the spread of democratization and more programmatic political competition (Barrientos 2013; Hunter and Brill 2016;

[11]The same concern holds for modes of electoral administration which permit informal modes of identity verification. The Carter Center (2013) notes that "The biggest concern in these more flexible systems is the use of tribal and community leaders to vouch for an applicant," (p.16) due to the discretion afforded to local authorities.

Kitschelt and Kselman 2013).[12] Investing in the legibility of relatively poorer citizens, who otherwise face mediated channels of brokerage to access the state, can then induce their political mobilization and support. This occurs through increasing the extent of their unmediated state-citizen interactions, which in turn increases their expectations of state service provision (Gottlieb 2019; Healy, Kosec, and Mo 2017; Weigel 2020).[13]

On the other hand, states incur distinctive costs from investing in the expansion of legibility to the full population. Most often cited as inhibiting such investments, the bureaucratic infrastructure to enumerate, identify, and classify citizens can impose prohibitively high costs on resource-poor states (Breckenridge 2014; Scott 1998). But beyond these resource costs, the preceding discussion suggests that state-led investments threaten the privileged access otherwise enjoyed by relatively wealthier citizens. As the state's informational reach expands, so does its ability to effectively redistribute resources to previously excluded groups. As a result, this expansion over time is likely to inhibit the tax morale and compliance of wealthier citizens as they come to realize that their resources might be redistributed broadly rather than narrowly (Besley 2020; Lieberman 2003). In low-enforcement contexts the collection of taxation relies heavily on such compliance. When so, and the development of capacity is slow-moving, over time relatively wealthier citizens might respond to local increases in capacity by reducing their fiscal support for the state absent costly complementary investments in its enforcement. This is likely to extend to reductions in the extent of their political support for the state (Kasara and Suryanarayan 2015).

State-led efforts to broaden legibility then have the potential to promote governance outcomes along one margin, by facilitating service delivery towards relatively poorer citizens and rendering access to the state more equitable, but over time *also* constrain outcomes

[12]Evidence from World Bank (2018c), for example, suggests that spending (as a share of GDP) on centrally-administered social protection programs has dramatically increased in the world's poorest countries in the past two decades.

[13]This also implies that local authorities face incentives to inhibit the implementation of formalizing schemes to the extent that they risk losing influence. Illustratively, a recent study used local leaders to distribute identity cards relating to food subsidies in an effort to reduce leakages. Only 30% of the cards were actually distributed to citizens, reflecting the incentives these local leaders may face to prevent the expansion of such schemes (Banerjee et al. 2018).

along others, by generating distributive conflict and political opposition. Since the development of more uniform state capacities implies differential gains across economic strata—in this case, increasing the threat of redistribution from rich to poor—citizens' offsetting distributive responses to changes in local capacity notably weaken the incentives that even well-intentioned states face to invest in the expansion of their local capacity.

3.3 Hypotheses

I draw a number of hypotheses from the preceding discussion. First, H1 posits that states do face 'supply-side' decisions in deciding to broaden their informational reach, rather than purely being constrained by the high costs of projecting authority across their territory. This hypothesis is not mechanical: as a large literature posits, citizens might instead hide from an intrusive state (Scott 2009; Szreter and Breckenridge 2012).

Hypothesis 1. *State-led investments increase the legibility of the local population.*

Second, broadening legibility lowers the cost of unmediated state-citizen interaction. This particularly affects the administration of policies for which access relies on such interaction, such as many social policies, and among relatively poorer citizens who otherwise face mediated channels of brokered access to the state. In turn, we should expect much weaker effects on access to public resources for which access is decentralized to local authorities rather than being centrally administered. This leads to H2:

Hypothesis 2. *Citizens in more legible areas have (**a**) increased access to policies that rely on state-citizen interaction; (**b**) these gains are particularly concentrated among relatively poorer citizens.*

Third, this reduced inequality in access creates relative losers across economic strata: the relatively non-poor, who lose their preferential access and risk their resources being redistributed more broadly. In response, when the coercive enforcement of taxation is limited, we may expect the wealthy to reduce their compliance with the state's demands for resources over time. This leads to H3:

Hypothesis 3. *Relatively wealthier citizens, in more legible areas, become less willing to comply with demands for taxation.*

Fourth, these changes ought to affect political engagement and mobilization. We should expect that the increased intensity of direct interaction with state agencies among the poor, in addition to potentially loosening their ties to local authorities, increases citizens' expectations of state social service delivery and the extent of their demands. Increased legibility might also directly affect participation by, for example, easing access to voter registration. This leads to H4:

Hypothesis 4. *Relatively poorer citizens, in more legible areas, become more politically mobilized.*

Finally, these channels have implications for net effects on political support for the state. Wealthier citizens are likely to disapprove of their governance as they no longer benefit from preferential access to the state. Effects among relatively poorer citizens trade off (1) the first-order effect of their increased direct access to the state; (2) the second-order effect that the fiscal resources available to meet their increased demands and expectations do not increase in tandem. This leads to H5:

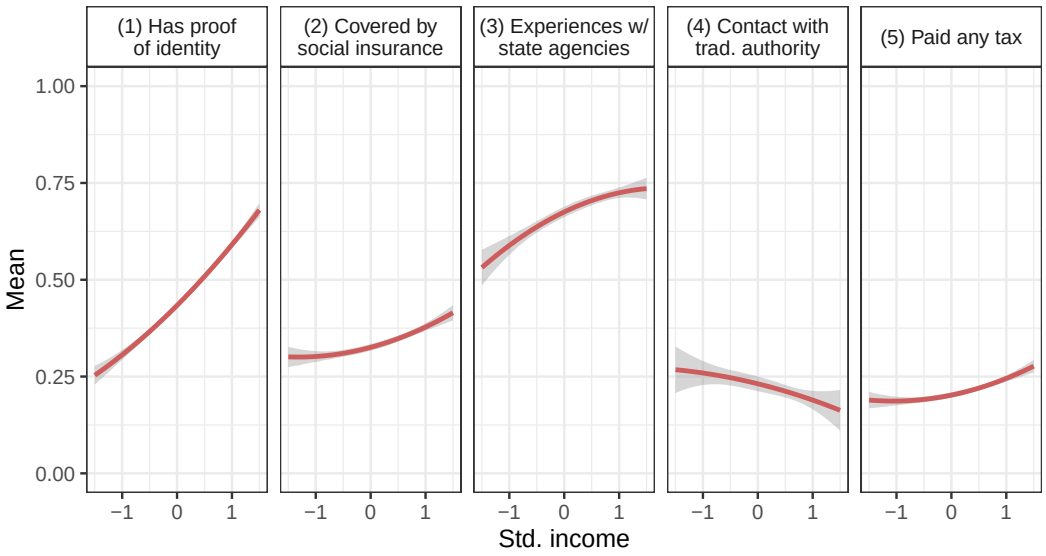
Hypothesis 5. *When both H3 and H4 hold, the expansion of legibility can lead citizens to reduce their political support for the state.*

3.4 Policy, capacity, and legibility in Ghana

I examine these hypotheses in the context of Ghana. Verifying the empirical relevance of the case to the framework, in Figure 3.2 I use the data sources described in the next section to illustrate how particular measures of state-citizen interaction vary along with citizens' income levels. The plots show that wealthier citizens are (1) more legible to the state, as proxied by their possession of state-issued identity documents; (2) more likely to be covered by a nominally redistributive state social insurance scheme; (3) more likely to

report positive experiences interacting with state agencies; (4) less likely to report having contacted a traditional authority in the last year; (5) more likely to have paid taxes to the state in the last year. With the context reflecting the regressive status quo considered by the theoretical framework, I discuss each component in turn below.

Figure 3.2: State-citizen interaction and income



X-axis provides a measure of household income depending on the data source used for each outcome. Panel (1), (2), (3), (5) use standardized (log) consumption per household member from Ghana Living Standards Survey data. Panel (4) uses an index of asset ownership from Afrobarometer.

3.4.1 State-led investments in legibility

Like many other postcolonial regimes, the Ghanaian state inherited an "informational void" upon its independence in 1957 (Breckenridge and Szreter 2012). Early post-independence efforts to expand the state's informational reach under the socialist Nkrumah regime involved several initiatives to enumerate and register its population.[14] These efforts, including the 1960 census and the passage of the *Births and Deaths Act*, were intended to facilitate central planning and to rationalize resource allocation (Serra 2018). Illustratively, motivating the passage of the latter act in parliament, the responsible Minister argued,

[14] Appendix B.1.1 provides supplementary information on the historical context of these efforts and similar ones in other countries.

> "Without reliable statistics from which an accurate calculation of the births and deaths rate of the nation may be made, our manpower requirements cannot be realistically assessed and our National Plan cannot be effectively implemented ... Vital registration is essential for our national progress and our socialist reconstruction" (Minister Bawumia, *Hansard*, 31 August 1965).

Parliamentarians immediately raised concerns about the expected success of efforts to induce citizens to be seen by the state absent high infrastructural investments (*The Ghanaian Times, 1 September, 1965*). With just 37 registry buildings and a highly rural population, Ghana's decentralized government units were given significant responsibility for encouraging the compliance of citizens. These units exerted little effort in inducing the registration of citizens at a scale meaningful for input to policy planning.[15]

Funding for the administration of efforts to register citizens has been enduringly low even as the practical use of documentation for citizens, as I describe below, has sizably increased.[16] Recent efforts to roll out modern biometric technologies to facilitate the identification of citizens have repeatedly stalled or failed entirely (Effah and Debrah 2018; Thiel 2017). Reviews underscore how the continued underfunding of such efforts, by facilitating significant informal rent extraction, multiplies the already-high costs citizens face when registering with the state (Peters, Mawson et al. 2015).[17] As Figure 3.2 shows, around half of the population possess state-issued identity documents with striking variation by income levels.

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[15] One member of parliament at the time correctly anticipated the connection between such efforts and rent extraction: "even though certificates are meant to be free, certificates are not, in fact, going to be free because of the possibility of having to grease the palm of the officer before getting a certificate" (Mr Kwaw-Swanzy, *Hansard*, 2 September 1965).

[16] Mehta and Assie (1979), for example, points to the unwillingness of local-level authorities to publicize and provide resources for the staffing of registries, which later became their formal responsibility following the 1988 constitution (Ayee 1994).

[17] Citizens seeking to obtain identity documents face high barriers to doing so and must often work through intermediaries: "community volunteers have effectively become "agents" assisting citizens to register births for illegal fees" (p.14) (Peters, Mawson et al. 2015), often charging fees up to $20 to facilitate registration (UNICEF 2012). Extracting "black revenues" incentivizes officers to prioritize adult and delayed registrations, which are associated with inflated fines and hence increased informal revenue generation (UNICEF 2012; Ghana Statistical Service 2015).

3.4.2 Indirect modes of governance

The state's persistent underinvestment in broadening its informational reach has gone hand in hand with the enduring influence of traditional local authorities, i.e. chiefs, as tools of local distribution. Although the Nkrumah regime sought to undercut the authority of traditional authorities who had been elevated by colonial policies of indirect rule,[18] subsequent regimes systematically incorporated non-state authorities into the formal state apparatus (Apter 1963; Boone 2003; Rathbone 2000). The extent of this incorporation was sufficiently high that administrative units were often designed to exactly match the boundaries of particular traditional authorities (Harris 1983).[19] As a result, the exercise of state authority over time has been characterized as indirect and highly decentralized (Ayee 1994), with only weak monitoring of local authorities (Herbst 1993; Tordoff 1994).

Scholars have argued that the relative absence of the central state, by weakening citizens' expectations of service provision, fortified community-based structures of redistribution (Chazan 1983). This absence, in turn, entrenched localized clientelist networks where access to limited public resources depended heavily on the influence and discretion of local authorities (MacLean 2010). While important work highlights the ethnic dimensions of national politics in Ghana, at the local-level these networks, particularly in more urban areas, are often highly multiethnic (MacLean 2010; Harding and Michelitch 2021; Ichino and Nathan 2013).[20]

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[18]Important regional variation exists. While Northern Ghana was a canonical example of indirect rule, the southern regions (Gold Coast) were relatively more directly ruled during the colonial period (Nathan 2019).

[19]Traditional authorities became formally incorporated into the administration of the state, such as through major decentralization reforms in 1974 and 1988 that mandated their representation (Tordoff 1994). Below their formal incorporation at the district level, administration at the local-level was significantly delegated to these agents, such as through their control of town and village committees responsible for managing local services (Rowat 1980).

[20]Since I focus on cleavages relating to income rather than ethnicity it is worth validating that the former is not just proxying for the latter. Figure B.8 disaggregates Figure 3.2 according to whether the respondent is a member of the local ethnic majority and shows that majority status is associated with differences in overall levels of state-citizen interaction. However, *within* majority/minority status, income has similarly pronounced effects across the different measures.

3.4.3 Development of social policies

Failures to expand the informational reach of the state could be rationalized by the absence of policies whose administration necessitates dense state-citizen interaction. Accordingly, focusing on policies relating to social assistance, earlier efforts were either specifically targeted at the local-level or otherwise intended to stimulate 'self-help' projects through the provision of informal labor and taxation (Aryeetey and Goldstein 2000).[21]

Ghana's democratic transition in the '90s reconfigured the incentives faced by the state to deliver welfare to the poor in pursuit of their political support (Harding 2020). Accordingly, and echoing broader global trends, an increasing number of individually-targeted social policies have been implemented which rely on dense state-citizen interaction (Ferguson 2012). One prominent example is the National Health Insurance Scheme (NHIS), funded by tax revenues, which was implemented in 2004 and aimed to eliminate informal user access fees (Akazili et al. 2014; Mensah, Oppong, and Schmidt 2010). Under this scheme, eligible individuals enroll and pay a small premium to receive nominally free medical care, with free enrollment for particularly poor individuals (Gobah and Liang 2011). A second example comes from the country's first cash transfer program, the Livelihood Empowerment Against Poverty (LEAP), which has expanded to provide welfare transfers to eligible households across the country (Oduro 2015).

However, the state's investment in broadening the legibility of its population has lagged behind the existence of these policies which rely on state-citizen interaction to validate eligibility. This does not imply that 'illegible' citizens are strictly excluded from access, but that they instead must access public resources via mediated brokerage. While government publications articulate a need for identity documents across many sectors, Peters, Mawson et al. (2015) review several salient uses—including voter registration and accessing LEAP transfers—but conclude they are "not absolutely required—other means of establishing iden-

[21] Aryeetey and Goldstein (2000) note how such policies were conditioned by the legibility of the local population: "The government lacks the means to identify recipients on any other than coarse geographical criteria ... One challenge for future research will be to develop low-cost, effective ways to identify recipients."

tity, such as statements from traditional leaders in villages, are possible and valid." (p.21). Similarly, while birth certificates are officially required for enrolling in the biometric Ghana Card scheme, local authorities are still able to vouch for eligible citizens (Thiel 2017). Such informal channels of access are consistent with the survey results of Fridy and Myers (2019), who find that a striking share of citizens continue to depend on local authorities—primarily chiefs—in order to resolve challenges in obtaining access to public resources.

3.4.4 Regressive access and progressive taxation

As a result, while relatively poorer citizens are not formally excluded from these schemes intended to benefit them, their higher barriers to access skew the distribution of benefits. Accordingly, different nominally redistributive social policies have persistently been found to have a highly regressive incidence (Demery et al. 1995; Kwadzo and Kumekpor 1994). For example, as Figure 3.2 shows and and existing research confirms, wealthier citizens are much more likely to be enrolled in NHIS than poorer citizens (Okoroh et al. 2018).

This regressive distribution of access to the state coexists with a progressive incidence of taxation, where relatively wealthier citizens are more likely to pay taxes. The administration of taxation has historically been weakly-enforced and the collection of revenues continues to rely heavily on citizens' compliance with the state's demands (Crook 2017; Crowder 1976). In turn, citizens' compliance has been shown to be strongly conditioned by their expectations of how the funds are likely to be used (Abdul-Razak and Adafula 2013). With a relatively narrow tax base, therefore, wealthier citizens' expectations about the ability of the state to redistribute their resources likely play an important role in their tax compliance decision. Prichard (2009) discusses this elite bias and highlights how "the ability to avoid taxation gives taxpayers a policy lever that is not available to those who pay little or no tax" (p.34) by resisting reforms that threaten their economic interests. Finally, Younger, Osei-Assibey, and Oppong (2017) find that the progressive incidence of taxation, combined with the regressive incidence of state access, overall implies very little redistribution from rich to poor or reduction in inequality.

3.4.5 Implications

Putting this together, the Ghanaian context closely maps to the theoretical framework. First, we observe a persistent underinvestment in infrastructure to render the population legible even as the state increasingly administers nominally redistributive social policies. Second, we observe a highly regressive incidence of access to the state, where relatively wealthier citizens enjoy privileged access to public resources while poorer citizens rely on the mediated, often clientelistic, brokerage of local authorities. Third, we observe a limited capacity to enforce taxation and a narrow tax base, where relatively wealthier citizens comply with taxation in the expectation that their resources are unlikely to redistributed broadly.

The theoretical framework suggests that state-led investments in legibility are likely to create distributive conflict by disrupting this regressive status quo. By equalizing access to public resources, relatively wealthier citizens are likely to withdraw their fiscal and political support for the state just as the expectations and demands of relatively poorer citizens increase. Potentially rationalizing the persistent 'truncation' of nascent welfare regimes, the distributive consequences of local capacity can provide an endogenous challenge, and imply particular disincentives, to the full expansion of the state's reach.

3.5 Research design

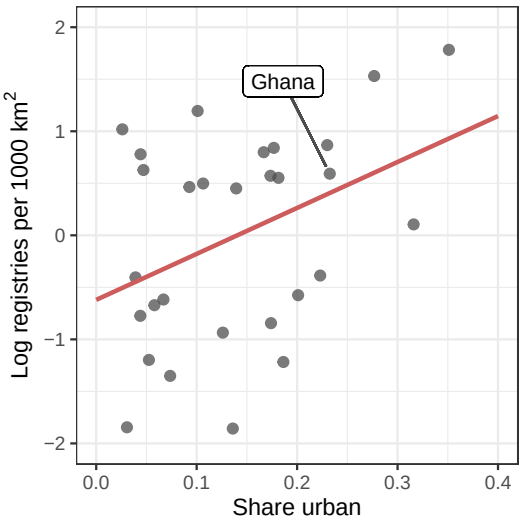
3.5.1 Inferential challenges

The theoretical framework implies a number of empirical challenges. First, local variation in legibility is likely confounded: characteristics of a given locality are likely to predict the state's supply of efforts to see its citizens as well as citizens' demands to be seen by the state. Illustrative of this, Figure 3.3 demonstrates how relatively more dense African countries, as proxied by the urban population share, have invested more in infrastructure to identify and register citizens.[22] This implies that a comparison of outcomes observed across areas with

[22]Appendix Figure B.9 demonstrates that the same pattern holds at the subnational level.

varying levels of legibility is unlikely to isolate its causal effects. Second, the theoretical framework implies that states often face weak incentives to broaden their informational reach. Empirical sources of variation in state-led efforts to increase legibility, in a sense, might then represent hard to rationalize 'off-equilibrium' behavior.

Figure 3.3: Population density and registration infrastructure in Africa



Source: International Institute for Vital Registration and Statistics (1981), World Bank. Y-axis measures standardized log number of registries per 1000 km^2. X-axis measures share urban population at the country-level in 1960.

To address the first challenge, I execute an empirical strategy which leverages quasi-random variation in the supply-side decision of the state to broaden the legibility of its population through registering its citizens. To address the second, I do this using a reform which was implemented because of external donor funding intended to help collect demographic information on citizens rather than being designed to directly affect state access, taxation, and political support. Isolating its largely unintended, but theoretically relevant, distributive consequences over time speaks to the incentives faced by states to invest in the legibility of their citizens.

3.5.2 Policy reform

In spite of issuing orders rendering civil registration compulsory almost immediately upon independence, registration rates remained relatively low for the subsequent decade. As discussed, this was mostly due to persistent underinvestment in infrastructure to facilitate citizens' registration (Peters, Mawson et al. 2015). In the mid-'70s, during Acheampong's military regime, a United Nations Population Fund (UNFPA) project initiated new efforts to register its population in order to improve the quality of demographic information available for policy planning (Mehta and Assie 1979; Serra 2018).

As part of this donor-funded project, civil registries—administrative buildings where citizens could register and receive identity documents like birth certificates—were built in a set of localities. Given the high cost of their operation and limited funds through UNFPA, these places were restricted to be relatively urban: all localities which, in the 1970 census, had above 5,000 people had a civil registry constructed between 1975 and 1976 (Mehta and Assie 1979). As a result, the number of civil registries in Ghana increased from 37 in 1965 to 256 in 1975. Figure B.10 plots the evolution in the number of civil registries over time by region, showing that most of its overall expansion happened during this period.[23] The UNFPA project documents do not motivate the construction of these registries as helping to ease citizens' access to the state through the provision of identity documents. The primary stated motivation, instead, was to solicit information on birth rates and other demographic trends. During this period, which as Figure B.11a shows well predates either democratization or the expansion of social policies, uses of identity documents were limited.[24]

We should expect that localities which had just above 5,000 population in 1970 might subsequently become more legible than those just below due to the discontinuous decrease in

[23]The largest urban centers received more than one registry, since the total number of localities with above 5,000 population was only 136 in the 1970 census. Table B.1 compares regional tabulations of the number of localities with more than 5,000 population against the distribution of urban civil registries as reported in 1975.

[24]This was compounded by the fact that most service provision had been decentralized to local authorities rather than being centrally administered (Ayee 1994).

citizens' direct cost of registering with the state. This should be interpreted as a treatment which persists through time rather than being instantaneous, since the registries continued to operate well beyond 1975 (Ghana Statistical Service 2015). In turn, this variation in the cost of registering with the state is likely to particularly matter over time as the state begins to administer policies at the individual-level, such as many social policies, through its implied reduction in state-citizen transaction costs. This naturally suggests a regression discontinuity design which leverages the population-based threshold for causal identification. I refer to localities which had above 5,000 people in 1970 as *treated*, since they later received a registry, and those which had below 5,000 as *control*, since they did not.

3.5.3 Data

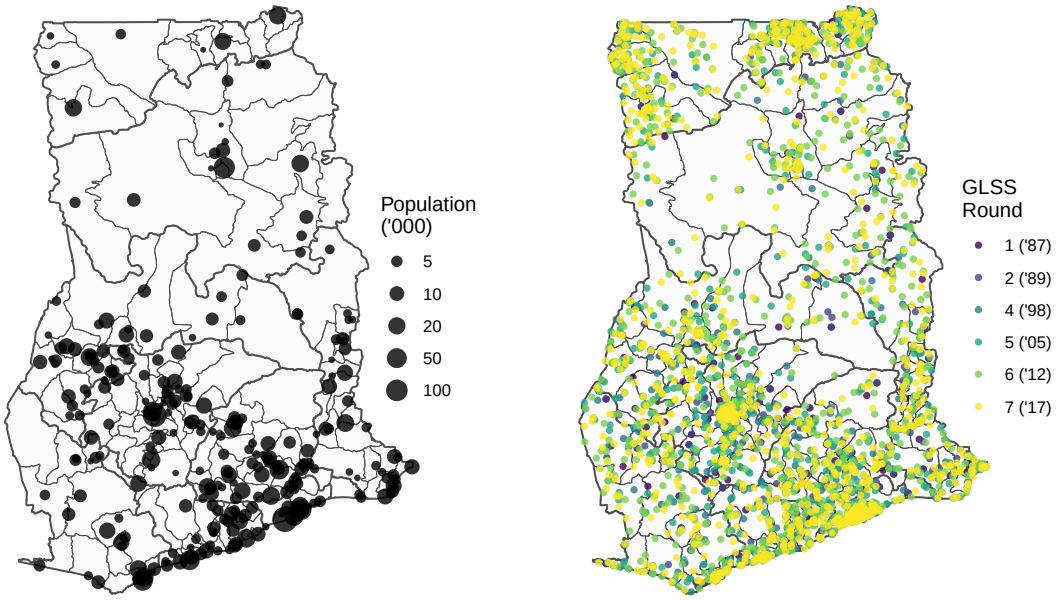
I draw on a number of archival and administrative data sources. First, I digitize archival data from the 1970 census for the basis of treatment assignment. This comprises information about all localities in the census volumes with a population above 3,000 people ($n = 275$), which together constituted a third of Ghana's population at the time. Covariates collected from the census include the age distribution of each locality, education levels, employment, agriculture, and birthplaces. I geolocate all of these localities using a set of sources.[25] I additionally digitize information on the same set of localities in the 1960 and 1984 census volumes by manually linking them. These localities are mapped in Figure 3.4a, and Table B.2 provides summary statistics for them.

Second, for the construction of the outcome measures described below, I primarily use the Ghana Living Standards Survey (GLSS), a rich nationally representative household survey that has been implemented in seven waves between 1987 and 2017. Drawing on a range of sources, I geolocate all enumeration areas from round 1 (1987), 2 (1989), 4 (1998), 5 (2005), 6 (2012) and 7 (2017).[26] This produces an overall sample of 250,000 individuals geolocated

[25]Sources for geolocation include documentation from the 1960, 1970 and 1984 censuses, the Africapolis OECD database, and the website *Ghana Place Names*.

[26]Based on correspondence with individuals involved in the implementation of round 3 (1991), it appears that those enumeration area maps have unfortunately been lost.

Figure 3.4: Data source geolocation



(a) Localities above 3,000 population in 1970 (b) GLSS enumeration areas

Thick lines represent 1970 region boundaries; thin lines represent 1970 local council boundaries.

to around 3,500 enumeration areas over a period of 30 years. Figure 3.4b plots their spatial distribution. For auxiliary outcomes I draw on geolocated Afrobarometer survey data from round 3 (2005), 4 (2008), 5 (2011), 6 (2014) and 7 (2017). The temporal span of these sources means that I can estimate the medium-term effects of the treatment and how these effects evolve over time as the state begins to administer an increasing range of policies to its citizens.

Third, to assign treatment status to respondents, I link each enumeration area (EA) from the two survey data sources to relevant localities in the 1970 census. Appendix B.1.2 provides a detailed description of this process and demonstrates robustness to varying the sample restrictions employed. In short, I restrict the outcome data to comprise only respondents in EAs which are very close to the localities which had close to 5,000 population in 1970 and exclude EAs close to major urban centers, which are *de facto* treated. This process generates a sample of 368 EAs, linked to 118 census localities which had close to 5,000 population, and contains data on over 12,000 individuals in the GLSS sample. An EA is *treated* if it is linked to a locality which had over 5,000 population, and *control* otherwise. Figure B.1 plots their spatial distribution, and Table B.3 provides descriptive statistics on the baseline survey sample.

Dependent variables

Outcomes are mostly drawn from the GLSS surveys due to its substantially larger sample size relative to Afrobarometer. First, to evaluate the effect of treatment assignment, I evaluate whether respondents possess identity documents.[27] Second, I consider outcomes relating to respondents' access to the state. I consider outcomes relating to (1) receipt of government transfers, such as through welfare transfers; (2) access to social health insurance;

[27]Every round of the GLSS surveys instructs enumerators to ask for identity documents to assess a respondent's date of birth. When the enumerator is unable to view these, their data of birth is recording as missing and only their full age in years is reported. Hence, I code a respondent as possessing identity documents when their date of birth, rather than rounded age, is reported in the data.

(3) how efficient they find their interactions with a set of state agencies.[28] For a set of placebo outcomes I consider indicators on access to local public goods relating to access to local schools and health facilities, for which access is decentralized to local authorities.

Then, to examine the consequences of this expected shock to the distribution of state-citizen interaction, I consider two groups of outcomes using outcomes from both the GLSS and Afrobarometer datasets. First, I consider effects on tax compliance using self-reported measures of tax payment and attitudinal measures. Second, I consider effects on levels of political support using measures of political engagement, such as voting and participation in local politics, and attitudes towards governance, including national-level political approval, perceived corruption, and trust. Figure B.11b presents a timeline of the temporal span of each of these main dependent variables.

Independent variables

Treatment. The primary independent variable, as described above, is whether individuals are assigned to *treated* or *control* localities based on the 1970 census. As such, the forcing variable for the regression discontinuity design is the population of the spatially linked census locality in 1970, where respondents in enumeration areas corresponding to localities which had above 5,000 population in 1970 are *treated*.

Income. The theoretical framework posits that the effects of the treatment are likely to be economically stratified. The most straightforward measure of citizens' income is simply to take measures of household consumption from the GLSS datasets at the time of survey enumeration.[29] For the Afrobarometer sample I construct a standardized index based

[28] For this latter measure I draw on a battery of questions which ask respondents about how efficient they found their interactions with a set of government agencies, and whether they had to pay bribes during these interactions. Table B.4 provides descriptive statistics on from this battery of questions by agency. I create a standardized z-score index aggregating across the different agencies and focus on the efficiency outcome in the main body of the text.

[29] Specifically, I take the standardized (log) measure of regionally-deflated total expenditure per capita in a given household, which is used by the statistics authority to compute overall welfare indices. Figure B.6 provides the distribution of this measure.

on respondents' asset ownership.[30] The obvious empirical concern is that these measures might be post-treatment biased, which would be the case if the local presence of a registry meaningfully affects individuals' aggregate consumption levels.

I address this concern in three ways. First, below I show that average household consumption is balanced at the threshold. Second, in Appendix B.1.3, across three independent data sources, I show that treatment assignment has little aggregate effect on local development over a period of multiple decades. Third, to address residual concerns,[31] I use machine learning to construct a *predicted* measure of household consumption. Appendix B.1.4 fully details this approach. In short, I compare the performance of a set of predictive algorithms which are trained on respondents in control localities using pre-treatment covariates (largely comprised of demographic characteristics). Finding a random forest to offer the best performance, I then use this model to predict consumption both within the control localities and out-of-sample in the treated localities. I report estimates using both observed and predicted income throughout.[32]

3.5.4 Estimation

I estimate the following equation to evaluate main effects:

$$y_{irtsl} = \beta_1^{RD}\mathbb{1}(\text{Pop}_{70} > 5000)_l + \beta_2^{RD}(\text{Pop}_{70} - 5000)_l + \beta_3^{RD}(\mathbb{1}(\text{Pop}_{70} > 5000)_l \cdot (\text{Pop}_{70} - 5000)_l) + \nu_r + \mu_t + \eta_s + \epsilon_{irtsl}, \quad (3.1)$$

where outcome y for individual i surveyed in region r in survey round s assigned to 1970 locality l is the outcome variable. This is regressed onto a treatment indicator, $\mathbb{1}(\text{Pop}_{70} > 5000)_l$,

[30] This comprises a index of all the assets Afrobarometer asks about, which I standardize by survey wave and by region.

[31] While *average* household consumption is balanced at the threshold, in line with theoretical expectations I find that consumption *inequality* is slightly lower above the threshold compared to below.

[32] Since the prediction exercise relies on observing a large number of pre-treatment characteristics, I am only able to implement this approach for the outcomes using the GLSS sample and not for the Afrobarometer sample.

for whether l had more than 5,000 population in the 1970 census and hence received a registry, the forcing variable defined as the deviation between l's population in 1970 and 5,000, and the linear interaction of the two. In the baseline specification I additionally include a set of fixed effects: region (ν_r), survey year (μ_t), and year of birth (η_s). While I demonstrate robustness to their exclusion, these fixed effects improve precision by respectively absorbing regional differences in access to the state, temporal differences in survey enumeration between waves, and cohort differences between individuals born in different years.[33] Standard errors are clustered at the level of the 1970 locality. Under the identification assumptions discussed below, β_1^{RD} identifies the local average treatment effect on y at the threshold.

Second, to examine heterogeneity, I estimate the following difference-in-discontinuity equation:

$$\begin{aligned} y_{irtl} = \beta_1^{DD} \mathbb{1}\big(\text{Pop}_{70} > 5000\big)_l + \beta_2^{DD}\big(\text{Pop}_{70} - 5000\big)_l + \\ \beta_3^{DD}\big(\mathbb{1}(\text{Pop}_{70} > 5000)_l \cdot \big(\text{Pop}_{70} - 5000\big)_l\big) + \beta_4^{DD}\big(\mathbb{1}(\text{Pop}_{70} > 5000)_l \cdot X_{irtl}\big) + \\ \beta_5^{DD} X_{irtl} + \beta_6^{DD}\big((\text{Pop}_{70} - 5000)_l \cdot X_{irtl}\big) + \\ \beta_7^{DD}\big((\text{Pop}_{70} - 5000)_l \cdot X_{irtl} \cdot \mathbb{1}(\text{Pop}_{70} > 5000)_l\big) + \nu_r + \mu_t + \eta_y + \epsilon_{irtl}, \quad (3.2) \end{aligned}$$

where X_{irtl}, a standardized measure of income (whether observed or predicted), and its interactions with the treatment indicator and forcing variable, is added to Equation (3.1). β_1^{DD} identifies the local average treatment effect on y among individuals for whom $X_{irtl} = 0$ (i.e. mean income) at the threshold, and β_4^{DD} estimates how this treatment effect varies among those with income one standard deviation above the mean. In the robustness tests I more flexibly estimate heterogeneous treatment effects by binning the data by income quintile and estimating Equation (3.1) within each bin.

[33] Although fixed effects are not necessary for identification in a regression discontinuity design (Lee and Lemieux 2010), in this empirical setting we should expect them to improve the precision of estimates substantially. For example, they help to soak up variation arising from differences in question phrasing between survey waves. I demonstrate the robustness of the results to the exclusion, or permutation, of the fixed effects exclusion below.

As usual for regression discontinuity designs, I restrict the sample to comprise individuals in localities which are within a given bandwidth of the population threshold. In the baseline specification I use a constant bandwidth of +/- 2,000 population and estimate each equation using OLS to accommodate the fixed effects. The relative sparsity of localities (the unit of treatment assignment) around the threshold, as Figure B.12 shows, necessitates this large bandwidth, which is slightly greater than that suggested by optimal bandwidth approaches applied to Equation (3.1).[34] The constant bandwidth, which I vary in the robustness tests for each outcome, enables me to hold fixed the effective sample irrespective of estimating equation and outcome variable.

Further, for some outcomes where I have sufficient temporal variation in the outcome variable because it appears in many survey rounds, I split the estimation by survey waves to trace out the evolution of treatment effects over time. In all tables I report the range of survey rounds for which I observe a given outcome. Last, I provide estimates for the main GLSS-based outcomes that consider both all respondents within a household as well as just considering the household head. This is both because certain variables, such as income, are only defined at the household-level, and because we should expect that if particular household members possess identity documents then the benefits could spill over to other household members.

3.5.5 Identifying assumptions

The central identifying assumption, common to regression discontinuity designs, is that a locality being just above, versus just below, a population of 5,000 in 1970 has no effect on relevant outcomes aside from through the construction of a civil registry in that locality several years later, such that right at the threshold the treatment is as-if randomly assigned.

[34]Implementing the bandwidth selection approach of Calonico et al. (2019) suggests an optimal bandwidth around 1,300 for Equation (3.1) with the GLSS sample. This bandwidth would be relatively underpowered for the heterogeneity analysis in Equation (3.2) and for analysis using the smaller Afrobarometer sample. The 'first stage' effect on the possession of identity documents remains similarly estimated around this bandwidth. Using randomization inference-based methods to estimate effects within more narrow bandwidths provides similar results (Cattaneo, Frandsen, and Titiunik 2015).

The key inferential threats typically undermining the use of such population-based thresholds relate to concerns over sorting, imbalance, and compound treatments (Gagliarducci and Nannicini 2013; Eggers et al. 2018). I consider each in turn, before addressing an additional concern relating to differential migration.

Sorting and balance

Systematic differences in either the density or characteristics of localities with just above 5,000 population in 1970, compared to just below, would suggest that the treatment assignment is non-random. To examine the possibility of sorting around the threshold I examine the density of localities around the threshold. Figure B.13 plots this density. McCrary tests fail to reject the null hypothesis of statistically identical densities on either side of the threshold using both the 1970 census data ($p = 0.44$) and the outcome data sources ($p = 0.79$). To further validate the lack of systematic differences around the threshold, I test for imbalance in locality-level covariates using the 1970 census. Table B.5 presents treatment coefficients from regressions using Equation (3.1) with every variable from Table B.2.[35] The table provides no evidence of locality-level differences in key variables nor of aggregate imbalance overall.

Compound treatment effects

The second concern is whether the same population threshold also determined the application of other policies, or the allocation of local public goods, which would then constitute a compound treatment effect. Intuitively, if this were the case, then we should observe discontinuous jumps in the presence of relevant facilities in treated localities at a subsequent point in time. To assess this possibility, I link every locality in the baseline discontinuity sample from 1970 to localities in the respective 1984 census volumes and record data on the presence of facilities relating to education, health, and water. Table B.6 presents treatment coefficients from locality-level regressions using Equation (3.1). Localities which had just

[35]These regressions only include region fixed effects since the unit of analysis is the locality.

above 5,000 inhabitants in 1970 appear indistinguishable in 1984 to those which had just below 5,000 inhabitants in 1970.[36] I repeat the same exercise using community-level datasets from the GLSS sample in Table B.7, which shows similar balance in other dimensions of the local presence of the state. I provide additional supporting descriptive information in Appendix B.1.5 that this population threshold did not coincide with thresholds used to assign different forms of local government.

Compositional differences

A final concern arises due to the temporality of the data: individual-level outcomes are observed, at a minimum, more than a decade after the reform.[37] This risks differential migration: if the treatment differentially led individuals in treated localities to move away, or people from other areas to move to the treated localities prior to the time of survey enumeration, then there would be problematic compositional differences between individuals in treated and control localities. To evaluate this concern, in Panel A of Table B.9 I assess balance on respondent characteristics using Equation (3.1) among all respondents (Columns 1-2) and restricting to household heads (Columns 3-4). The estimates suggest that respondents observed in treated localities do not systematically differ from individuals in control localities in terms of their demography.[38]

Since I observe respondents' place of birth in the GLSS data, the estimates additionally suggest that respondents enumerated in treated localities are similarly likely to have been born in that locality compared to respondents enumerated in control localities. Further,

[36]Localities are more likely to have a bore hole for their water supply, but the statistical significance of one covariate out of 18 is consistent with chance. Since only 7% of the sample uses a bore hole for access to water, the overall consequences of this imbalance are likely to be limited. Further, bore holes are typically considered to be built through community coproduction rather than central government investment (Nathan 2019).

[37]In several senses, this is beneficial: for example, given that the relevant laws governing birth registration are intended to induce registration as close to the time of birth as possible, we should expect individual-level effects, and their responses to them, to accrue through people's lives rather than immediately at the time of registration.

[38]Table B.10 provides the same for the Afrobarometer sample.

respondents have similar aggregate economic characteristics, both pre-treatment (proxied by their parents' educational status) and post-treatment (using the measures of income described above). Panel B restricts the sample to the 65% of respondents born in the locality of enumeration and finds a similar pattern of balance across all such respondents (Columns 5-6) and just restricting to household heads (Columns 7-8). Across the samples, 4 out of 56 coefficients are imbalanced at the 10% level, which is consistent with chance. In sum, there is good evidence that the empirical design causally identifies the effects of a state-led investment in legibility at the threshold.

3.6 Results

3.6.1 Effects on possession of identity documents

As a test of H1, Table 3.1 estimates treatment effects on whether individuals possess proof of identity. Column 1 estimates Equation (3.1) using the sample of respondents born inside the locality of enumeration.[39] The point estimate suggests that among all surveyed individuals born in localities which, in 1970, had just above 5,000 population, they are 21 percentage points (pp) more likely to possess proof of identity than those living in localities which, in 1970, had just below 5,000 population ($p < 0.01$). Compared to the control mean of 47 pp, this represents an approximately 40%, or 0.4 standard deviations (σ), effect size. The estimate is robust to various permutations of the fixed effects, or to the use of no fixed effects at all (column 5).[40] Figure 3.5, aggregating up to the locality-level, visually confirms the discontinuous jump in the possession of identity documents at the threshold.

I probe the robustness of these results in a number of ways. First, to ensure that the treatment effect is not a coincidental function of the population threshold, in Table B.8 I

[39]We should expect individuals who were born in the locality of enumeration to be particularly exposed to the treatment due to the laws governing birth registration: the *Births and Deaths Act* of 1965 stipulates that individuals can only obtain birth certificates in their district of their birth. I demonstrate robustness to this sample restriction below.

[40]Figure B.14 fully permutes the combinations of the three levels of fixed effects used in the baseline estimation both additively and interactively.

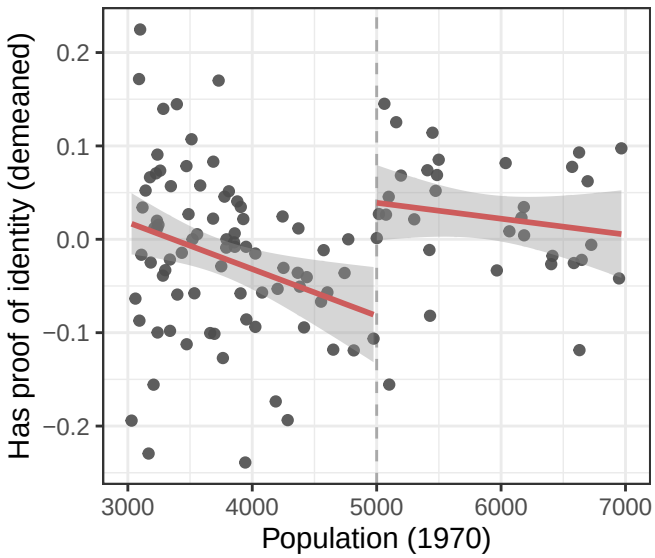
Table 3.1: Effects on possession of identity documents

	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.21***	0.26***	0.12*	0.18**	0.20***
	(0.06)	(0.08)	(0.07)	(0.07)	(0.08)
Control Mean	0.47	0.47	0.47	0.47	0.47
Region FE	✓	✓			
Survey year FE	✓		✓		
YOB FE	✓			✓	
Survey rounds	[1-7]	[1-7]	[1-7]	[1-7]	[1-7]
EAs	357	357	357	357	357
Clusters	114	114	114	114	114
Observations	7279	7279	7279	7279	7279

Outcome is an indicator for respondent possessing identity documents. 'Region' refers to regions as defined in 1970 census; 'Survey year' refers to GLSS survey round enumeration year; 'YOB' refers to year of birth of respondent.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Figure 3.5: Plot of treatment effect on possession of identity documents



Note: To match the baseline estimating equation, observations are demeaned by region, survey year, and year of birth before being averaged to the 1970 locality-level. Raw data, without demeaning, is plotted in Figure B.15.

re-estimate the treatment effects using placebo thresholds drawn from the 1960 and 1984 censuses. Second, in Figure B.16a I vary the bandwidth used for the estimation. Third, in Table B.11 I additionally include respondents *not* born inside the locality, and restrict the sample to only comprise household heads. Fourth, in Table B.12 I estimate effects by cohort of birth, separately for those born inside the locality and those born outside.[41] Fifth, in Table B.13 I estimate effects while permuting the exclusion of one survey enumeration round.

Together with the evidence for the validity of the research design, these results show that the discontinuous assignment of registries to localities in 1975 significantly increases citizens' possession of identity documents later in time.[42] Localities just above the threshold are quasi-randomly more legible at the time of enumeration than those just below.

3.6.2 Effects on access to the state

Next, to test H2a, in Table 3.2 I examine how access to the state changes at the threshold. As described above, I consider outcomes relating to access to government transfers, social insurance, and self-reported experiences of interacting with state agencies. Across each outcome, I present outcomes considering the full sample as well as just restricting to household heads.[43] The results suggest that access to the state increases discontinuously at the threshold. Individuals in localities quasi-randomly assigned a registry are 7 pp more likely to have received a transfer from the government in the past year ($p < 0.05$); 18 pp more likely to have been covered by national social health insurance ($p < 0.01$); and have found

[41] Individuals born outside the locality act as a placebo, since they are generally ineligible to register in the locality of enumeration.

[42] Even with the local assignment of a registry in treated localities, 30% of respondents still possess no legal proof of identity. This could be due to a number of reasons—losing documentation over time, the high informal costs associated with registration, or due to measurement noise. As described above, I code the possession of identity documents using the non-missingness of a specific date of birth, since the survey questionnaires indicate to leave date of birth missing if the enumerators are unable to see identity documents. However, the exact phrasing used for this question varies by round, and we should expect compliance by enumerators to be imperfect.

[43] This restriction is used because some outcomes are observed at the household, rather than individual, level.

their interactions with agencies of the state to be 0.5σ more efficient ($p < 0.05$). Across each outcome, effect sizes are substantively large compared to levels in control localities. Figure B.19 graphically plots the treatment effect for the three outcomes and Figure B.17a varies the bandwidth used in the estimation.

Table 3.2: Effects on access to the state

	A. Govt transfers		B. Social insurance		C. Experiences	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.08**	0.07**	0.18***	0.16***	0.52**	0.48*
	(0.03)	(0.03)	(0.06)	(0.06)	(0.25)	(0.26)
Sample	All	Head	All	Head	All	Head
Control mean	0.04	0.04	0.39	0.40	-0.05	-0.07
Survey rounds	[4-7]	[4-7]	[5-7]	[5-7]	[6-7]	[6-7]
EAs	328	328	300	300	234	234
Clusters	111	111	109	109	102	102
Observations	9185	4364	8854	4272	3167	2119

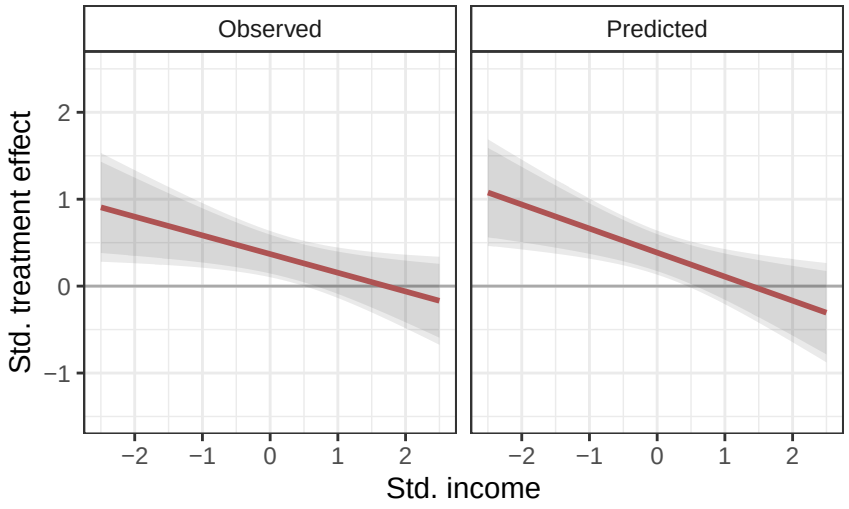
DVs: Panel A: Anyone in household has received a transfer from from government in last year, including pensions, social security, or cash transfer programs; B: Respondent has been covered by National Health Insurance scheme in the past; Panel C: Standardized z-score index of how efficient respondent has found their interactions with a set of state agencies.
$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

To probe the drivers of these results, in Table B.14 I disaggregate the government transfers outcome into the specific type of transfer received, which suggests that the effects are partially driven by increased access to cash transfers. In Table B.15 I provide evidence that respondents know more about state agencies just above the threshold than just below, and that they report paying fewer bribes during their interactions. Further, the theoretical framework posits that this variation should have more limited effects on access to local public goods, where access is typically delegated to local authorities rather than being centrally controlled. Accordingly, in Table B.16 I show that individuals in treated localities have no differences in their access to education or healthcare compared to individuals in control localities. The validity checks outlined above rule out some alternative explanations for these

results. Most importantly, I have shown that the local presence of the state—aside from the presence of identity registries—is balanced at the threshold, as are demographic and economic covariates.

Next, to test H2b, I test whether the treatment has economically stratified effects. The theoretical framework posits that the treatment should particularly affect access for the poor, who otherwise face high barriers to direct access to public resources or must rely on mediated brokerage. Using Equation (3.2), Figure 3.6 plots how the estimated treatment effect at the threshold varies with respondents' income, whether observed or predicted.[44] I use a standardized z-score index of the component outcomes from Table 3.2 as the dependent variable.

Figure 3.6: Marginal effects plot of treatment effects on access to the state



90% and 95% confidence bands plotted. Outcome is a standardized z-score index of the outcomes used in Table 3.2. Plots vary in whether they use an *observed* or *predicted* measure of income (see Appendix B.1.4).

The results suggest that the increases in access are particularly concentrated among citizens with lower levels of income: the estimated treatment effect decreases with either measure of income, and becomes insignificant for respondents with income more than 0.5σ above mean levels. Table B.17 shows that the coefficient on this interaction term is sig-

[44] As explained above and described in detail in Appendix B.1.4, I address concerns about income being post-treatment using a machine learning-based prediction exercise.

nificantly negative ($p < 0.05$). The left panel of Figure B.18 varies the bandwidth used in the estimation and the top row of Figure B.20 flexibly estimates treatment effects by income quintile, suggesting that individuals in the highest income quintile have significantly decreased access. To provide additional descriptive support for these results, in Figure B.21 I nonparametrically plot observed income against the probability of the household receiving transfers from the government. This suggests that access to transfers is *regressive* in control localities (i.e. the rich are more likely to receive transfers than the poor), but *progressive* in treated localities.

3.6.3 Distributive consequences

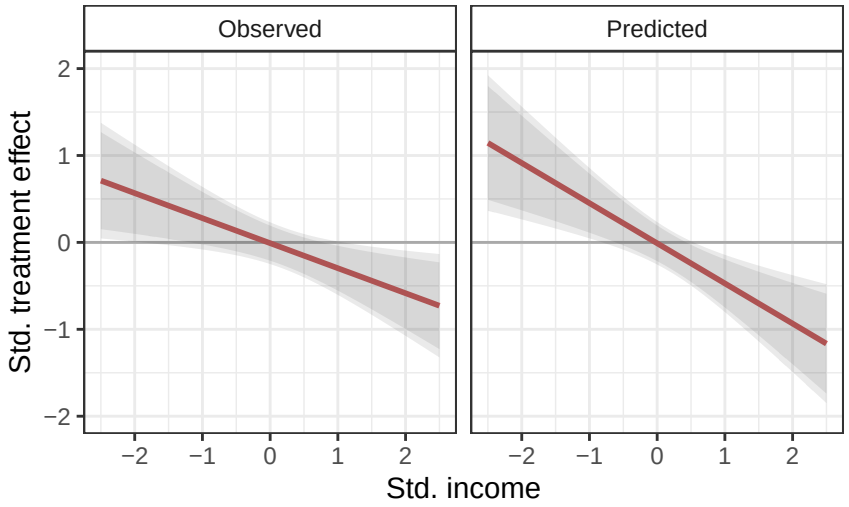
These results suggest that increases in legibility, by reducing state-citizen transaction costs, facilitate increased access to resources which are administered through state-citizen interaction rather than via mediated brokerage. These benefits are particularly concentrated among relatively poorer citizens. In line with the theoretical framework, I consider how this shock to the distribution of access to the state affects two different dimensions of governance outcomes: first, the extraction of taxation; second, the consolidation of political support.

Taxation

As a test of H3, I examine whether patterns of tax payment appear different above the threshold compared to below. Using a standardized continuous measure of self-reported household tax payment as its outcome, and estimated using Equation (3.2), Figure 3.7 estimates the treatment effect and how it varies with respondents' income level. The plot suggests that the *average* estimated treatment effect—i.e. among citizens with standardized mean zero income—is close to zero. This is visually confirmed by Figure B.22, which provides little evidence of discontinuous average changes at the threshold.

On the other hand, the plot suggests notable effects on the *composition* of citizens paying taxes above the threshold compared to below. Focusing on the plot using observed income, the treatment effect is significantly negative at the 95% level among individuals with income

Figure 3.7: Marginal effects plot of treatment effects on tax payment



90% and 95% confidence bands plotted. Outcome is a continuous (log+1) measure of how much household paid in taxes (central or local) last year. Plots vary in whether they use an *observed* or *predicted* measure of income (see Appendix B.1.4). Figure B.23 provides equivalent plots using a binary measure of payment.

more than one standard deviation above the mean and positive among individuals with income below the mean. Table B.18 shows that these interaction coefficients are statistically significantly negative ($p < 0.05$). The middle panel of Figure B.18 varies the bandwidth used in this estimation and the second row of Figure B.20 flexibly estimates treatment effects by income quintile. Descriptively, as Figure B.21 illustrates, in control localities the incidence of taxation increases with income while in treated localities it is relatively flat.

Informed by the theoretical framework, there are two explanations for why increased legibility might inhibit the supply of taxation by relatively wealthier citizens. First (1), by rendering access to the state more progressive and threatening their privileged access to resources, it might crowd out the willingness of the rich to quasi-voluntarily comply with demands for taxation. Second (2), by reducing the authority and discretion of informal local authorities, the coercive efficacy of tax collection might be reduced. I provide three pieces of evidence which together suggest that (1) is more responsible for the observed effects than (2).

First, I examine the temporal evolution of these treatment effects. Prior to the rollout

of the policies I consider above, access to the state remained more regressive in treated localities. To be consistent with (1), then, we should expect that the treatment effect on tax payment among wealthier citizens is particularly negative in the presence of these recently implemented policies. To test this intuition, using the fact that questions about tax payment are asked in GLSS rounds which predate the expansion of these policies, in Table B.19 I split the sample into survey waves corresponding to 1998-2005 versus 2012-2017. The results show that the negative treatment effect on tax payment among the wealthy at the discontinuity is only present in more recent waves of the survey. The gradual development of these effects suggests that increased access to the state among the poor has spillover effects on behavior by the rich.

Second, I evaluate how attitudes towards tax compliance vary at the threshold. To be consistent with (1), we should expect that tax morale and norms of compliance are weaker among relatively wealthier citizens. The results in Table B.20 use outcomes from the Afrobarometer sample relating to citizens' beliefs over whether it is justifiable to avoid paying taxes. These demonstrate that citizens in localities just above the threshold are significantly more likely to consider it wrong to avoid paying taxes, but that this treatment effect disappears among citizens at higher levels of income. Instead, at the threshold, relatively wealthier citizens are significantly more likely to believe that it is *understandable* to avoid paying taxes and less likely to believe that it should be *punishable*.

Third, I consider whether the effects are attributable to changes in the authority of local authorities. In Table B.21 I use a battery of outcomes relating to traditional authorities in the Afrobarometer sample, comprising their perceived trustworthiness, engagement in corruption, approval towards them, and their influence. The estimates suggest that while relatively wealthier citizens trust traditional authorities slightly less at the threshold, the other outcomes provide only weak evidence of negative treatment effects at the threshold for this group.[45] By contrast, as I show below, attitudes towards formal governance institutions

[45]While the interaction term in Panel C is weakly significantly negative ($p < 0.1$), because the main effect is weakly positive, the treatment effect only becomes significantly negative at extremely high levels of income.

strikingly worsen at the threshold.

In sum, these results are primarily consistent with the compliance of relatively wealthier citizens with demands for taxation decreasing in response to losing their preferential local access to public resources. The fiscal response from broadening the state's informational reach is negative among the citizens who otherwise benefit from the regressive status quo.

Political support

With these results suggesting that the state might not *fiscally* benefit from the increased legibility of its population, I next consider the extent to which it *politically* benefits. First, as a test of H4, I examine how measures of political engagement vary at the threshold. In Table 3.3, I use Equation 3.1 to estimate effects on outcome variables relating to self-reported voting in the last election, participation in local meetings, and attendance of political rallies. These results provide evidence that citizens are more politically engaged just above the threshold compared to below. Respondents are significantly more likely to report voting in the last election and to report participating in local political meetings ($p < 0.01$), and insignificantly more likely to report participating in rallies. Figure B.24 graphically plots the treatment effect for the three outcomes and Figure B.17b varies the bandwidth used in the estimation.

Next, I show that these treatment effects are again concentrated among relatively poorer citizens. In Figure 3.8 I use a standardized z-score index which aggregates the outcomes from Table 3.3 to estimate the difference-in-discontinuity specification (Equation 3.2). The plot, supported by the underlying regressions presented in Table B.22, demonstrates that the treatment effect significantly decreases with respondents' income levels ($p < 0.01$) and becomes significantly negative at the very highest levels of income. The right panel of Figure B.18 varies the bandwidth used in this estimation and the bottom row of Figure B.20 flexibly estimates treatment effects by income quintile, showing strongly positive effects for the poorest quintile.

In principle, increased participation could result *directly* from increased legibility—e.g.

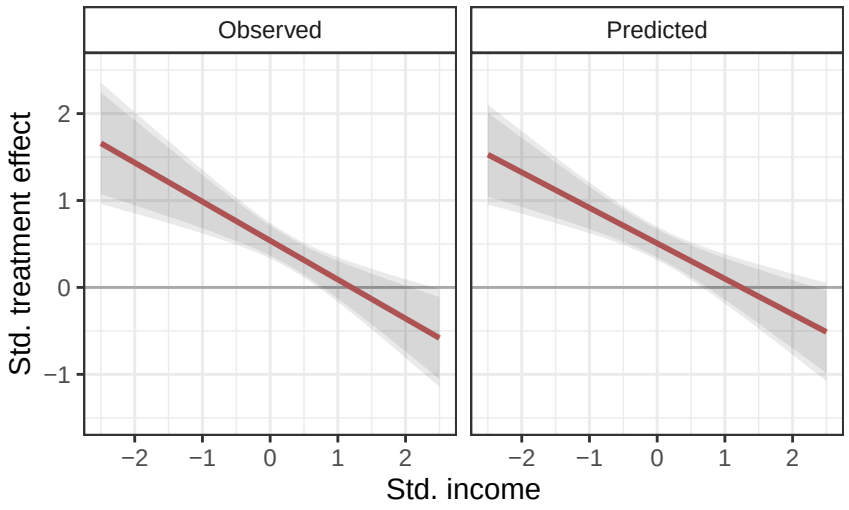
Table 3.3: Effects on political engagement

	A. Voted		B. Local meetings		C. Rallies	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.06***	0.05**	0.32***	0.29***	0.09	0.10
	(0.02)	(0.02)	(0.08)	(0.09)	(0.06)	(0.07)
Sample	All	Head	All	Head	All	Head
Control mean	0.89	0.91	0.31	0.33	0.26	0.26
Survey rounds	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]
EAs	234	234	234	234	234	234
Clusters	102	102	102	102	102	102
Observations	3166	2119	3166	2119	3166	2119

DVs: Panel A: Respondent voted in last election; B: Respondent has attended local meetings in the past three years; Panel C: Respondent has attended political rallies or protests in the past three years.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Figure 3.8: Marginal effects plot of treatment effects on political engagement



90% and 95% confidence bands plotted. Outcome is a standardized z-score index of whether respondent voted in last election; attended local meetings in last three years; and attended political rallies in last three years. Plots vary in whether they use an *observed* or *predicted* measure of income (see Appendix B.1.4).

citizens can use identity documents to register to vote—or *indirectly* via increased access to state service provision and reduced reliance on intermediation. While difficult to entirely adjudicate between these channels, I provide some evidence supporting each one. First, for the direct channel I use a set of survey questions about why respondents failed to vote. Table B.23 shows that respondents in treatment localities are significantly *less likely* to cite identity verification problems when seeking to cast their vote, consistent with the treatment having direct effects on the cost of participation. Second, for the indirect channel, I provide some evidence consistent with citizens' political expectations and allegiances being affected. Consistent with a shift in political allegiances, Table B.24 shows that respondents feel freer to vote without feeling pressured. Consistent with increased expectations, Table B.25 shows that poorer citizens associate a larger number of economic and political rights, such as the provision of jobs and protections of political rights, with democracy in Ghana.

As a result, then, the quasi-random local variation in state-citizen informational linkages induces a reduction in tax payment by relatively wealthier individuals over time and an increase in the political engagement of relatively poorer individuals. I consider net effects on levels of political support and attitudes towards governance, examining the intuition of H5: that the treatment simultaneously increases exposure to the demands of relatively poorer citizens while also plausibly inhibiting its fiscal ability to meet them. As a result, with sufficiently unsatisfied expectations, *both* groups have reason to disapprove of their governance.

For this, I turn to the Afrobarometer sample and consider three standardized index outcomes. These relate to respondents' perceptions of government performance in providing services and addressing economic issues; perceptions of the extent to which different national political institutions—primarily elected political figures—engage in corruption, and perceived trust in these institutions. Table 3.4 provides results and Figure B.25 graphically plots the main treatment effect for the three outcomes and Figure B.17c varies the bandwidth used in the estimation.

The results suggest that respondents in treated localities are less satisfied with the quality

Table 3.4: Effects on attitudes towards governance

	A. Performance		B. Corruption		C. Trust	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	-0.32**	-0.36**	0.36***	0.38***	-0.03	-0.02
	(0.16)	(0.15)	(0.13)	(0.13)	(0.16)	(0.17)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		0.01		-0.08		-0.10
		(0.13)		(0.15)		(0.13)
Survey rounds	['05-'17]	['05-'17]	['05-'17]	['05-'17]	['05-'17]	['05-'17]
EAs	174	174	174	174	174	174
Clusters	71	71	71	71	71	71
Observations	1392	1392	1392	1392	1392	1392

DVs: Standardized index of (A) Perceptions of government performance in providing services and addressing economic issues; (B) Perceptions of levels of corruption across different political instutitons; (C) Perceptions of trust in different political institutions.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is a measure of observed income based on asset ownership (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

of their governance overall. Panel A shows that respondents, on average, approve 0.32σ less of government performance ($p < 0.05$), and that this does not vary by respondents' income levels. Panel B shows that respondents in treated localities, consider national political institutions to engage in corruption 0.36σ more so than respondents in control localities ($p < 0.01$), even as they consider their own interactions with state agencies to be more efficient. Panel C suggests overall null effects on trust in political institutions, even as most respondents receive increased access to the state.

I disaggregate the results in Table B.26 by splitting each index into its components. These results suggest relatively broad negative effects on attitudes towards the state, whether in terms of performance on services versus economic issues, or perceptions of corruption among elected and non-elected political figures. Again, these results provide little evidence of heterogeneity by income level. In sum, respondents in localities where the state's informational reach is more even are notably *less* satisfied by the quality of their governance. Supply-side investments that broaden the informational reach of the state, then, may fail to incur either fiscal or political returns.

3.7 Conclusion

The inability of many developing countries, particularly those in Africa, to effectively collect taxes and to uniformly implement policies is typically thought to be a problem of limited state capacity. This paper has argued that distributive conflicts within society can also undermine the effective and universal implementation of policies when the state-led development of new capacities alters gains across economic strata. Since shifts in local capacity imply distributive effects which can lead to political disappointment and discontent, the benefits states accrue from investing in the legibility of the full population are weaker than otherwise assumed. As a result, what the state knows about its citizens often lags well behind the existence of policies nominally intended to target, and benefit, them. Weak incentives—and not just limited resources—help to explain observed, durable inequalities in access to the state.

While ethnic inequalities dominate the study of redistributive politics in Africa, this paper suggests that state-led efforts play a crucial role in shaping class-based economic inequalities. The theoretical framework posits that the state's truncated reach is underwritten by a complementarity: facing high barriers to interacting with the state, relatively poorer citizens must rely on local authorities to broker access; while relatively wealthier citizens enjoy privileged access to resources and in exchange are more likely to comply with the state's demands for taxation. As the empirical results confirm, disrupting this regressive status quo has stratified effects: it crowds out the quasi-voluntary compliance of the wealthy, while it increases the demands and expectations of the poor. Together, citizens overall are more disappointed in their governance in localities where the state's informational reach is broader. Local variation in legibility then has important effects on who, and how, citizens can make claims on public resources.

Both theory and results have a rich set of implications for understanding variation in incentives to invest in such capacity, both across and within countries. First, it suggests that the sequencing of state-building matters: the status quo, which often regressively benefit

relatively wealthier citizens, creates dynamic challenges to the full expansion of the state's reach. In such settings, which are common across developing countries, the implementation of nominally progressive policies *before* the expansion of the state's capacities limits their universalizing potential.

Second, it suggests that states which are relatively insensitive to the expectations of their citizens might also be better positioned to invest in technologies which facilitate more effective service delivery. This provides a different logic, rather than citizen surveillance, for why relatively less democratic countries have invested more intensively in modern biometric technologies to identify and register their citizens. Third, it suggests that states which are able to make complementary investments in the coercive enforcement of taxation, rather than relying heavily on citizens' instrumental compliance, are less likely to incur the distributive consequences which undermine these investments.

More generally, the weak domestic incentives that states often face to develop such informational capacity highlights the important role of international agencies in pushing for sustainable reform. Just as the United Nations Population Fund was responsible for the reform I leverage in this paper in the '70s, donors including the World Bank and UNICEF play an outsized role in advancing the biometric "identification revolution" sweeping across many developing countries today (Gelb and Metz 2018). These projects, consistent with my results, are frequently rationalized as helping to improve service delivery to otherwise excluded groups. While this policy impetus has led to the announcement of many technologically sophisticated schemes, their enduringly incomplete expansion to the full population highlights the relevance of several distributive forces outlined in this paper. Ensuring that states sustainably expand their reach while minimizing the incidence of these distributive conflicts is an important avenue for future work. Future research which parses these relationships—between political incentives, policy implementation, and the distributive implications of state capacity—is needed to shed light on the sustainable expansion of the state's reach across its territory.

4 | Building the Biometric State: Citizen Response to a State-Building Intervention in Uganda[1]

4.1 Introduction

Classic studies underscore how citizens' resistance to intrusion hinders the consolidation of the state's control over its territory. In the influential work of Migdal (1988), for example, states remain weak because citizens' allegiances to local social structures—the clan, chiefdom, or tribe—hinder states' efforts to acquire social control.[2] When the state becomes stronger than society, citizens either become subject to despotism (Acemoglu and Robinson 2020; Mann 1984) or are forced to continually evade the centralizing efforts of the state (Scott 2009). Intrusive state-building efforts, then, must succeed in spite of citizens' limited demand: citizens in strong societies do not want to be seen by the state (Scott 1998).

This paper examines the extent to which this intuition holds true and hence, implicitly, the extent to which limited investments in state capacity tend to represent a "demand-side" problem. I suggest that prior literature has understated the extent to which these seemingly intrusive efforts, ex ante, represent ambiguous signals for citizens. This potentially owes to the largely historical nature of studies on state-building, especially in more developed country settings, which more easily study the downstream policy consequences of capacity

[1]With thanks to Torben Iversen, Horacio Larreguy, Evan Lieberman, Pia Raffler, Emily Sellars, and Jon Weigel for feedback, and audiences at APSA, ASA, and Harvard. With thanks to Sam Olweny for assistance with the data, and to staff at the Electoral Commission and National Identification and Registration Authority.

[2]Migdal (1988) defines social control as "the currency over which organizations in an environment of conflict battle each other" (p.32), constituted by citizens' compliance with state demands; the state's legitimation; and citizens' voluntary use of state institutions.

investments rather than the initial ways in which citizens interpret, and update in response to, signals of a stronger state (Soifer 2016).

Building on the Weberian perspective of state capacity as the ability of the state to implement policies and enforce rules (Besley and Persson 2009; Geddes 1994), recent work broadly disaggregates state capacity into its *extractive* and *productive* dimensions—that is, how capacity is constituted by resources which facilitate either extracting from, or supplying welfare to, citizens (Berwick and Christia 2018; Hanson and Sigman 2021).[3] One of the key resources which underlies both dimensions is information: what the state knows about its citizens conditions the implementation of a wide set of policies (Lindvall and Teorell 2016). The centralizing efforts of states to solicit information from citizens—take, for example, the enumeration of a census—could therefore signal either the future extractive intentions of the state or its ability to supply welfare for its citizens (Brambor et al. 2020). Understanding which signal dominates is a challenging empirical question since it is difficult to parse citizens' response to a (potentially ambiguous) state-building investment from their response to its realized policy consequences. Taking one example, recent studies on taxation suggest both that citizens might update positively in response to *efforts* to tax them (Weigel 2020) but negatively in response to *actually being taxed* (Christensen and Garfias 2021).

In the context of modern Uganda, an electoral authoritarian regime, I study the rollout of biometric identity cards in 2014-15 and leverage a research design which enables me to isolate the short-run effects of this state-building investment on citizens' beliefs and allegiances. I characterize this rollout as intrusive and centralized, with the army sidestepping local political structures to extract biometric data from citizens, but also ex ante welfare-ambiguous from the citizen's perspective, with leaders declaring that solicited "Data will be used for taxation, public administration and many other functions" (*Registration of Persons Bill*, 2014). In the context of a relatively coercive state, the scheme captured data from nearly all of the country's population within six months.

[3]Berwick and Christia (2018) disaggregate capacity into extractive, coordination, and compliance dimensions; Hanson and Sigman (2021) disaggregate capacity into coercive, extractive, and administrative dimensions.

Empirically, I leverage the fortuitous timing of a social survey administered during the staggered card issuance process to generate *spatial* variation in citizens' exposure to the state-building intervention. I combine this with *temporal* variation, in a difference-in-differences framework, to estimate citizens' response.[4] The results suggest that citizens update strikingly positively in their perceptions of the authority and legitimacy of the state—for example, considering it more important to obey the law and pay their taxes. Parsing the competing signals of state capacity, I find strong evidence that citizens construe the rollout as a productive signal of economic modernization and little evidence that citizens weigh its extractive consequences for freedom and coercion. The treatment effects are relatively stable across political and economic subgroups and are not driven by social desirability bias.

In what constitutes a relatively challenging case, the evidence suggests that citizens do not hide from the seemingly intrusive efforts of the state to see them. Rather, citizens' attitudes and expectations appear positively affected by, and relatively elastic to, such schemes. Although citizens might initially *demand* a stronger state, using both difference-in-differences and cross-sectional evidence I provide suggestive evidence that they become disappointed over time by the realized policy consequences, or lack thereof, of the state's increased informational resources. Specifically, I demonstrate that citizens' positive updating dissipates as the rollout proceeds; that over a longer time period, citizens update negatively about their perceived freedoms; and that qualitatively, modernizing promises of digitalizing service provision are unmet by the realities of interacting with the bureaucracy to make claims on public resources. Together, this implies that such investments are importantly constrained on the "supply-side" by the state's sensitivity to the (possibly disappointed) expectations of its citizens, and hence more likely to be feasible where political competition is weak (Garfias and Sellars 2021; Gottlieb 2019).

[4]Importantly these identity cards, at the time of issuance, had no concrete use or policy consequences and only later became important for exercising claims on public resources.

4.2 Interpreting signals of state capacity

4.2.1 Citizens' response to state-building

Minimalist definitions, in the Weberian tradition, define the capacity of the state as its ability to implement policies and rules (Besley and Persson 2009; Geddes 1994). The sources of its capacity, accordingly, constitute the state's available resources, whether material or social, which facilitate particular forms of policy implementation (Lindvall and Teorell 2016). Acquiring these resources is generally construed as an interactive, generative process where the state competes for citizens' allegiances and obedience against social institutions (Migdal 1988; Soifer 2008). Efforts to unilaterally overrule these alternative institutions, especially absent the state's ability to penetrate civil society, tend towards despotism (Acemoglu and Robinson 2020; Mann 1984).

Studies of state-building interventions most often focus on the extent to which such schemes facilitate *extractive* policies, by which states can monitor, coerce, and surveil their populations to secure resources (Berwick and Christia 2018).[5] These studies emphasize how the development of the state's extractive capacities, such to facilitate taxation, creates losers: citizens become subjected to the state's extractive whims (Migdal 1988) while local social institutions are undercut by the state's centralizing aims (Garfias and Sellars 2021). Scott (2009), in his influential account, describes how citizens then decide how to evade the capricious state as best they can. Together, they imply that the expansion of the state's extractive capacities confronts a "demand-side" problem: the state expands its reach in spite of citizens' resistance.

The latent resources which ultimately facilitate the extractive policies of the state often, ex ante, have ambiguous implications for citizens' welfare. That studies of citizens' response to state-building interventions have tended to emphasize the resistance of citizens and social institutions, perhaps, owes to their historical focus which has led scholars to study citizens'

[5]Extraction, illustratively, is a near universal feature of recent work which disaggregates the dimensions of state capacity (Berwick and Christia 2018; Hanson and Sigman 2021).

response to the *policy consequences* of capacity investments rather than the investments themselves (Soifer 2016). Because such resources—as Lindvall and Teorell (2016) categorize them, information, finances, and human resources—are just as easily deployed for *productive* purposes of delivering welfare as they are for *extractive* purposes, state-led efforts to develop these resources represent mixed signals for citizens. Efforts to build a land cadaster, for example, might simultaneously signal the future extractive ability of the state to tax its citizens as well as its improved productive ability to supply and target public goods (D'Arcy and Nistotskaya 2017).

4.2.2 Ambiguous signals and unmet expectations

This ex ante ambiguity suggests that citizens' initial expectations about the policy consequences of state-led investments are likely to be uncertain, particularly when facing novel technologies of state control. Historical evidence demonstrates the ways in which these uncertain expectations can be manipulated by state actors to induce compliance even for interventions which ex post facilitate extraction and predation.

The literature on African census administration in the colonial era, for example, underscores its deep fiscal association: the colonial state counted citizens to tax them (Kuczynski 1948; Ittmann, Cordell, and Maddox 2010). Postcolonial states, inheriting populations wary of enumeration (Shaul 1952), sought to reshape citizens' expectations about *why* the state needed to count them. Focusing on the Ghanaian census of 1960, Serra (2018) describes these efforts, showing how "for the newly independent states, the collection, construction, and dissemination of statistics came to be seen as an important precondition of economic and social modernization" (p.665). To reshape citizens' beliefs about the policy consequences of supplying information to the state, *even though* the census continued to be used to inform tax administration, the Nkrumah regime enlisted teachers and students to persuade the broader population that enumeration would instead lead to economic modernization and the future provision of public goods (Jerven 2013).

Recent evidence from sub-Saharan Africa underscores how citizens' short-run responses

to state-led investments are both relatively elastic and generally positive. Weigel (2020), for example, shows how efforts to tax citizens induce citizens to update their beliefs about the utility of engaging with the government; Croke (2021) shows how citizens positively respond to signals of welfare provision by the state; and Gottlieb (2019) shows how state-led interventions increase citizens' expectations of programmatic service delivery. This contrasts with evidence on how citizens's perceptions of the state worsen over a longer time horizon, as the realized consequences of these investments for policy implementation are realized (or fail to do so). Illustratively, evidence from sub-Saharan Africa suggests that citizens over time express more negative views about the the state in localities where the state's presence is higher (Brinkerhoff, Wetterberg, and Wibbels 2018; De Kadt and Lieberman 2017).

Together, this suggests that the issue is not that citizens typically seek to hide from the state. Rather, because the ex ante welfare implications of even intrusive state-building schemes are often ambiguous, citizens' fungible expectations are raised by state actors in order to induce their compliance. Over time, if these heightened expectations are unmet by the reality of subsequent policy implementation, citizens become disappointed and disillusioned. Consistent with the Tocquevillian logic of reforms prompting political contestation (Finkel and Gehlbach 2020; Healy, Kosec, and Mo 2017), this suggests that states' sensitivity to the ex post disappointment of citizens shapes their initial "supply-side" incentives to invest in resources which strengthen their capacities.

4.3 Technologies of biometric identification

Recent years have seen the rapid expansion of biometric identification systems in developing countries, described by some authors as an "identification revolution" with nearly half of all sub-Saharan African countries having introduced such a scheme since 2010 (Gelb and Metz 2018). As a result, and often heavily supported by donor efforts, today many of the world's most sophisticated technologies to identify and register citizens are to be

found in the world's poorest countries.[6] While the effective coverage of these schemes varies significantly—from 15% in Malawi to 95% in Botswana (Demirguc-Kunt et al. 2018)—their rapid diffusion across the region represents a potentially significant innovation in the capabilities of the state (Gelb and Clark 2013).[7]

As Breckenridge (2014) argues, identification technologies "lie at the heart of the work that the state arrogates to itself" by shaping the information available to the state and its bureaucracy. These technologies, then, represent basic investments in the state's informational resources available for policy implementation—whether towards *extractive* or *productive* ends (Lindvall and Teorell 2016; Soifer 2008).[8]

Along the extractive dimension, schemes seeking to bolster states' informational capacities enhance their ability to monitor and coerce their populations (Lee and Zhang 2017; Scott 1998). Cooper (2002), for example, demonstrates how such technologies have historically been linked to coercion while Breckenridge (2014) and Posel and Dubow (2000) document the ways in which the South African state has employed biometric technologies to surveil its population over time. Increasing reliance on formal documentation, in turn, has been argued to represent a form of *structural violence* perpetrated upon the poor by facilitating their exclusion (Gupta 2012). Debates, largely confined to developed country settings, focus on citizens' fears about the encroaching reach of the surveillance state (Gelb and Clark 2013; Ferguson 2015).

Along the productive dimension, in low-information settings, the provision of welfare is rendered challenging by difficulties in observing individuals' eligibility for accessing partic-

[6] As Breckenridge (2014) notes, "it is still incongruous, in the light of the wider scholarship on the new surveillance state, that the most powerful biometric surveillance systems are being developed in the poorest countries."

[7] The drivers of this rapid shift are multifaceted: the one-off administrative costs of biometric rollouts are far lower than for more traditional civil registration technologies (Gelb and Clark 2013); a significant international epistemic community has formed around the issue (Haas 1992); and the increasing salience of welfare regimes in developing countries has increased governments' returns to being able to know and identify its citizens (Honorati and Yemtsov 2015).

[8] Justifying the aggressive expansion of a Kenyan scheme to gather fingerprint data, one Kenyan official underscored the value of the project because "Data is the new oil" (*Al Jazeera*, 2020).

ular public goods and services (Barrientos 2013; Ferguson 2015). In turn, existing research demonstrates how these novel technologies can increase the delivery of welfare transfers and reduce opportunities for corruption in service delivery (Muralidharan, Niehaus, and Sukhtankar 2016). Beyond social assistance, supplying biometric information to the state through these schemes increasingly shapes the ability of citizens to easily make claims on the state (Manby 2013). More broadly, technologies of registration have been linked to improvements in security and crime prevention (Caplan and Torpey 2001) while signalling the developmentalist ambitions of the state (Mkandawire 2001).

4.3.1 Ugandan biometric rollout

In spite of their rapid expansion, there exists relatively little systematic evidence on how citizens construe the uncertain implications of these capacity investments for future policy implementation—as Breckenridge and Szreter (2012) write, "the political and economic effects of the relatively cheap and efficient biometric registers still have to be assessed." I provide such evidence focusing on the highly intensive rollout of biometric identification technologies in Uganda, a canonical electoral autocracy (Tripp 2010).

The Government of Uganda initiated its ambitious scheme aiming to biometrically register citizens in order to tackle a host of governance issues—including electoral administration, tax collection, and crime reduction—rendered challenging by the state's difficulties in identifying and monitoring citizens.[9] After initial attempts in 2012 collapsed due to procurement irregularities, a group of government agencies under the Ministry of Internal Affairs was constituted to implement the National Security Information Systems (NSIS) process following the passage of the *Registration of Persons Bill* in 2014.[10] This bill made clear the multifaceted implications of the scheme, suggesting that "Data will be used for taxation, public

[9]Doyle (2012) notes that citizens' ability to strategically deny, or change, their stated identity to state agents was a primary motivation in the Ugandan case: "the frequency with which individuals changed their names ... explains to a large extent the current enthusiasm of African governments for biometric systems of population registration."

[10]As one MP noted, "billions of shillings have gone down the drain and we had nothing on paper. I am glad that at last, we are now discussing results and we are seeing progress on this important matter."

Figure 4.1: Issuance Rollouts (Arua and Masindi Districts)

administration and many other functions." The coalition of government stakeholders was subsequently institutionalized into the National Identification and Registration Authority (NIRA) which maintains formal responsibility for maintaining the national identity register.

The implementation of the biometric registration scheme had demonstrably coercive aspects. The rollout, executed without significant donor funding support, was primarily implemented by the Uganda People's Defence Force (UPDF), the national army, under command of the central state.[11] General Aronda Nyakairima, in charge of the initial rollout, characterized the investment as "a way to monitor and know where people are. It is another element to be added on to our arsenal of security weapons" (*The Report*, October 13, 2015). In practice, the rollout comprised a *registration* phase and an *issuance* phase. First, in the *registration* phase from April to September 2014, army-affiliated groups traveled from parish to parish across the country to comprehensively solicit information and biometric data from residents. Second, in the *issuance* phase from December 2014 to September 2015, similar groups of army officials returned to each parish across the country to deliver national identity cards to registered citizens.[12] Figure 4.1 depicts a few such events.

[11]Indicative of its continued ties to security forces, the National Identification Registration Authority (NIRA) remains based at a military base in central Kampala.

[12]All adults were eligible to register, which was free. Citizens were required to register at their place of residence, and identity verification was normally performed either using alternative documents or through the

4.3.2 Evidence from parliamentary debates

The clearly extractive implications of the rollout are well-documented by parliamentary debates which took place at the time. Both phases of the rollout were heavily centralized and largely sidestepped local political authorities.[13] As one MP noted in a parliamentary debate on the topic, "When you look at the structure laid down, the personnel in this structure are people from the security. [...] You left out the good players like the teachers, religious leaders and even we, Members of Parliament, are not part of this structure. Why don't you involve these people? Even the civil society was excluded." (*Hansard*, 29 July 2014). Another noted that "the Army has actually secretly taken over without announcing this. [...] There is no collaboration—the Army is bulldozing others." (*Hansard*, 29 July 2014).

The potentially negative implications of the scheme for citizens' exclusion and surveillance were also quickly noted by representatives—"It was done by the top level, collected by intelligence and security operatives. They will use that data in any way they need and differentiate some people" (*Hansard*, 3 February 2015), and "the moment you get this exercise centralised, I am sure you will have disenfranchised many citizens" (*Hansard*, July 29 2014). Last, political leaders clearly understood that the scheme might help facilitate the extraction of taxes: "Many people don't pay taxes in this country because we don't even have data about them. With this exercise ... it would be very easy to know that this person has carried out this transaction, the money is in the bank, you just have to use this identity card and see how you can generate revenue." (*Hansard*, July 29 2014).

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verification of a Parish Citizenship Verification Committee comprised of local leaders. Additional verification was completed by the Central Citizenship Verification Team at Kololo, a Sub-country Review Committee, and a National Review Committee. Many challenges with verification were nonetheless reported in cross-border regions, particularly with Kenya and South Sudan.

[13] While village leaders such as LC1 chairmen were often used to verify the eligibility of individuals seeking to enroll in the scheme, they had little other involvement.

4.3.3 Ex ante uncertainty and ex post exclusion

The intrusive effort by the Ugandan state to register its population closely maps onto behavior by the the despotic states feared by authors like Scott (1998) and Mann (1984). A natural implication of this seemingly despotic behavior would be that citizens seek to avoid being seen by the state when confronted by such an effort. However, while historical examples do exist of local groups effectively hiding from the state (Doyle 2006), in this instance the state was remarkably successful at registering citizens: NIRA reports that 99% of eligible citizens had been registered within a year, while social survey data suggests that 95% of adults had registered for a biometric identity card within a few years (Afrobarometer 2017).

While these high compliance rates surely partially reflect the coercive manner of the registration and issuance process, they also likely owe to citizens' ex ante beliefs that enrolling in the scheme would facilitate their access to welfare. The state aggressively employed developmentalist rhetoric, akin to the propaganda studied by Lindvall and Teorell (2016), to justify the rollout. The registration scheme was marketed using the slogan "*My Country, My Identity*" and firmly branded as a modernizing nation-building project. Citizens were told that their biometric identity cards would facilitate improved access to electoral participation, the formal banking sector, and would enable them to more easily access areas of service delivery including health care and education. Importantly, given the state's relatively high bureaucratic capacity, such consequences for improved access and exclusion were likely considered credible by registrants.

Together, the ex ante signal sent by the rollout is aptly summarized by Doyle (2012): biometric technologies "can serve equally as a foundation for poverty alleviation and social justice, and as a means of state control through the better monitoring of citizens and exclusion of non-citizens ... [Uganda has] introduced civil registration using humanitarian rhetoric adopted from Western liberal donors, but employing mechanisms which suggest that the regime is as much concerned with reducing its own vulnerability." Given this ex ante

ambiguity, compounded by the novelty of the technology involved, it is therefore not obvious whether citizens' behavioral response ought to prioritize the extractive or productive future implications of the state-building investment.

Importantly, as discussed below, over time many of the exclusionary concerns about the scheme have been subsequently realized ex post, with civil society groups decrying the ongoing use of biometric data to, increasingly and comprehensively, restrict citizens' access to public resources.[14] Recent efforts to tie the receipt of COVID-19 vaccines to biometric registration, for example, were only narrowly defeated by legal challenges. The Initiative for Social and Economic Rights (2021) documents widespread exclusion from accessing public resources enabled by the national identity card program, characterizing the scheme overall as a "national security weapon."

4.4 Research design

Isolating the attitudinal effects of citizens' exposure to state-building schemes poses two inferential challenges. First, *which* citizens select into exposure to such efforts is likely to be confounded. For example, social survey data from Uganda suggests that—even employing fine-grained location fixed effects—systematic imbalances exist in who interacts with the state. As Figure C.2 shows, respondents who report more frequent state interactions are more likely to be older, male, and more educated than those who report less frequent interactions.

Second, as discussed above, capacity investments often generate compound sequencing effects which imply empirical challenges. For example, a government aiming to increase taxation might (1) invest in its resources to do so by creating a new land cadaster, before (2) subsequently implementing a policy which leverages these increased resources by using the cadaster to increase monitoring and enforcement (D'Arcy and Nistotskaya 2017). Be-

[14]Further, there is strong evidence that the scheme has undercut the authority of local leaders, who "are increasingly seen as powerless to resolve crucial issues around identification and access to social services by the local populations they serve" (Initiative for Social and Economic Rights 2021).

cause citizens might respond differently to the investment before its policy consequences are realized,[15] isolating how citizens update their beliefs and expectations in response to the (possibly ambiguous) signal of the initial investment *alone* is rendered temporally challenging.

4.4.1 Staggered issuance of biometric identification

To overcome these inferential challenges, I leverage the staggered expansion of Uganda's biometric identification scheme combined with the fortuitous timing of a social survey. As detailed above, after having initially registered citizens, the Government of Uganda began the issuance of biometric identity cards to its citizens. This issuance process took place in different districts over time between December 2014 and September 2015. Figure 4.2 illustrates the staggered expansion of the issuance process, showing the share of districts where issuance had commenced by a given month.[16] Importantly, within the time span of this staggered rollout, Afrobarometer locally fielded round 6 of its social survey in May 2015. By this point, issuance had taken place in 38 (34%) of the country's districts. Figure C.1 shows that these districts are distributed across all four of Uganda's regions (C.1a) and how these districts compare to districts where the rollout had yet to occur (C.1b). Citizens in earlier-targeted districts are relatively wealthier, younger, and have more pre-existing local public goods than those targeted later.

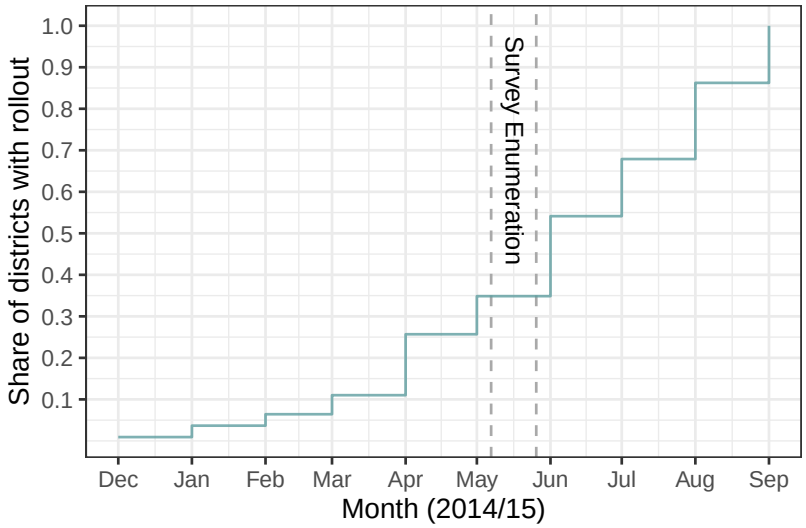
Solving the second inferential challenge, the close temporal proximity of the enumeration timing to the issuance process means that effects on citizens' beliefs and attitudes are more likely to be driven by the ambiguous *signal* of capacity than by its effects on concrete policies. By the time of survey enumeration, generally just one to two months following issuance, there were essentially no uses for the biometric identity cards—it is only in the

[15]For example, while a new land cadaster might signal the state's modernizing ambitions and thereby improve citizens' attitudes towards the state, being individually subjected to increased taxes might well have the opposite effect (Christensen and Garfias 2021).

[16]Data on the timing of the issuance process at the district-level is taken from a combination of official reports, interviews, and social media announcements by the relevant government authority (NIRA).

subsequent years that the biometric IDs have become increasingly important in controlling accessing to a wide range of public resources. The survey timing potentially allows me to evaluate how citizens' attitudes, beliefs, and expectations were shaped by short-run exposure to the intensive rollout more than it allows me to consider their behavioral response and effects on political engagement.

Figure 4.2: Expansion of issuance process over time



Note: District-level card issuance rollout dates come from administrative reports, interviews, press releases, and social media.

Solving the first inferential challenge, the timing of survey enumeration generates *spatial* variation in whether survey respondents had been exposed to the rollout: at the time of enumeration, only some districts had been 'treated' by the initiation of the issuance process. By itself, this variation is likely to be confounded by the non-random selection of which districts to target earlier rather than later in the process (see Figure C.1b). I therefore combine this spatial variation with *temporal* variation by drawing on additional geolocated Afrobarometer survey data from round 4 (2008), 5 (2012), and 7 (2018). As detailed more formally below, this suggests the viability of a difference-in-differences design which identifies short-run effects on beliefs and expectations leveraging the partial exposure of districts to treatment during the enumeration of the 2015 survey.

4.4.2 Data

I draw outcome data from geolocated Afrobarometer rounds 4 (2008), 5 (2012), 6 (2015), and 7 (2017), while data on treatment assignment comes from administrative data on the rollout of the issuance process across districts over time described above.

Independent variables. The treatment variable Rollout_{dy} is an indicator defined at the district-year level. It takes on a value of 1 if the issuance process had taken place in district d by year y. As a result, the treatment variable takes on a value of 0 across all districts *prior* to 2015, a value of 1 across all districts *after* 2015, and a value of 1 *only* in the subset of districts where issuance had taken place prior to survey enumeration in 2015 (Figure C.1a). Importantly, the treatment is defined as the reduced form effect of citizens' potential exposure to the rollout rather than the actual receipt of a biometric identity card.

Dependent variables. While Afrobarometer does not directly ask participants about their exposure to the rollout,[17] in most waves respondents are asked about their experiences with particular government agencies. To measure exposure to the rollout, then, I use a proximate outcome relating to whether respondents report receiving any kind of permit or document from the government in the year prior to enumeration.[18] For a placebo outcome, I use an analogous survey question about whether respondents have interacted with government agencies relating to service delivery in the year prior to enumeration.

The primary outcomes consist of citizens' attitudes and allegiances towards the central state. The first outcome I focus on is respondents' beliefs about the legitimacy and primacy of the state: whether citizens are obliged to pay taxes, obey the law, and obey the government more broadly. Second, to assess the extent to which any updating about the state's authority is driven by the *productive* versus *extractive* signal sent by the rollout, I consider families of questions relating to (1) economic performance and future expectations; (2) their perceptions

[17]The Afrobarometer round 7 survey in Uganda does ask about citizens' possession of a national ID, but this is the wave following the initial rollout

[18]The phrasing of this question varies slightly across rounds: for example, in round 5 it asks whether respondents have *ever* received a document or permit, while in round 6 it asks about *the last year*.

of coercion, freedom, and surveillance.

4.4.3 Estimation

I estimate the following baseline equation:

$$y_{idry} = \beta \text{Rollout}_{dy} + \mu_d + \nu_{ry} + \epsilon_{idry}, \tag{4.1}$$

where survey responses for individual i in district d in region r in survey year y are regressed onto the treatment indicator Rollout_{dy}. To absorb temporal variation across years, and how these might differ across regions, I add region-year fixed effects ν_{ry}. To absorb spatial variation across respondents in different parts of the country, I add time-invariant locational fixed effects, μ_d.

Because districts are small, and hence many appear in only a subset of the waves of the survey data, in the baseline specification these locational fixed effects are defined at the region-group level, where *group* indicates whether a given respondent is in a district where the rollout had commenced by the time of survey enumeration in 2015.[19] In additional specifications I add a vector of individual-level and enumeration area-level controls, which marginally improve precision,[20] and use district rather than region-group fixed effects, which substantially reduce the amount of available variation to estimate effects. Standard errors are clustered at the district-level throughout.[21]

β is the coefficient of interest, which estimates the reduced form effect of (potential) exposure to the state-building intervention on citizens' beliefs, attitudes, and expectations. Intuitively, it compares the magnitude of the differences in y across treated and control districts during the mid-rollout survey enumeration relative to *either* the prior *or* the subsequent survey waves. β estimates a causal quantity conditional on two assumptions. First,

[19] As a result, there are 8 time-invariant region-group fixed effects.

[20] As detailed below, since these controls are well-balanced when used as outcomes on the left-hand side of Equation (4.1), their inclusion on the right-hand side should not in expectation substantially change estimates of β.

[21] Significant district proliferation has taken place in Uganda over time (Grossman and Lewis 2014). For consistency, I geolocate all survey respondents to their district as defined in 2014, when the issuance process began.

common to difference-in-differences designs, we require that *absent* the rollout, trends in outcome variables would have been parallel over time across districts which were treated in 2015 and those which were not. I more formally assess the plausibility of this identifying assumption below, where I simulate the rollout to have taken place instead during prior survey rounds (2008 or 2012), or during the subsequent survey round (2017), rather than 2015. Intuitively, given the design, it is possible to demonstrate that both pre-2015 and *post*-2015 trends are parallel.[22]

Second, because the outcome survey data is administered as a repeated cross-section which is representative at the region, rather than district, level, it is possible that compositional differences in the survey sample over time across districts could be correlated with treatment assignment. This would be the case, for example, if the rollout itself affected the types of citizens willing to participate in a social survey administered shortly thereafter. To assess whether this is a concern, in Figure C.3 I estimate a vector of pre-treatment covariates at the individual-level as outcomes on the left-hand side of Equation (4.1). Across these variables, there is little systematic evidence of imbalances in the estimates of β which would otherwise provide evidence of compositional differences correlated with treatment.

A final concern regarding the research design relates to social desirability bias, which would induce β to estimate the effect of exposure to the rollout on *survey responses* but not citizens' underlying beliefs. Recent work suggests that surveys administered close to major events can lead respondents to systematically overstate socially desirable attitudes and behaviors, such as voting (Singh and Tir 2021), and so the close timing of the surveys to the issuance process could raise similar concerns. To assess this, in Table C.1 I assess whether treated respondents report similar levels of past political participation over the prior year, which is socially desirable but implausibly affected by treatment. I find little evidence of imbalances, suggesting that social desirability bias is likely to be a relatively small concern.

[22]Since there is only district-level variation in treatment status during the rollout year, as I permute this rollout year in these tests the treatment status of all districts *prior* to the simulated rollout year is zero and *after* the simulated rollout year is 1.

4.5 Results

4.5.1 Effects on exposure to rollout

To assess whether the research design isolates meaningful variation in citizens' exposure to the rollout, I use an indicator for whether respondents report interacting with the government to obtain any permit or document in the past year. While an imperfect measure—respondents might have received documents unrelated to the identity cards, and those who report not receiving any documents might still have been exposed to the rollout process—this outcome ought nonetheless to provide an informative 'first stage' with regard to sign if not the magnitude of the estimate. In addition, in Panel B I consider whether respondents report interacting with the government to try and access other public services, such as health care, electricity, schools, and water over the prior year. Table 4.1 provides results.

Table 4.1: Effects on receipt of documents from government

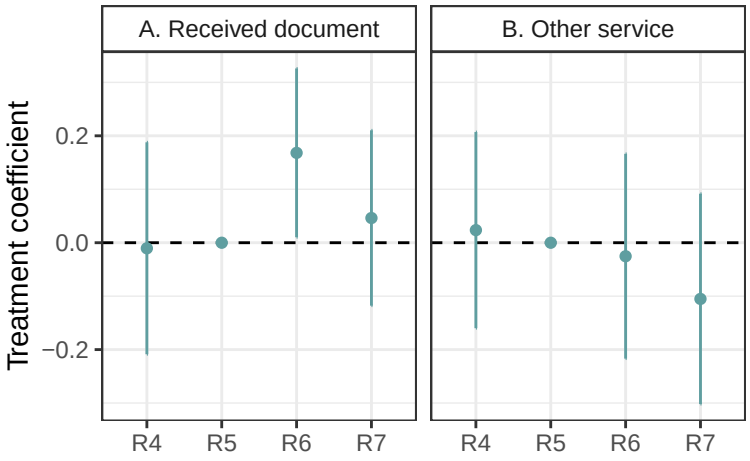
	A. Received document				B. Other services			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rollout (β)	0.085**	0.093**	0.075**	0.085**	0.032	0.034	0.031	0.034
	[0.038]	[0.037]	[0.037]	[0.036]	[0.025]	[0.025]	[0.026]	[0.027]
Outcome mean	0.56	0.56	0.56	0.56	0.83	0.83	0.83	0.83
Outcome range	[0-1]	[0-1]	[0-1]	[0-1]	[0-1]	[0-1]	[0-1]	[0-1]
Region-round FE	✓	✓	✓	✓	✓	✓	✓	✓
Region-group FE	✓	✓			✓	✓		
District FE			✓	✓			✓	✓
Controls		✓		✓		✓		✓
Observations	8375	8375	8375	8375	8375	8375	8375	8375
Clusters	105	105	105	105	105	105	105	105

DVs: (A) respondent interacted with the government to get an ID document in the prior year; (B) respondent has interacted with the government to access other services in the prior year.
All specifications are estimated using OLS using Equation (4.1). *Rollout* is defined as respondent being in a district where, in 2015, biometric card issuance had commenced by the time of survey enumeration.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the district-level in parentheses.

Column 1 corresponds to the baseline estimate of β from Equation (4.1). It suggests that the difference in the probability of respondents reporting having recently received a document from the government, between treated districts and control districts, is 8.5 percentage

points (pp) greater in 2015 (the mid-rollout year) relative to either before or after 2015 ($p < 0.05$). For the measurement reasons highlighted above, this is likely an underestimate of the true difference in exposure. The magnitude of β is slightly larger when including controls (Column 2), but otherwise similar when adding district fixed effects (Column 3), or both controls and district fixed effects (Column 4). As a placebo, in Panel B there is comparatively weaker evidence of treatment effects on respondents' levels of interaction with the government relating to other areas of service delivery.

Figure 4.3: Exposure to rollout estimated by wave



Note: Figure estimates β in Equation (4.1) while interacting the *group* indicator with a dummy for each survey wave. R5 (2012) is the excluded round, while R6 is the mid-rollout round. Specification includes region-round fixed effects, region-group fixed effects, and clusters standard errors at the district-level.

Further supportive of the research design, in Figure 4.3 I estimate Equation (4.1) while interacting the cross-sectional treatment indicator with a survey round dummy. This plot suggests that the positive treatment effects on respondents obtaining state documentation are *only* observed during the mid-rollout survey wave in 2015, and hence that both pre-trends and post-trends are parallel.

4.5.2 Effects on attitudes and expectations

Next, with evidence supporting the validity of the research design, I consider how respondents interpret this signal of capacity and update their attitudes towards the state.

First, following Migdal (1988), I consider the net effects on citizens' attitudes towards the primacy and legitimacy of the state as a social actor. I consider four standardized outcomes: whether the police have the right to make people obey the law; whether the tax authority has the right to make people pay taxes; whether courts have the right to make laws that people must obey; and whether the government has the right to make people obey its rules. In addition, I create a standardized z-score index aggregating these constituent components.

Table 4.2: Effects on perceptions of state authority

	A. Index		B. Courts		C. Police		D. Tax		E. Govt	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Rollout	0.27***	0.26***	0.18**	0.13*	0.26***	0.24***	0.28***	0.27***	0.14**	0.13*
	[0.07]	[0.07]	[0.07]	[0.07]	[0.08]	[0.08]	[0.07]	[0.08]	[0.07]	[0.07]
Region-round FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region-group FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓		✓
Observations	5992	5992	8447	8447	8447	8447	8447	8447	5992	5992
Clusters	105	105	105	105	105	105	105	105	105	105

DVs are all standardized: (A) z-score index of other items; (B) courts have the right to make decisions citizens must obey; (C) police have the right to make citizens obey the law; (D) tax authorities have the right to make people pay taxes; (E) government has the right to make people obey its rules.

All specifications are estimated using OLS using Equation (4.1). *Rollout* is defined as respondent being in a district where, in 2015, biometric card issuance had commenced by the time of survey enumeration. * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors clustered at the district-level in parentheses.

The results in Table 4.2 suggest that respondents' perceptions of state authority are notably increased by their exposure to the intensive rollout. Focusing on the aggregate index in Panel A, quasi-random exposure to the intervention increases respondents' perceptions of the state's authority by 0.26σ ($p < 0.01$). There are positive effects on the perceived authority of each state institution, with the strongest effects observed for the authority of the police (0.26σ, $p < 0.01$) and the tax authority (0.27σ, $p < 0.01$). Effects on perceptions of court authority and general government authority are positive but smaller in magnitude. The estimates are robust to the inclusion of controls in even-indexed columns.

Citizens' positive updating about the state's authority could be consistent with the rollout being interpreted either as a productive or an extractive signal. Productively, such updating might be driven by citizens' expectations of future provision being raised by the

developmentalist signal of the rollout. With increased economic expectations, citizens then afford the state more authority for the implementation of its policies. Extractively, the effects on perceived authority might be driven by citizens updating about the ability of the state to monitor and coerce citizens into compliance with its demands.

To measure effects on beliefs about the positive welfare implications of the scheme, I use a standardized index of questions which measure respondents' national economic evaluations: how it is currently; how it compares to the situation one year prior; and how they expect the economic situation to be one year in the future.[23] To measure effects on beliefs about monitoring and control, I use a standardized index of questions which measure how free the respondent considers themselves to be: to express their views; to join organizations; to vote as they like; and how careful they need to be when talking about current affairs.

Table 4.3: Productive versus extractive signals of capacity

	A. Economic signal		B. Monitoring signal	
	(1)	(2)	(3)	(4)
Rollout	0.14**	0.15**	0.00	0.01
	[0.07]	[0.07]	[0.07]	[0.07]
Region-round FE	✓	✓	✓	✓
Region-group FE	✓	✓	✓	✓
Controls		✓		✓
Observations	8447	8447	8447	8447
Clusters	105	105	105	105

DVs are all standardized: (A) z-score index of variables relating to economic perceptions and expectations; (B) z-score index of variables relating to monitoring and perceived freedom (higher values indicate more perceived freedom).

All specifications are estimated using OLS using Equation (4.1). *Rollout* is defined as respondent being in a district where, in 2015, biometric card issuance had commenced by the time of survey enumeration. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the district-level in parentheses.

Using the index relating to citizens' economic expectations, Panel A in Table 4.3 indicates that respondents exposed to the rollout are around 0.14σ more positive in their economic

[23]Unfortunately, equivalent questions about respondents' *personal* economic evaluations are only asked in rounds 4 and 5.

evaluations of the country ($p < 0.05$), which remains robust to the addition of controls in column 2. Table C.2 disaggregates this index into its components. This shows that the positive treatment effect is almost entirely driven by changes in respondents' future expectations: respondents expect the country's economic situation to improve by 0.20σ ($p < 0.01$) over the following year. Panel B, using the outcomes relating to citizens' perceptions of monitoring and freedom, suggests null treatment effects. Disaggregating the index into its subcomponents in Table C.3, there is no evidence of statistically significant or substantively large treatment effects across any of the subcomponents. Because perceptions of freedom might be an imperfect proxy for perceived monitoring by the state, in Table C.4 I assess supplementary related outcomes: how easy people find it to avoid paying their taxes, and whether law-breakers tend to be punished. Neither outcome provides evidence of treatment effects.

Supplementary analyses suggest that these effects are relatively uniform across subgroups. In Table C.5, I assess the extent of heterogeneity in the treatment effect estimates from the standardized indices in Table 4.2 and 4.3 based on indicators of (1) partisanship (NRM or non-NRM); (2) high/low income; (3) high/low education. There is no evidence of heterogeneous treatment effects regarding the state authority outcomes. Regarding the economic and monitoring signals, there is weak evidence that wealthier citizens update more negatively regarding their perceived freedom, and that NRM supporters update more positively regarding their economic evaluations than regarding their perceived freedom. Finally, in Table C.6, motivated by work on banal nationalism (Billig 1995; Robinson 2016), I show that exposure to the rollout has weakly positive, and statistically insignificant, effects on citizens' extent of affiliation with the nation over their ethnic group.

4.6 Discussion

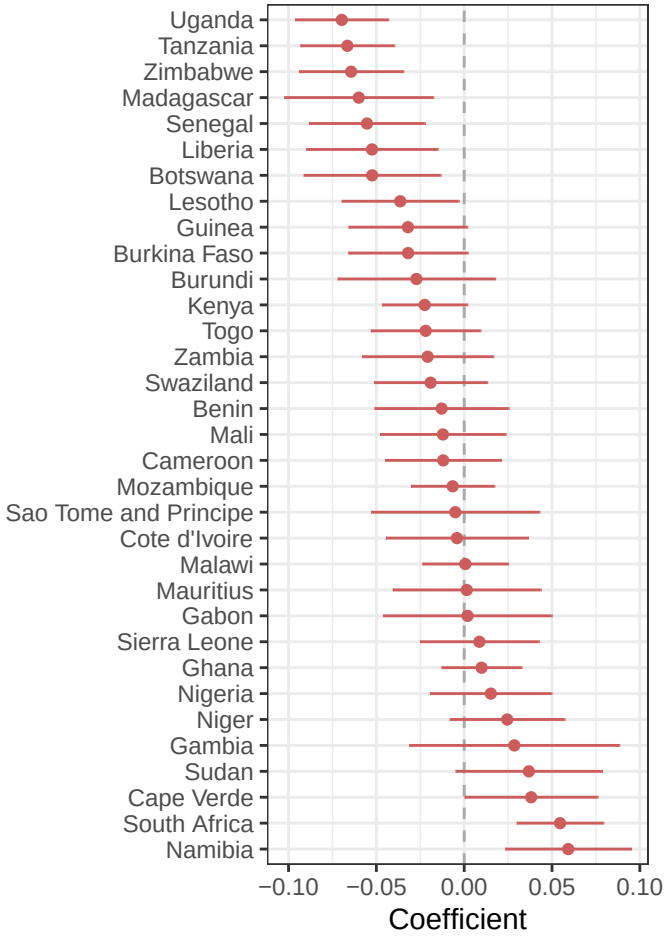
The results establish that citizens' beliefs about the state's authority are relatively elastic, in the short-run, to an intensive state-building intervention. Driving these effects, there is

limited evidence that citizens interpret the rollout as an extractive signal of state capacity even in spite of the highly coercive, unilateral aspects of its implementation. Instead, it seems that citizens initially much more heavily weigh the productive, modernizing potential of the scheme to facilitate future service delivery.

That the *productive* signal of capacity seemingly dominates the *extractive* signal, in this setting, is plausible for three reasons. First, because the scheme was effectively administered by the state—citizens registered, and the research design isolates variation in the state delivering on its promise to supply identity documents to citizens in exchange—this might be seen as a positive signal for citizens' future economic welfare. Second, the intervention itself, involving relatively sophisticated technology, was seen as evidence of state modernization in many of the debates surrounding its implementation described above. Third, since the scheme had little immediate value for citizens (at least by the time of survey enumeration), the rollout emphasized the government's claims that it would facilitate *future* improvements in public goods and service delivery.

Nonetheless, that citizens update more about the benefits of increased capacity than its costs is rendered surprising by cross-sectional evidence. In Figure 4.4, pooling Afrobarometer data from all countries and waves, I estimate the correlation between citizens' perceptions of state authority and the extent of their interactions with state institutions while controlling for individual-level covariates and enumeration area-level fixed effects. This suggests that, across most countries in sub-Saharan Africa, more frequent exposure to the state over time in a given locality is associated with *decreased* perceptions of its authority. Uganda, indeed, is the most extreme example across the entire sample: a one standard deviation increase in citizens' interactions with the state, holding fixed their locality, correlates with a 0.07σ ($p < 0.01$) decrease in their perceptions of its authority.

Figure 4.4: Relationship between citizens' interactions with the state and their perceptions of state authority



Note: Figure presents point estimates and 95% confidence intervals from regressing respondents' standardized perceptions of state authority (Panel A in Table 4.2) onto their standardized usage of public services (e.g. getting ID documents, interacting with police, getting medical treatment) in the prior year. Regressions control for enumeration area-level fixed effects and respondents' income, age, gender, and education.

4.6.1 Evidence on citizens' response to realized policy implications of the scheme

I suggest that this pattern of results is consistent with citizens' heightened expectations regarding the benefits of state capacity becoming disappointed by its ex post implications (or lack thereof) for policy implementation. I provide evidence towards this drawing on three pieces of evidence. First, the research design provides variation in how *recently* the registration rollout had occurred across different districts in the country (see Figure 4.2). As noted above, while the cards were implemented with promises relating to service delivery, the initial utility of the cards for accessing public resources was extremely limited. Leveraging this temporal heterogeneity, I consider whether the treatment effects estimated above decay over time.

Table 4.4: Heterogeneous effects by recency of rollout

	A. Authority		B. Economic signal		C. Monitoring signal	
	(1)	(2)	(3)	(4)	(5)	(6)
Rollout	0.27***	0.29***	0.14**	0.20***	0.00	0.04
	[0.07]	[0.07]	[0.07]	[0.07]	[0.07]	[0.07]
Rollout × Early		-0.13		-0.24**		-0.14*
		[0.12]		[0.10]		[0.09]
Region-round FE	✓	✓	✓	✓	✓	✓
Region-group FE	✓	✓	✓	✓	✓	✓
Observations	5992	5992	8447	8447	8447	8447
Clusters	105	105	105	105	105	105

Early is a cross-sectional indicator for districts which were exposed at the beginning of the national rollout. DVs are all standardized: (A) Standardized index of outcomes from Table 4.2; (B) Standardized index of outcomes from Table C.2; (C) Standardized index of outcomes from Table C.3 (higher values indicate more perceived freedom).

All specifications are estimated using OLS. * p < 0.1, ** p < 0.05, *** p < 0.01. Heteroskedasticity-robust standard errors in parentheses.

In Table 4.4, I therefore estimate Equation (4.1) while interacting the treatment indicator with a cross-sectional dummy for whether respondents are in districts which were *early-treated* by the rollout.[24] These results provide some evidence of temporal heterogene-

[24]Specifically, I define the indicator *Early* if respondents are in a district where the rollout had commenced

ity. Respondents in districts where the rollout had been less recent at the time of survey enumeration, relative to respondents in recently-treated districts, update insignificantly less positively about the state's authority (column 2); significantly less positively about their economic expectations (0.24σ, $p < 0.05$, column 4); and weakly significantly more negatively about their perceived freedom (-0.14σ, $p < 0.1$, column 6).[25] Together, before specific policy consequences of the biometric rollout had been realized, these results provide some evidence that citizens' short-run updating towards the state diffuses over time.

Second, I implement an alternative difference-in-differences design to consider the longer-run effects of the biometric rollout on citizens' attitudes and expectations as the policy implications of the scheme begin to be realized. Specifically, I leverage the fact that survey round 5 (2012) asks whether respondents have *ever* received an identity document from the state,[26] and that the post-rollout round 7 (2017) survey asks whether respondents have received a *biometric* identity card from the state (true for 95% of the sample). Using these, I create a standardized district-level measure of the intensity of the biometric rollout defined as the increase in citizens' receipt of ID documents from the state between the two rounds.[27] I combine this spatial variation with temporal variation from whether the survey was administering after the biometric cards had actually begun to be used in service delivery (i.e. comparing 2017 to pre-2017 survey waves).

In Table 4.5, I estimate these difference-in-differences effects on the same set of standardized outcomes as in Table 4.4. These estimates indicate overall null longer-term effects of the intensity of the state-building scheme on citizens' perceptions of state authority and their economic expectations. Suggestively, though, citizens perceive themselves to be 0.05σ *less*

more than one month prior to survey enumeration. This corresponds to 12/38 of the districts in which the issuance rollout had commenced prior to survey enumeration.

[25]Considering the main effect for respondents in early-treated districts (i.e. Rollout + Rollout × Early), estimates are weakly significantly positive for perceived authority; null for economic evaluations; and weakly insignificantly negative for perceived freedom.

[26]In all other survey rounds, this question asks whether respondents have received an identity document from the state in the prior year.

[27]In the few cases where fewer respondents report having received a biometric card in 2017 relative to any identity document in 2012, I assign these districts a value of 0.

Table 4.5: Longer-run outcomes as a function of rollout intensity

	A. Authority		B. Economic signal		C. Monitoring signal	
	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{17}$ × Intensity	-0.00	-0.00	0.03	0.01	-0.06**	-0.05**
	[0.04]	[0.04]	[0.05]	[0.05]	[0.02]	[0.02]
Region-round FE	✓	✓	✓	✓	✓	✓
Region-group FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Observations	2744	2744	4408	4408	4408	4408
Clusters	61	61	61	61	61	61

$Post_{17}$ is an indicator for the 2017 survey wave; *Intensity* is a standardized district-level measure of the difference in ID document receipt between 2012 and 2017. DVs are all standardized: (A) Standardized index of outcomes from Table 4.2; (B) Standardized index of outcomes from Table C.2; (C) Standardized index of outcomes from Table C.3 (higher values indicate more perceived freedom). Smaller sample size is due to missingness in the intensity variable when a district does not appear in both 2012 and 2017 survey waves. All specifications are estimated using OLS by regressing outcomes onto region-round FE, region-group FE, and the interaction of $Post_{17}$ × Intensity. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the district-level in parentheses.

free ($p < 0.05$) in districts where the intensity of the biometric rollout was higher, indicative of the scheme's ex post realization of extraction and monitoring more so than the increased supply of welfare.

Third, that citizens' interpretation of the ambiguous state capacity signal shifts over time—from ex ante economic optimism to ex post concerns about monitoring by the state—is borne out by qualitative evidence. While the scheme was initially heralded by international donors (World Bank 2018a), civil society organizations have since documented the ways in which citizens' hopes have been left unfulfilled. Evidence from 459 interviews compiled by Initiative for Social and Economic Rights (2021), for example, makes this clear: "Our research suggests that the *Ndaga Muntu* [national ID] is a national security tool that has caused severe exclusion" (p.11), now becoming a de facto prerequisite for nearly any interaction between citizen and state. Further, they note that "*Ndaga Muntu* is a system that may be digital at its core but is still mostly analog on the periphery," to the extent that citizens' actual interactions with the bureaucracy continue to ultimately rely on complex, antiquated document-keeping systems scattered across government offices. Despite initial

signals to digitalize and streamline the delivery of services to citizens, ex post the scheme has realized few of these promises.

Taken together, citizens' initial response underscores the ambiguous signal sent by state-building investments. Despite a classic literature arguing that citizens' demand for such intrusive schemes ought to be limited—given their implications for coercion and the sidelining of social institutions—in this setting, citizens seem to *demand* a stronger state due to its implications for the future provision of welfare. In spite of its coercive aspects, citizens appear to have updated relatively little in the short-term about the potentially extractive implications of supplying biometric information to the state. But, as the ex post implications of such investments become apparent over time, citizens' initially raised expectations tend towards disappointment and disillusionment.

Together, this temporal inconsistency in citizens' response to complex state capacity signals rests on two factors. First, it relies on the technological novelty of state-building interventions, which shapes the malleability of citizens' expectations about their downstream implications for policy implementation. Second, it depends on the state's sensitivity to the disappointed, or even misled, expectations of its citizens. Such unmet expectations have long been associated as a destabilizing consequence of modernizing reforms, as citizens' expectations rise faster than the realized ability of the state to meet them Huntington (1968). In this setting, the Ugandan government was able to intensively fortify its informational resources because of the ease of couching such a scheme in modernizing, developmentalist discourse, as well as the fact that its low levels of political competition insulate it from the disappointment of its citizens.

A | Appendix to Chapter 2

A.1 Supplementary information

A.1.1 Legal history of compulsory birth registration in Tanzania

The legal framework for birth registration in Tanzania is provided through Chapter 108 of the Tanzanian legal code. Chapter 108, the Birth and Death Registration Act, was originally passed in 1920 and came into force on 1 April, 1921. The act has been repeatedly amended since then. No provision for the compulsory registration of any births was made until 1949, when the *Births (Non-Native Compulsory Registration) Order* amended Section 26 of the act to read:

> "The registration of the birth of a child shall be compulsory if either one or both parents are of European or American origin or descent or, in the case of a child born out of wedlock and not recognised by its father, if the mother is of European or American origin or descent."

The registration of Tanzanians across the country remained voluntary. In 1962 the act was amended as per Government Notice (G.N.) 478/62[1] to state the following in Section 28:

> "The Minister may, by order published in the Gazette, extend, from a date to be named in the order, the provisions of this Act relating to the compulsory registration of births and deaths to all persons in Tanzania of any particular race, class, tribe or other group, or to all or some of the inhabitants of any particular town, district, or other area, and from and after the said date the

[1] I follow the standardized syntax of the *African Law Digest* where the initial digits reflect the relevant Government Notice number in a given year and the final two digits reflect the year.

registration of births and deaths shall, in such cases, be compulsory instead of being optional."

Applying this amendment, the provisions relating to compulsory registration were first extended to the districts comprising Dar es Salaam. Under G.N. 58/66, published 9 February 1966, the *The Births and Deaths Registration (Dar es Salaam Municipality) Order*, the registration at birth of all individuals born after 1 March 1966 was made compulsory. Compulsory registration was then extended to a further set of districts under G.N. 175/66 published on 11 June 1966, the *The Births and Deaths (Compulsory Registration) Order*, to all individuals born in those districts after 1 July 1966. These districts are listed in the order as Arusha, Bukoba, Dodoma, Iringa, Kigoma, Lindi, Mbeya, Morogoro, Moshi, Mtwara, Musoma, Mwanza, Tabora, and Tanga.

This extension of compulsory registration was accompanied by changes to the price of registering births under G.N. 275 on 18 September 1966, the *The Registration of Births and Deaths (Amendment) Rules*. Under this notice, the cost of registration for individuals born *either* in a district without compulsory registration *or* before the date when registration became compulsory in a given district were set at 5 TSh for individuals under 5 years, 10 TSh for individuals between five and ten years old, and 30 TSh for individuals above 10 years. For those individuals born in *after* registration had become compulsory in a given district, the cost of registration was 5 TSh for individuals registered within three months of birth and 30 TSh for individuals registered more than three months after their birth. 30 TSh was equal to approximately $4 in 1966 and approximately $33 today[2].

Compulsory birth registration was not extended to any more districts until 1981, under the decentralized reforms discussed in the paper. This sequence of extending compulsory registration comprised the *Births and Deaths Registration (Kinondoni, Ilala, Temeke, Bagamoyo and Moshi Rural District) Order* (1981), the *Births and Deaths Registration (Morogoro Rural District) Order* (1982), the *Births and Deaths Registration (Specified Districts) Order* (1982), the *Births and Deaths Registration (Specified Districts) Order* (1986), the

[2]U.S. Treasury (1966) cites an exchange rate of 7.133 TSh per USD on 30 September 1966.

Births and Deaths Registration (Specified Districts) Order (1988), the *The Births and Deaths (Compulsory Registration) Order* (1994) and the *Births and Deaths Registration (Mufindi District) Order* (1996).

Even after these reforms birth registration remained voluntary in a substantial amount of the country. Birth registration became *de facto* compulsory for all births in 2002 under the UNICEF-sponsored *Compulsory Registration Programme* (Registrar General's Office 2005). However, the law to enforce compulsory birth registration across the whole country was only finally amended under the *Written Laws (Miscellaneous Amendments) Act* of 2009. This amendment changed Section 26 of the act to read:

> "The registration of birth and death shall be compulsory."

Regarding punishments for violation, Section 29 of the act states that:

> "Any person who, being under an obligation to register the birth or death of any person, refuses to register or to state any of the prescribed particulars, shall be guilty of an offence and shall be liable on conviction to a fine not exceeding five hundred shillings or to imprisonment for a term not exceeding one month, or to both such fine and imprisonment..."

In Figure A.11 I plot this sequential expansion of compulsory birth registration across the districts of mainland Tanzania from 1950 to 2018. I plot the cumulative share of districts where birth registration was compulsory in a given year, demonstrating the staggered expansion of compulsory regulations over time.

A number of countries in sub-Saharan Africa implemented similarly targeted reforms in the early post-independence period intended to register citizens. These, broadly, were unsuccessful in inducing citizens' compliance beyond a narrow portion of the population. Makannah (1985) documents how birth registration was often rendered 'compulsory' by the state in a geographically targeted fashion, very similarly to the Tanzanian case, with wealthier urban areas targeted first before registration was declared a legal requirement

across the whole country.[3] Part of the reason for the failure of these reforms was attributed to the prohibitively high cost of infrastructure to register citizens, but other work makes clear that the benefits of registering with the state were often diffuse from the perspective of poorer citizens (Linder 1982; Powell 1981). Scholarly work at the time noted how, though states might benefit from improved systems of vital registration, political will to expand these schemes was often missing (Podlewski 1971).

A.1.2 Evidence on later reforms

In this section I provide descriptive evidence on these later reforms aiming to induce registration. Following a United Nations Mission to discuss progress relating to the 1978 Census, the improvement of civil registration across the country was identified as a key issue. This was codified under project URT/79/P05 "Reorganization and Expansion of the Civil Registration System" by the United Nations Population Fund (UNFPA). Four districts were initially selected in 1981, with an initial plan to expand the reform to an additional 21 randomly-selected districts (UNFPA 1983). Due to resource constraints, the expansion was reduced first to eight additional districts and eventually to just four. These districts, according to a later evaluation of the UNFPA Tanzania country programme, were specifically chosen on the basis of their birth registration rates and their ease of accessibility (Edouard 1987; Padmanabha 1993).

At the core of these reforms was the decentralization of administration which had been proposed in the early '70s (Wood 1971). Specifically, responsibility for the registration of births was assigned to 'ten-cell leaders' in a given community:

> "Registration of births and deaths will be re-organised such that the registration process starts at village level. In villages with village governments, the village managers will be appointed registration officers." (UNFPA 1982)

Compliance with these new responsibilities, however, was limited. One progress report

[3] In former French colonies, states often enforced requirements on registration if citizens lived within a given radius of a registration center rather than in a given district (Brass, Coale, and Demeny 1968).

pointed to the "reluctance of village managers and village secretaries to complete registration forms as they do not consider such function as being part of their duties." (UNFPA 1982). Additionally, relatively fewer citizens in the more rural districts affected by the reforms were aware of the need to register, nor the uses of registration. This was facilitated by limited publicity or informational campaigns around the project along with high rates of personnel turnover (Edouard 1987). As a result of these challenges, reports indicated that "a sharp decline in the number of registered events was evident in all project areas in 1983 when compared with the number of events in 1982" (UNFPA 1982). The project effectively ended in 1987 and saw little improvement during that time. From 1987, in the midst of a deep recession, the government expanded compulsory registration to a large set of additional districts under G.N. 842/88 with little effect. A 1993 review concluded that "A review of the implementation of the project would support the view that it has not made a significant impact on the system" (Padmanabha 1993).

Consistent with this, estimating the effects of the 1980s reform on registration rates using the census sample yields a small, but *negative*, effect on registration rates. Table A.11 provides results. The effects of the most recent wave of reforms, in 2009, yields a null effect on registration—likely since the legal extension had already *de facto* been made several years prior.

A.1.3 Evidence on the exclusion restriction

The exclusion restriction assumption requires that variation in exposure to the reforms leveraged in the research design only affects outcomes through increasing the probability of being registered at birth. I provide evidence supporting the plausibility of this assumption in two ways.

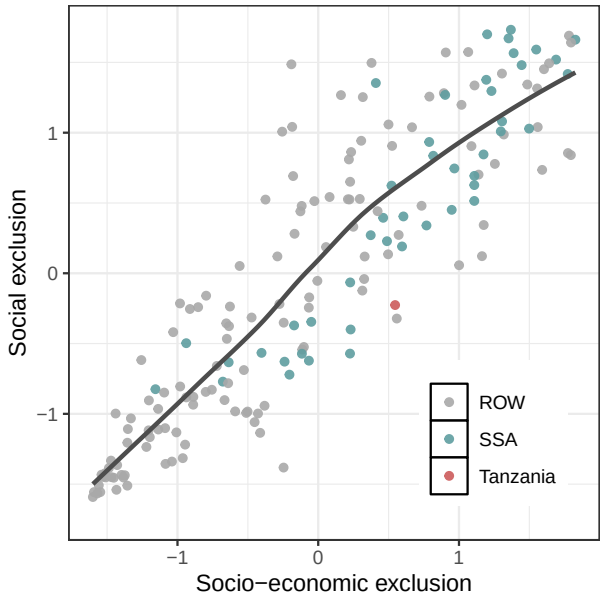
First, the exclusion restriction could be violated if other contemporaneous reforms specifically targeted the set of treated districts. To evaluate this, I construct a dataset of the text of all legislation ($n = 528$) passed in Tanzania in the period around the reform using data from the Southern African Legal Information Institute (SAFLII). For each document I code

the presence of relevant words to assess whether there were concurrent increases in legislation applied to the towns where the birth registration reform was passed. Figure A.12 plots the frequency of these different topics for each year from 1962 to 1970. The plot suggests that the incidence of legislation specifically mentioning the reform districts was minimal during this period, and legislation mentioning towns, or town councils, was generally decreasing. Analysis of all such laws mentioning these towns provides little evidence of other confounding reforms, nor of changes to the administration of town councils during this period. Thorough qualitative analysis of the Government Gazette and all supplemental notices issued by the Tanzanian government held by the Tanzania National Archives during this period provides little evidence of reforms applied specifically to this set of districts in a window around the reforms that were not additionally applied to the control districts.

Next, the exclusion restriction could be violated if broader changes during this period had particular effects on individuals born after the reform year in treated districts. The clearest such candidate is the passage of the *Arusha Declaration* in 1967, which marked Tanzania's shift towards socialism under the *ujamaa* philosophy of self-reliance and rural development. Three points suggest that *Arusha* is not a major threat for the empirical strategy. First, *Arusha* had almost entirely rural implications. The most rural districts are excluded from the baseline analysis sample by construction and results are robust to different specifications of control districts. If anything, any hypothetical *Arusha*-related effects should bias the IV coefficients *downwards* as development priorities shifted away from urban districts. Second, scholarly accounts suggest that *Arusha* had limited effects even on rural development until well into the 1970s, when efforts to reorganize the countryside intensified (Hyden 1975). Third, since exposure to the reform is defined by year of birth, any *Arusha*-based argument would have to link the *year* of individuals' birth with a confounding story in a way that also explains the observed pattern of estimates.

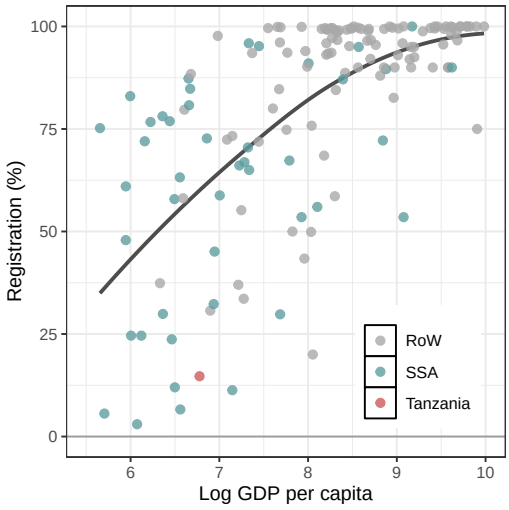
A.2 Additional figures

Figure A.1: Exclusion from public resources based on economic versus social status

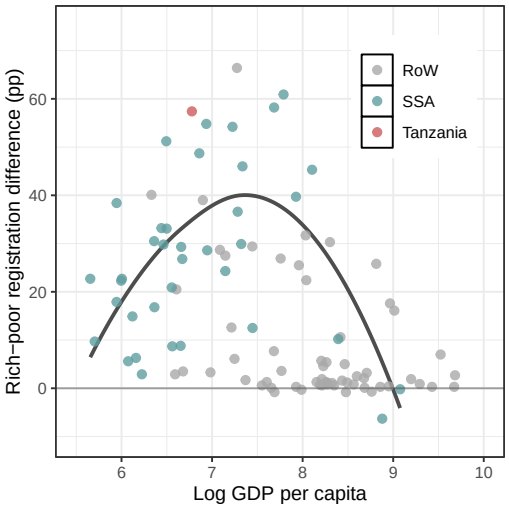


Source: V-DEM v.11.1 dataset. Figure plots the relationship between exclusion from public resources based on *socioeconomic* status (`v2xpe_exlecon`) against exclusion from public resources based on *social* (typically ethnic) status (`v2xpe_exlsocgr`).

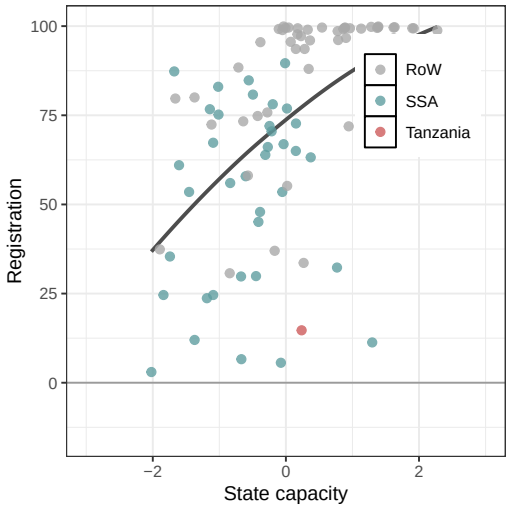
Figure A.2: Income, registration, and inequality (supplementary)



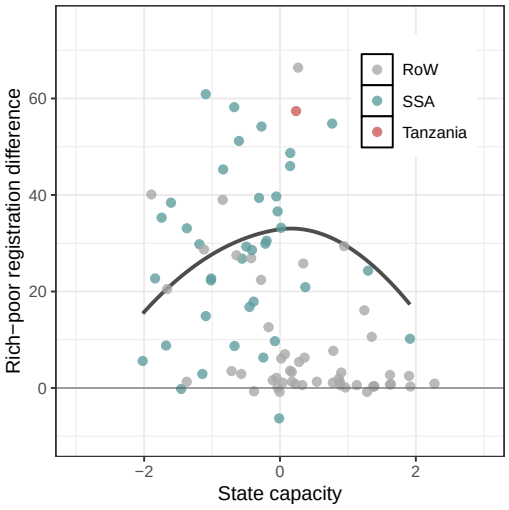
(a) Income and registration rates



(b) Income and registration inequality



(c) Hanson and Sigman (2021) measure of state capacity and registration rates



(d) Hanson and Sigman (2021) measure of state capacity and registration inequality

Sources: World Bank, UNICEF, Political Risk Services Group, Hanson and Sigman (2021).

Figure A.3: Measure of Tanzania's state capacity over time

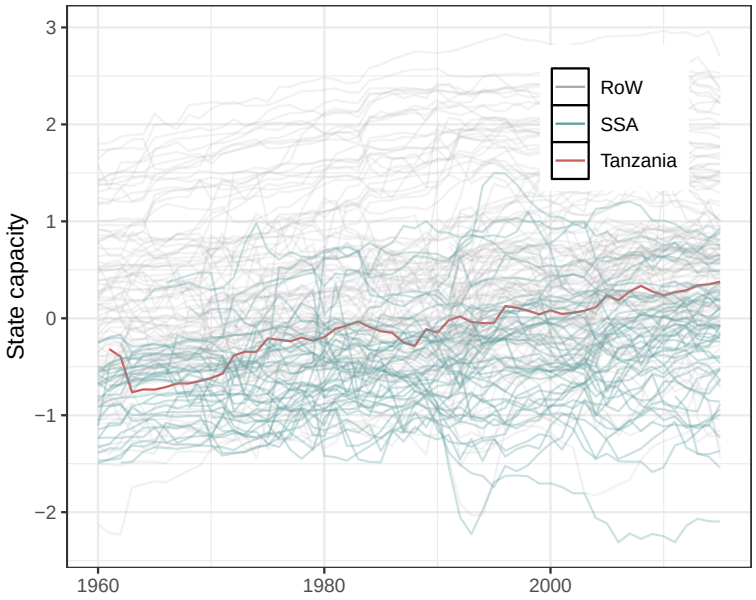


Figure plots the Hanson and Sigman (2021) measure of state capacity at the annual level. Tanzania gains independence in 1961.

Figure A.4: Spatial distribution of treated and control districts

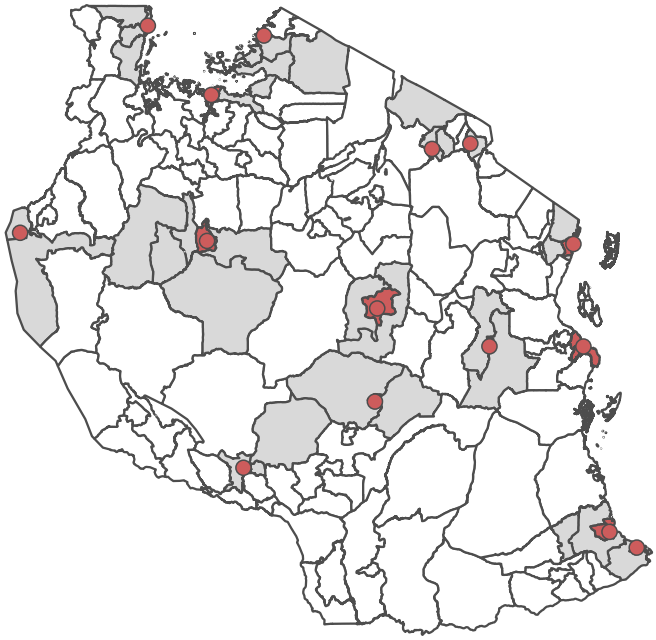
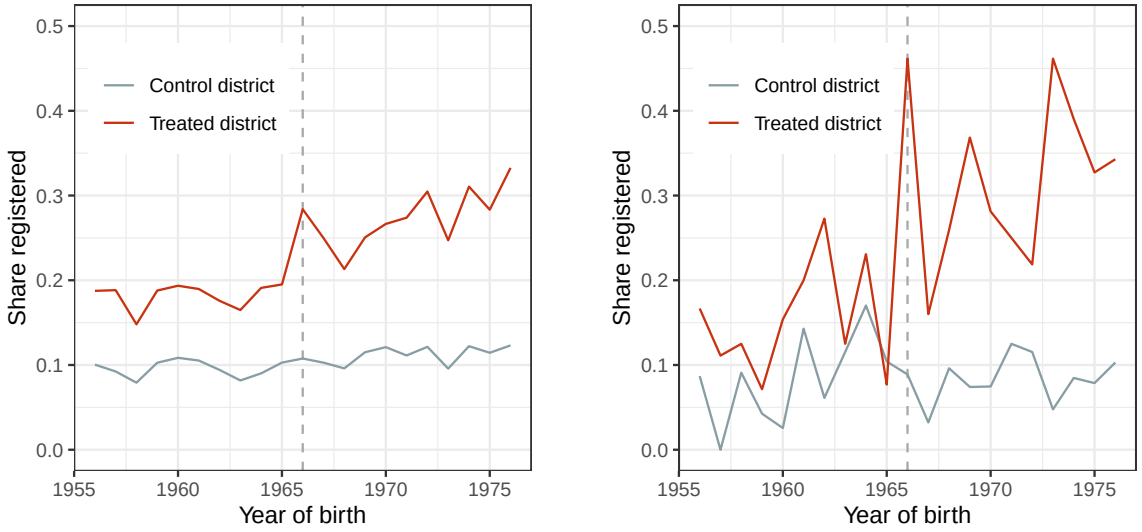


Figure plots the spatial distribution of *treated* (red) and *control* (gray) districts used in the baseline analysis. District boundaries as defined in 2012 Population and Housing Census.

Figure A.5: Trends in registration rates



(a) Census

(b) National Panel Survey

Figures display the average share of registered individuals across treated and control districts over time. Panel (a) uses the baseline Census sample; Panel (b) uses the baseline National Panel Survey sample.

Figure A.6: First stage district jackknife

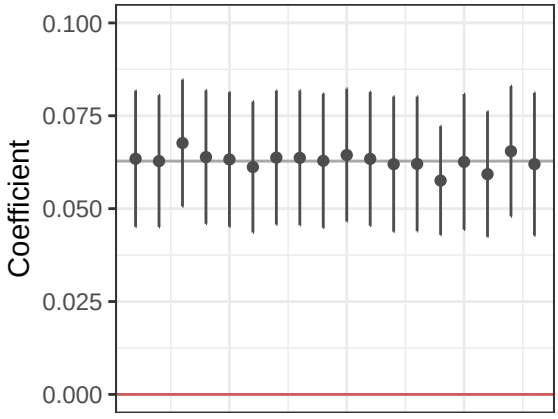


Figure estimates a jackknife of the the first stage coefficient while sequentially dropping each treated district. 95% confidence intervals provided. Horizontal gray line provides the overall first stage coefficient.

Figure A.7: Effects on education access by grade

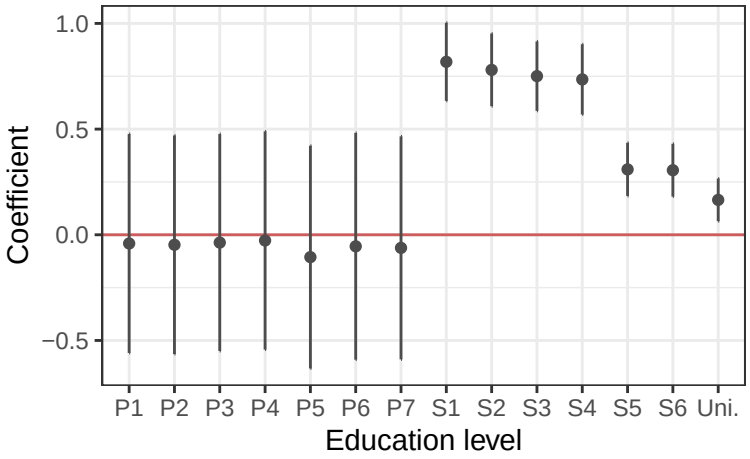


Figure estimates treatment effects on having completed a given grade of education. Primary education runs from P1 to P7; Secondary education runs from S1 to S4 ('Ordinary level') or to S6 ('Advanced level'). 95% confidence intervals provided.

Figure A.8: Trends in education access

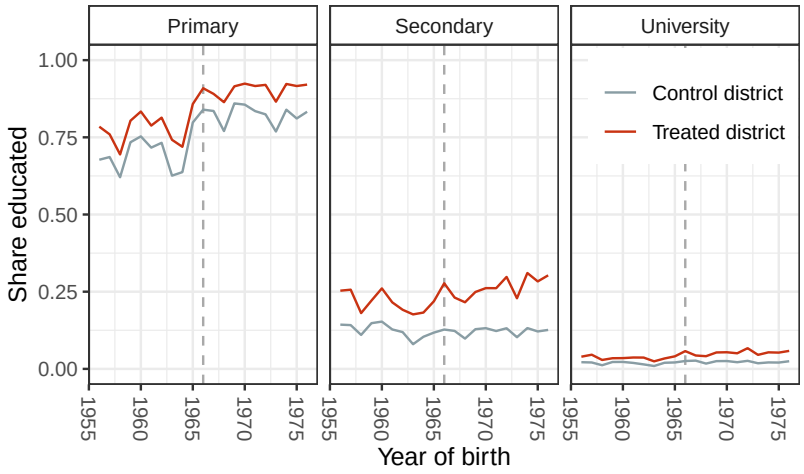


Figure displays the average value of variables measuring access to education across treated and control districts over time using the baseline Census sample.

Figure A.9: Trends in social security access

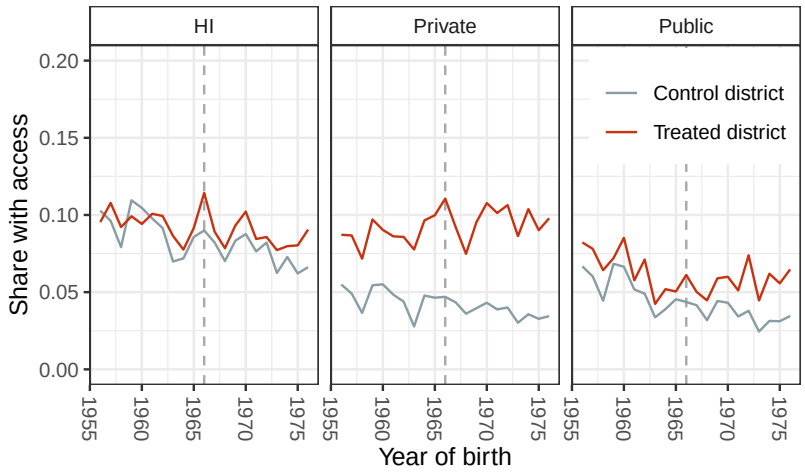


Figure displays the average value of variables measuring access to social security across treated and control districts over time using the baseline Census sample.

Figure A.10: Trends in tax payment

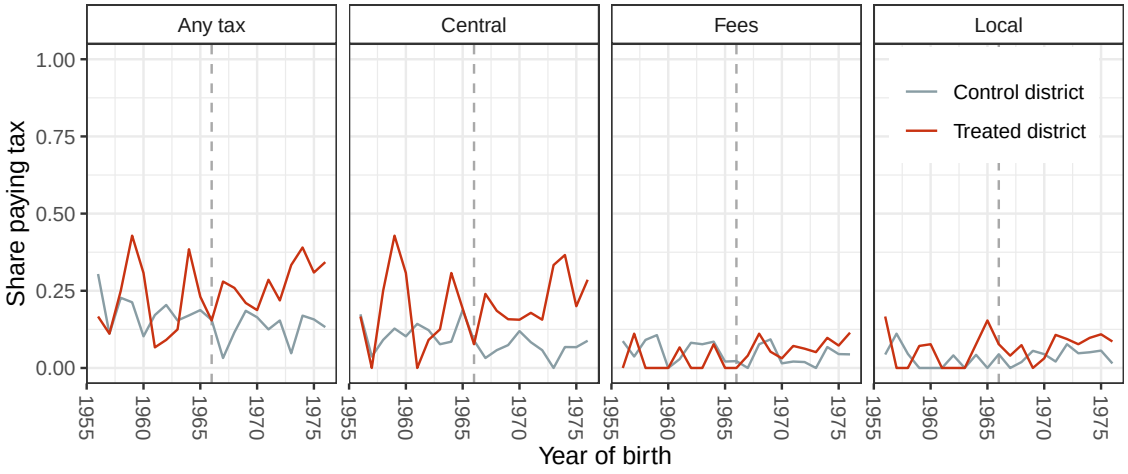


Figure displays the average value of variables measuring tax payment across treated and control districts over time using the baseline NPS sample.

Figure A.11: Expansion of compulsory registration across districts over time

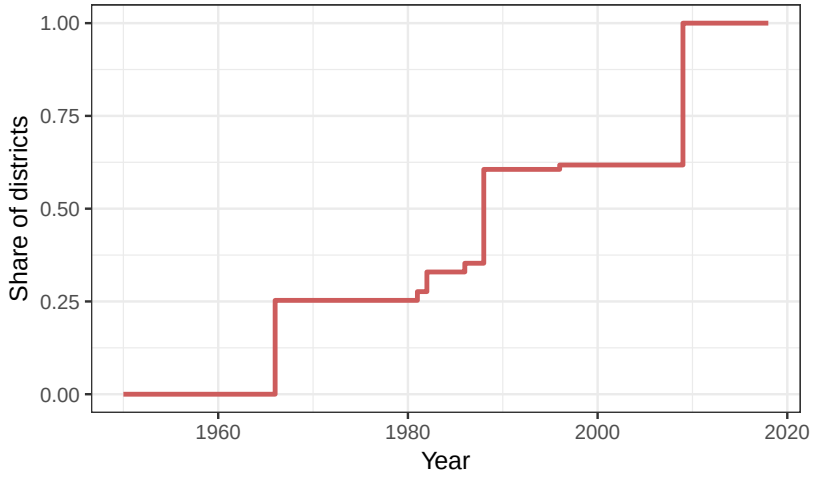


Figure displays the cumulative share of districts targeted by a compulsory registration order by year.

Figure A.12: Relevant legislation during reform period

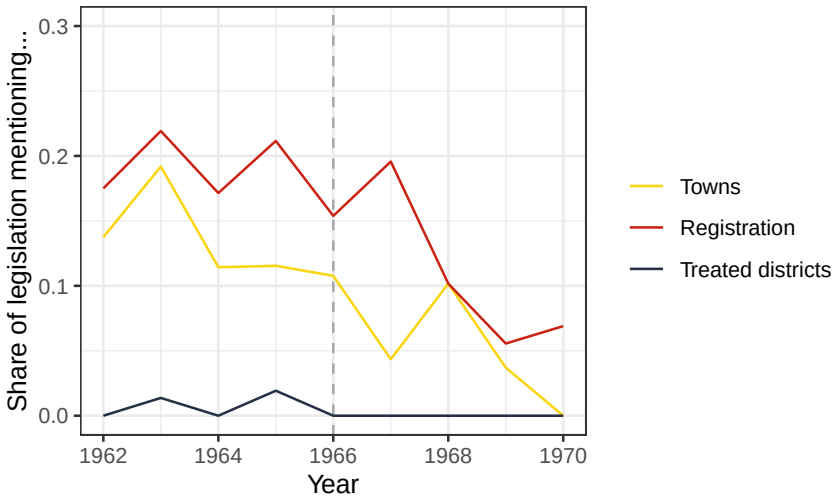


Figure displays the share of relevant legislation being passed in a given year.

A.3 Additional tables

Table A.1: Descriptive statistics (Census)

	Both		Treated		Control	
	Mean	SD	Mean	SD	Mean	SD
A. Sample characteristics						
Age	45.01	5.71	44.35	5.57	45.22	5.74
Male	0.48	0.50	0.48	0.50	0.47	0.50
Tanzanian citizen	0.99	0.08	0.99	0.10	0.99	0.07
Father alive	0.38	0.48	0.40	0.49	0.37	0.48
Mother alive	0.60	0.49	0.61	0.49	0.60	0.49
B. First stage variables						
Registered	0.14	0.34	0.24	0.43	0.10	0.30
Born in treated district	0.24	0.43	1.00	0.00	0.00	0.00
Born after reform	0.63	0.48	0.68	0.47	0.62	0.49
Reform	0.16	0.37	0.68	0.47	0.00	0.00
C. Outcome variables						
Education: Primary	0.79	0.41	0.86	0.35	0.77	0.42
Education: Secondary	0.15	0.36	0.24	0.43	0.12	0.32
Education: University	0.03	0.16	0.05	0.21	0.02	0.14
National Health Insurance Fund	0.08	0.27	0.09	0.28	0.08	0.27
Private pension	0.05	0.22	0.09	0.29	0.04	0.19
State pension	0.04	0.20	0.06	0.23	0.04	0.19
Observations	182635		44124		138511	

Data source is the the 2012 National Population and Housing Census. Sample restricted to cohorts born within ten years of reform in either *treated districts* or *control districts*.

Table A.2: Descriptive statistics (NPS)

	Both		Treated		Control	
	Mean	SD	Mean	SD	Mean	SD
A. Sample characteristics						
Age	41.85	5.82	40.98	5.53	42.28	5.91
Male	0.47	0.50	0.48	0.50	0.47	0.50
Parent has primary education	0.59	0.49	0.69	0.46	0.55	0.50
Parent has secondary education	0.09	0.28	0.16	0.37	0.05	0.22
Parent has university education	0.02	0.15	0.05	0.22	0.01	0.10
B. First stage variables						
Registered	0.15	0.35	0.27	0.45	0.08	0.28
Born in treated district	0.32	0.47	1.00	0.00	0.00	0.00
Born after reform	0.66	0.47	0.73	0.45	0.63	0.48
Reform	0.24	0.42	0.73	0.45	0.00	0.00
C. Outcome variables						
Paid any tax	0.19	0.39	0.27	0.44	0.15	0.36
Paid fees	0.05	0.22	0.06	0.23	0.05	0.21
Paid local tax	0.05	0.21	0.07	0.26	0.04	0.18
Paid central tax	0.13	0.34	0.22	0.41	0.09	0.28
Observations	1468		477		991	

Data source is National Panel Survey (2010, 2014). Sample restricted to cohorts born within ten years of reform in either *treated districts* or *control districts*.

Table A.3: Assigning district of birth to treatment

District (1966)	District (2012)	District (1966)	District (2012)
Arusha	**Arusha Urban**	Morogoro	**Morogoro Urban**
Arusha	Arusha Rural	Morogoro	Morogoro Rural
Arusha	Meru	Morogoro	Longido
Bukoba	**Bukoba Urban**	Morogoro	Mvomero
Bukoba	Bukoba Rural	Mtwara	**Mtwara Urban**
Bukoba	Muleba	Mtwara	Mtwara Rural
Bukoba	Misenye	Musoma	**Musoma Urban**
Dodoma	**Dodoma Urban**	Musoma	Musoma Rural
Dodoma	Dodoma Rural	Musoma	Bunda
Dodoma	Bahi	Musoma	Butiama
Iringa	**Iringa Urban**	Musoma	Serengeti
Iringa	Iringa Rural	Mwanza	**Ilemela**
Iringa	Kilolo	Mwanza	**Nyamagana**
Kigoma	**Kigoma Urban**	Mwanza	Busega
Kigoma	Kigoma Rural	Mwanza	Magu
Kigoma	Uvinza	Tabora	**Tabora Urban**
Kilimanjaro	**Moshi Urban**	Tabora	Kaliua
Kilimanjaro	Moshi Rural	Tabora	Sikonge
Lindi	**Lindi Urban**	Tabora	Urambo
Lindi	Lindi Rural	Tabora	Uyui
Lindi	Ruangwa	Tanga	**Tanga Urban**
Mbeya	**Mbeya Urban**	Tanga	Mkinga
Mbeya	Mbeya Rural	Tanga	Muheza
Mbeya	Mbarali		
Mzizima	**Ilala**		
Mzizima	**Kinondoni**		
Mzizima	**Temeke**		

Table lists all treated districts (bold) and all control districts (non-bold) in the baseline specification.

Table A.4: Assigning district of birth to treatment (supplementary)

A.	Township	District in 2012 Census
	Arusha	Arusha Urban
	Bukoba	Bukoba Urban
	Dar es Salaam	Ilala, Kinondoni, Temeke
	Dodoma	Dodoma Urban
	Iringa	Iringa Urban
	Kigoma	Kigoma Urban
	Lindi	Lindi Urban
	Mbeya	Mbeya Urban
	Morogoro	Morogoro Urban
	Moshi	Moshi Urban
	Mtwara	Mtwara Urban
	Mwanza	Ilemela, Nyamagana
	Tabora	Tabora Urban
	Tanga	Tanga
B.	'Former township'	District in 2012 Census
	Bagamoyo	Bagamoyo
	Chunya	Chunya
	Kahama	Kahama Urban
	Kilosa	Kilosa
	Kimamba	Kilosa
	Kondoa	Kondoa
	Korogwe	Korogwe Urban
	Lushoto	Lushoto
	Mpwapwa	Mpwapwa
	Nachingwea	Nachingwea
	Nansio	Ukerewe
	Pangani	Pangani
	Shinyanga	Shinyanga Urban
	Singida	Singida Urban
	Songea	Songea Urban
	Tukuyu	Rungwe

Table lists all locality classified as urban in the 1967 Census (Volume II). All districts in Panel A had the reform applied. Districts in Panel B did not have the reform applied. Employed to define control districts in Panel D of Table B.11.

Table A.5: First stage (robustness)

	+/- 5 cohorts			All cohorts		
A. Varying included cohorts	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.05***	0.05***	0.06***	0.16***	0.14***	0.04***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Time trends	None	Region	District	None	Region	District
Outcome mean	0.13	0.13	0.13	0.25	0.25	0.25
Observations	96884	96884	96884	1320543	1320543	1320543
	-Reform year			**-Heaped ages**		
B. Excluding birth years	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.06***	0.05***	0.03***	0.07***	0.06***	0.07***
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Time trends	None	Region	District	None	Region	None
Outcome mean	0.14	0.14	0.14	0.15	0.15	0.15
Observations	176747	176747	176747	131211	131211	131211
	District-level			**Individual-level**		
C. Controls	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.05***	0.05***	0.03***	0.06***	0.05***	0.04***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Time trends	None	Region	District	None	Region	District
Outcome mean	0.14	0.14	0.14	0.14	0.14	0.14
Observations	182635	182635	182635	182635	182635	182635
	Urban			**Unrestricted**		
D. Changing control districts	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.05***	0.04***	0.04***	0.06***	0.05***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Time trends	None	Region	District	None	Region	District
Outcome mean	0.15	0.15	0.15	0.10	0.10	0.10
Observations	110969	110969	110969	619982	619982	619982
	+/- 10 cohorts			All cohorts		
E. National Panel Survey dataset	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.16***	0.16***	0.23***	0.12***	0.14***	0.17***
	(0.04)	(0.03)	(0.06)	(0.03)	(0.03)	(0.05)
Time trends	None	Region	District	None	Region	District
Outcome mean	0.23	0.23	0.23	0.36	0.36	0.36
Observations	1468	1468	1468	5327	5327	5327

DV: respondent has a birth certificate. Panel A: sample restricted to cohorts born within 5 year of reform, or no restriction. Panel B: excluding individuals born in reform year, or those who report their age rounded to five years. Panel C: interacting vector of district-level controls with post-reform year indicator, or linearly adding additional individual-level controls. Panel D: redefining the set of control districts to comprise other districts classified as urban in 1967 census, or all other districts in the country. Panel E: replicating the first stage using the NPS sample.

Table A.6: First stage (alternative estimation)

A. Regression discontinuity	(1)	(2)	(3)	(4)	(5)	(6)
Born after reform	0.10***	0.04	0.03**	0.03**	0.01	0.00
	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Bandwidth	2	5	10	15	20	30
Outcome mean	0.22	0.22	0.24	0.27	0.30	0.37
Observations	10827	22689	44124	72740	106205	190696
B. Household fixed effects	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.06	0.04	0.05***	0.08***	0.11***	0.17***
	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Bandwidth	2	5	10	15	20	30
Outcome mean	0.13	0.13	0.14	0.15	0.16	0.21
Observations	45574	96884	182635	284823	396047	660253

Outcome variable is whether respondent has a birth certificate. Data source is the 2012 national census. Panel A: estimated using a local linear regression in the set of treated districts. Coefficient represents the change in registration probability among cohorts just after, versus just before, the reform. Panel B: Equation (2.2) estimated using household-level fixed effects.

* p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors clustered at the district of birth-level in parentheses.

Table A.7: Placebo outcomes

	Male	Tanzanian	Father alive	Mother alive
	(1)	(2)	(3)	(4)
Reform	0.01	-0.00	0.00	0.00
	(0.01)	(0.00)	(0.01)	(0.01)
Outcome mean	0.477	0.994	0.376	0.602
Observations	182635	182635	182635	182635

Table uses individual-level characteristics as dependent variables in Equation (2.2). DVs: (1) individual is male; (2) individual is Tanzanian; (3) individual's father is alive; (4) individual's mother is alive.

Specifications estimated using OLS including district of birth and year of birth fixed effects and control for gender. Exposure to reform is an indicator for being born after reform in a *treated* district. SEs clustered at the district of birth-level in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table A.8: Effects on literacy

	Any (1)	Kisw. (2)	Eng. (3)
Registered (β^{OLS})	0.12***	0.11***	0.33***
	(0.01)	(0.01)	(0.01)
$\widehat{\text{Registered}}$ (β^{IV})	-0.09	-0.08	0.58***
	(0.24)	(0.24)	(0.11)
DV Mean	0.78	0.78	0.16
DV SD	0.41	0.42	0.37
FS F-statistic	50.7	50.7	50.7
ρ(Income, DV)	0.15	0.14	0.32
Observations	182635	182635	182635

DVs are all indicators. (1) individual is literate in any language; (2) individual is literate in Kiswahili; (3) individual is literate in English.
ρ(Income, DV) provides the coefficient from a regression of the standardized DV onto a standardized measure of asset ownership.
β^{OLS} estimated using Equation (2.1); β^{IV} estimated using Equation (2.3). All specifications include district of birth and year of birth fixed effects and control for gender. SEs clustered at the district of birth-level in parentheses. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table A.9: Effects on access to social security

			Private		State			
	Any	HI	NSSF	PPF	PSPF	GEPF	LAPF	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Registered (β^{OLS})	0.22***	0.11***	0.08***	0.03***	0.08***	0.01***	0.02***	0.01***
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
$\widehat{\text{Registered}}$ (β^{IV})	0.24**	0.12	0.18***	-0.04	0.10*	0.03**	0.03	0.01
	(0.11)	(0.07)	(0.05)	(0.04)	(0.05)	(0.01)	(0.02)	(0.02)
DV Mean	0.15	0.08	0.04	0.01	0.03	0.00	0.01	0.02
DV SD	0.36	0.27	0.20	0.11	0.18	0.05	0.09	0.12
FS F-statistic	50.7	50.7	50.7	50.7	50.7	50.7	50.7	50.7
ρ(Income, DV)	0.30	0.18	0.22	0.15	0.19	0.06	0.11	0.03
Observations	182635	182635	182635	182635	182635	182635	182635	182635

DVs are all indicators. (1) individual in a household with access to any social security fund; (2) individual in a household with access to National Health Insurance Fund (NHIF); (3) individual in a household with access to National Social Security Fund (NSSF); (4) individual in a household with access to Parastatal Pension Fund (PPF); (5) individual in a household with access to Public Service Social Security Fund (PSSSF); (6) individual in a household with access to Government Employees Provident Fund (GEPF); (7) individual in a household with access to Local Authorities Pension Fund (LAPF); (8) individual in a household with access to any other social security fund.
ρ(Income, DV) provides the coefficient from a regression of the standardized DV onto a standardized measure of asset ownership.
β^{OLS} estimated using Equation (2.1); β^{IV} estimated using Equation (2.3). All specifications include district of birth and year of birth fixed effects and control for gender. SEs clustered at the district of birth-level in parentheses. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table A.10: Effects on exposure to taxation (all cohorts)

	All	Fees	Local	Central
	(1)	(2)	(3)	(4)
Registered (β^{OLS})	0.10***	0.00	0.04***	0.09***
	(0.01)	(0.01)	(0.01)	(0.01)
$\widehat{\text{Registered}}$ (β^{IV})	0.46**	0.26*	0.02	0.42**
	(0.20)	(0.14)	(0.15)	(0.19)
DV Mean	0.20	0.05	0.05	0.13
DV SD	0.40	0.22	0.21	0.34
FS F-statistic	21.3	21.3	21.3	21.3
Observations	5326	5326	5326	5326

DVs are all indicators. (1) individual in a household which has paid any tax in the last year; (2) individual in a household which has paid any fees to the government in the last year; (3) individual in a household which has paid council rates in the last year; (4) individual in a household which has paid taxes to the central government in the last year.
NPS sample restricted to all adults born in either *treated* or *control* districts.
β^{OLS} estimated using Equation (2.1); β^{IV} estimated using Equation (2.3). All specifications include district of birth and year of birth fixed effects and control for gender. SEs clustered at the district of birth-level in parentheses. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table A.11: Comparison of registration reform effects

	Pooled	'66	'80s	'09
	(1)	(2)	(3)	(4)
Reform	0.16***	0.06***	-0.01**	0.00
	(0.02)	(0.01)	(0.01)	(0.00)
Reform × '80s	-0.16***			
	(0.02)			
Reform × '09	-0.17***			
	(0.02)			
Outcome mean	0.19	0.10	0.18	0.24
Observations	4331868	582086	1673553	1931778

Table estimates the effects of the '66 reform, '80s reforms, and '09 reform on registration as discussed in Appendix A.1.2. Column (1) estimates using the complete census sample and tests for differences between the effects of later reforms compared to '66. Columns (2) to (4) separately estimate effects of each reform, restricting each sample to cohorts born close to reform year. All specifications are estimated using OLS and include district of birth and year of birth fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the district of birth-level in parentheses.

B | Appendix to Chapter 3

B.1 Supplementary information

B.1.1 Historical context of identity registration

A literature in historical demography points to the scarcity of colonial efforts to register citizens (Kuczynski 1948; Szreter and Breckenridge 2012; Cooper 2016).[1] This informational vacuum stemmed from colonial structures designed to control trade and extract economic surplus rather than to implement policy evenly across thinly settled territories (Young 1994; Cooper 1996; Herbst 2000). Administration was typically exercised at the community-level, working through local leaders, rather than at the individual-level (Almond and Coleman 1961). Even in French colonies, associated with more 'direct' rule, efforts to expand the *état civil* to local populations were extremely limited until the final years of colonial rule (Cooper 2012). Information, when it rarely *was* solicited from individuals, was usually tied to the demands of taxation (Shaul 1952; Ittmann, Cordell, and Maddox 2010). As a result, states generally inherited populations wary of registration and the absence of state capabilities to exert uniform control over their territories.

While these states faced relatively similar initial informational conditions, the subsequent targeting of their investments to identify and register citizens differed. Many of these states were—at least initially—highly developmentalist with leaders who predicated their economic development strategies on intensive state intervention (Mkandawire 2001). As

[1] While settler colonies, such as South Africa, often exerted substantial efforts to register subjects to control internal movement and labor supply (Breckenridge 2014), these were the exception rather than the rule.

part of this, the registration and identification of citizens was often considered a necessary condition for modernization and the construction of a rational bureaucratic apparatus (Podlewski 1971; Serra 2018).[2] Administration in non-targeted areas tended to be exercised at the community-level, relying upon political intermediaries and local monitoring structures. Assessing taxation and evaluating voter eligibility, for example, was considered far simpler in rural communities with defined local leaders and comparatively immobile populations (Mackenzie and Robinson 1960; Due 1963). Similarly, the substantial challenge of ascertaining citizenship in the early post-independence era was rendered especially challenging by heterogeneous urban networks (Peil 1971). Anonymity to the state posed a larger challenge in localities lacking such monitoring structures.[3]

B.1.2 Data geolocation and treatment assignment

Census data. I record all locality-level variables reported in the 1970 census gazette for localities with a total population above 3,000 ($n = 275$). The variables reported in the census include a number of demographic and socioeconomic variables. Using census documentation I digitize maps of all Regions and *Local Councils*, as existing in 1970, and record additional information on whether *Local Councils* are classified as *Local*, *Urban*, *Municipal*, or *City*.[4]

[2]Serra (2018) points to the profusion of censuses undertaken by newly independent states in the 1960s, arguing that "for the newly independent states, the collection, construction, and dissemination of statistics came to be seen as an important precondition of economic and social modernization" (p.665). This link similarly drove the expansion of identification systems in now-developed countries: modernizing pressures, with the breakdown of traditional community structures, necessitated state interventions to monitor and manage rapidly shifting populations (Caplan and Torpey 2001).

[3]The demographer Andre Podlewski, in 1971, alluded to the point:

> "Is it possible for a state to exist without a system of vital registration? The answer is decidedly in the affirmative; many people have lived (and are still living) without any official document authenticating their birth, their marriage and their death. However, these persons were not unknown in the societies to which they belonged... Nonetheless, such individuals were neither known nor recognized outside their immediate circle or territory. Thus, they could not leave without running considerable risks, as their status would not be recognized" (p.255)

[4]Councils were defined as *Urban*, or above, when they contained a locality with population above 20,000.

Using census maps and a number of other sources,[5] I geolocate all the localities with a population about 3,000. Figure 3.4a maps the localities.

Outcome data. For the primary outcome data source (Ghana Living Standards Surveys), I draw on on archival and administrative records to geolocate all enumeration areas from round 1 (1987, 178 EAs), 2 (1989, 170 EAs), 4 (1998, 300 EAs), 5 (2005, 580 EAs), 6 (2012, 1,200 EAs) and 7 (2017, 1,200 EAs).[6] Each survey constitutes a nationally representative sample and there is no evidence that the existence of registries affected survey sampling. This produces an overall sample of 250,000 individuals geolocated to around 3,500 EAs in the GLSS sample. Figure 3.4b maps the full set of EAs using the GLSS sample.

For the secondary outcome data source (Afrobarometer), I draw on geolocated data files relating to round 3 (2005, 150 EAs), 4 (2008, 150 EAs), 5 (2011, 300 EAs), 6 (2014, 300 EAs), 7 (2017, 300 EAs). Each survey constitutes a nationally-representative sample. This produces an overall sample of 9,600 individuals geolocated to around 1,200 EAs in the Afrobarometer sample.

Assigning enumeration areas to treatment. Localities, as defined in 1970, are 'treated' when their population exceeded 5,000 and 'control' when their population was below 5,000. Assigning EAs to treatment therefore rests on defining whether an EA is sufficiently spatially proximate to a treated, or control, locality.

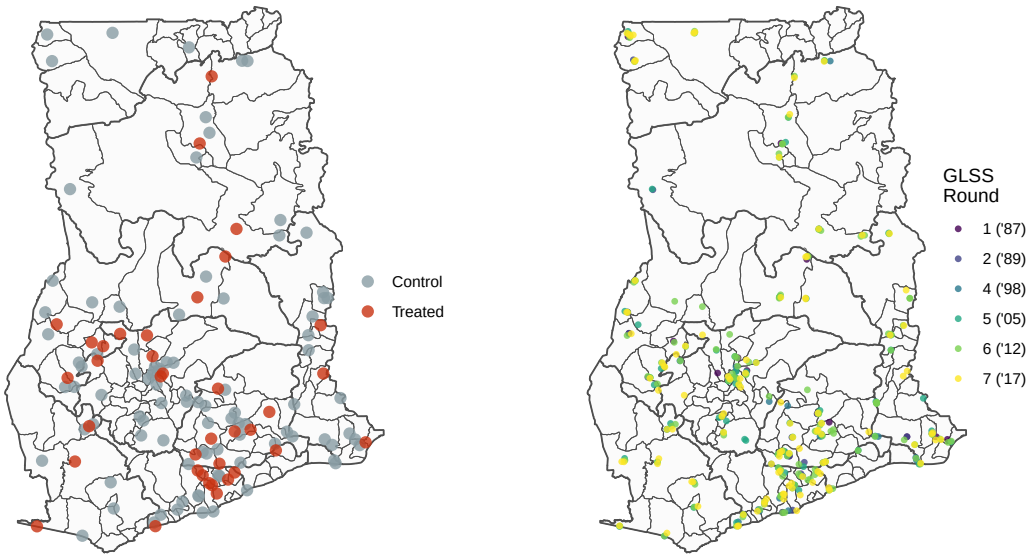
I impose two restrictions on the set of census localities included in the discontinuity sample. First, I exclude all census localities with a population above 7,000, or those inside *Urban Councils* or above, with the justification that localities in such councils are often peripheral settlements to major urban centers and hence *de facto* treated. This restriction applies to 27/143 Local Councils. Second, I restrict the set of census localities to be only those at least 5 km from another census locality with *different* treatment status. The

[5]Sources for geolocation include documentation from the 1960, 1970 and 1984 censuses, the Africapolis OECD database, and the website *Ghana Place Names*.

[6]Based on discussions with individuals involved in the implementation of round 3 (1991), it appears that those enumeration area maps have now unfortunately been lost.

intuition for this restriction is similar to the first restriction: if a control locality is very close to a treated locality, it is *de facto* treated. Therefore I only consider census localities, in *Local Councils*, with population between 3,000-7,000, which are relatively far from other localities with differing treatment assignment. These restrictions leave 133 census localities in the discontinuity sample. Figure B.1a plots the spatial distribution of resulting treated and control localities.

Figure B.1: Treatment assignment



(a) Census discontinuity sample

(b) GLSS discontinuity sample

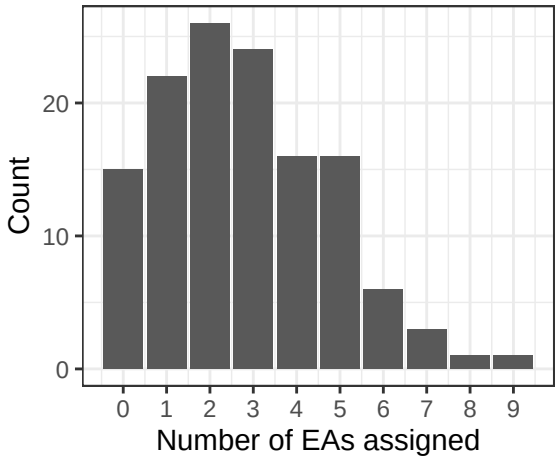
Thick lines represent 1970 region boundaries; thin lines represent 1970 local council boundaries.

Then, to assign EAs to the discontinuity set of localities, I impose two conditions. First, for each EA, I compute its closest locality in the census data. I exclude all EAs matched to localities excluded by the above conditions: when the locality is above 7,000 population, inside an *Urban Council*, or close to another locality with differing treatment status. Second, for the remaining EAs, I consider an EA to match a census locality when it is within 5 kilometers (km) of the census locality.[7] This process generates a sample of 368 EAs,

[7]Due to its much smaller sample size, when using the Afrobarometer sample I increase the linking radius to 10 km.

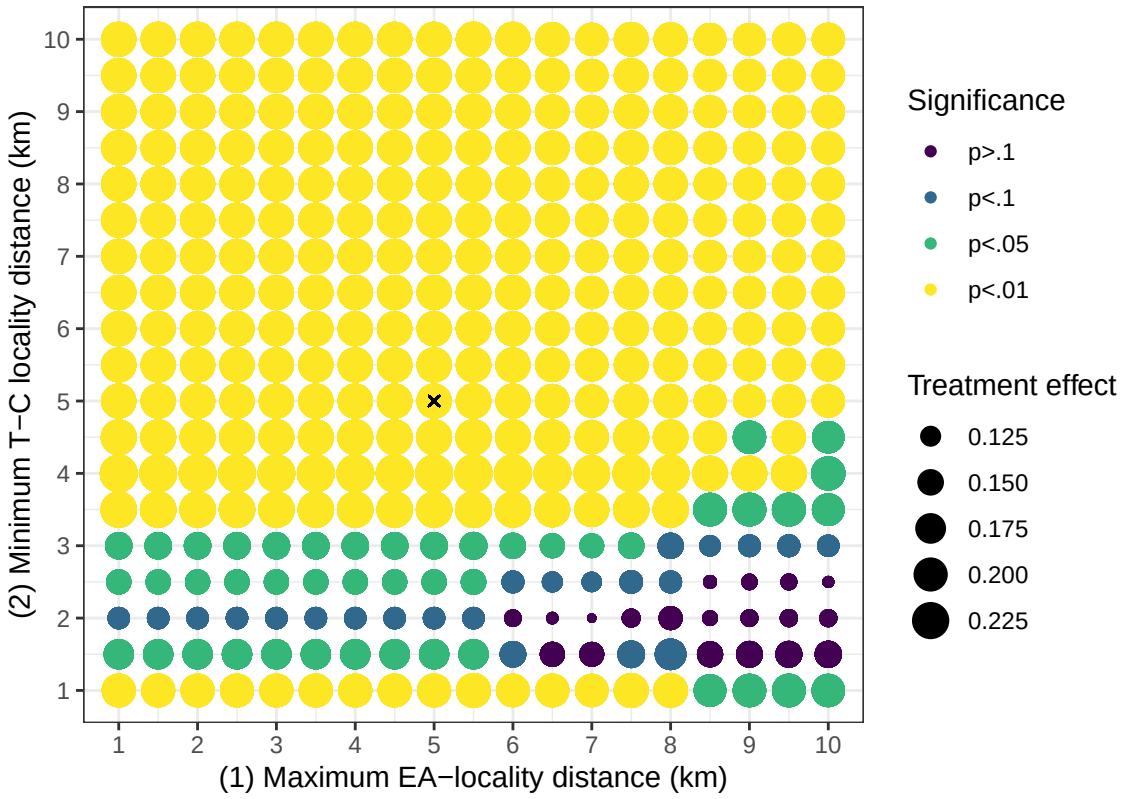
linked to 118 out of the 133 total potential census localities, containing data on over 12,000 individuals. The distribution of EAs, across all the GLSS waves, linked to a given census locality in the discontinuity sample is plotted in Figure B.2. In the baseline sample, the average distance between a GLSS EA and its linked 1970 census locality is 1.9 km. Figure B.1b plots the spatial distribution of resulting EAs.

Figure B.2: Distribution of linked EAs per census locality in discontinuity sample



I assess the robustness of the main estimates to the two threshold distances employed in this data processing stage—i.e. (1) restricting to EAs within 5 km of a census locality, and (2) excluding census localities within less than 5 km of another census locality with differing treatment status. In Figure B.3, I estimate the baseline specification of the possession of identity documents with varying values of these two parameters on the different axes in 1 km increments between 1 and 10 km. The size of each circle represents the estimates treatment effect magnitude, while its color represents its relative degree of statistical significance. The figure suggests that the treatment effect is robust to varying either of these threshold distances.

Figure B.3: Treatment effect with varying distance thresholds for inclusion in discontinuity sample



Plot presents the estimated treatment effect from Table 3.1 while permuting (1) the maximum threshold distance for linking EAs to census localities; (2) the minimum threshold distance each census locality must be from another census locality with different treatment status. Size of circles represents estimated treatment coefficient, color represents relative degree of statistical significance of the coefficient. × indicates the thresholds used in the analysis.

B.1.3 Aggregate effects on local development

Existing research ties variation in local state capacity, whether measured by the physical presence of state agencies in a locality (Acemoglu, García-Jimeno, and Robinson 2015; Rogowski et al. 2020) or by local variation in legibility (D'Arcy and Nistotskaya 2017; Lee and Zhang 2017), with aggregate development outcomes. I assess whether there exists a discontinuous change in measures of local development at the threshold in three ways. First, using the GLSS data, I construct a standardized measure of welfare for each household across the waves between 1987 and 2017.[8] Second, I employ yearly data on night lights from the DMSP-OLS satelittes, which are widely used to measure local development outcomes (Henderson, Storeygard, and Weil 2012). For each available year, which comprises the period from 1992 to 2013, I construct a measure of local light intensity around sample localities.[9] Third, I use granular estimates of locality population over time using satellite-based population data from LandScan, which estimates annual population counts in every one square kilometer cell of a global grid from 2000 to 2018 (Dobson et al. 2000).[10]

I plot standardized estimates of β^{RD} at the threshold, using versions of Equation (3.1), in Figure B.4. Estimates using the welfare data (Panel A) provide no evidence of treatment effects on average consumption. Estimates using the satellite data (Panel B) provide no evidence of imbalances in night light brightness. Estimates using the population data (Panel C) provide little evidence of effects on locality size over time. Across the outcome measures, there is consequently little evidence of the treatment affecting average local development outcomes.[11]

[8] I pool rounds 1 (1987) and 2 (1989) due to the limited number of observations in each when estimating by-round treatment effects. These rounds were implemented as a pseudo-panel and some households are sampled twice. I include one observation per unique household.

[9] In line with the restrictions used for the GLSS sample, I draw a five kilometer buffer around every locality in the sample and compute the (log+1) average value of the night lights in a given year within that buffer. Results are robust to using linear, or binary, outcome specifications.

[10] Again, I draw a five kilometer buffer around every locality in the sample and compute the (log+1) total population of all the cells within the buffer. Results are robust to using linear outcome specifications.

[11] Estimating aggregate effects on each outcome, pooled across years, also suggests an absence of any treatment effect.

Figure B.4: Aggregate development outcomes

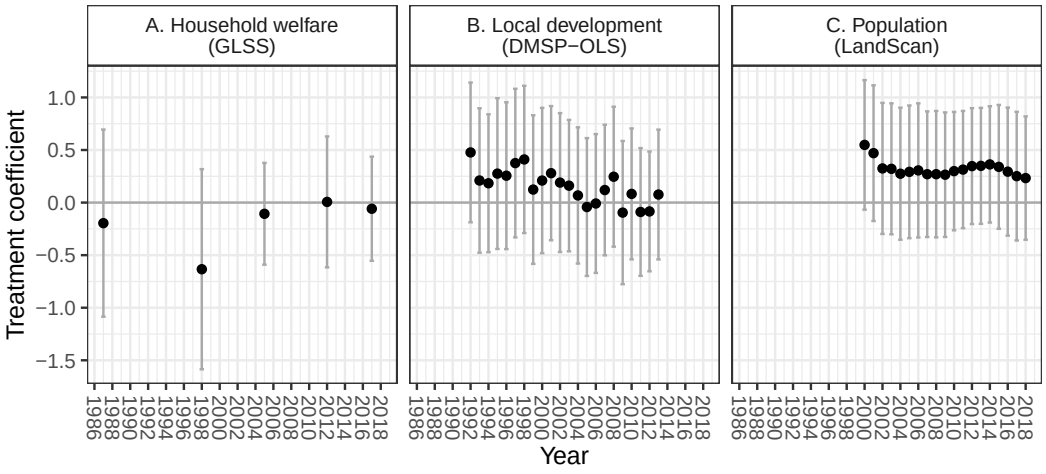


Figure estimates β^{RD} using versions of the baseline Equation (3.1). Both specifications include region fixed effects. 95% confidence intervals plotted.

B.1.4 Predicting income

There might be residual concerns regarding the use of observed household consumption in Equation (3.2). For example, while *average* consumption is balanced at the threshold, I find some evidence that the *variance* of income is lower just above the threshold compared to just below. This implied reduction in economic inequality is consistent with theoretical expectations. Therefore, to address these residual concerns, I use tools from machine learning to construct a *predicted* measure of per-capita household consumption. The basic approach is to train predictive algorithms using plausibly pre-treatment covariates in control localities, and then to fit them to data in the treated localities.

First, I subset the data to only comprise household heads in control localities (i.e. population of locality in 1970 was below 5,000) and take a vector of individual-level characteristics which are plausibly orthogonal to treatment assignment. These characteristics comprise (1) region of birth; (2) survey round; (3) age; (4) ethnicity; (5) gender; (6) educational attainment of respondent's parents. I restrict the exercise to comprise household heads since consumption is only observed at the household-level. I then fully interact these variables to-

gether into a high-dimensional matrix, and drop all predictors which are either low-variance or highly correlated ($\rho > 0.95$).

Using this matrix pertaining to respondents assigned to control localities, I randomly sample an 80% training set and 20% hold-out set and select model parameters using five-fold cross-validation. In the table below I compare prediction performance across a number of different candidate algorithms by computing the root mean squared error (RMSE) and R^2 of the *predicted* hold-out values compared to the *observed* values in the hold-out partition.

Model	RMSE	R^2
OLS	1.14	0.11
LASSO	1.08	0.16
Random forest	1.06	0.18

Figure B.5: Predicted versus observed consumption

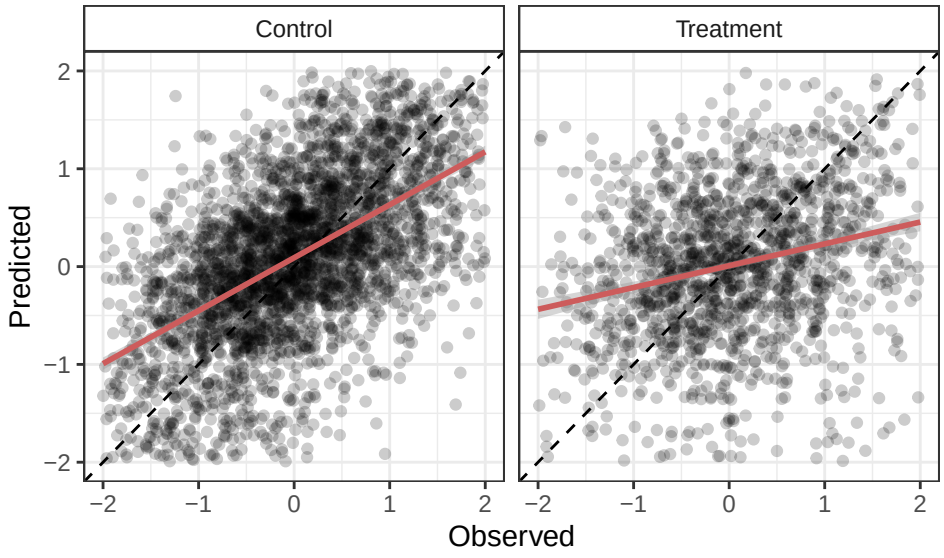


Figure plots observed standardized log consumption per household member (x-axis) against predicted standardized log consumption per household member (y-axis). Red line is a linear line of best fit.

This exercise suggests that random forest offers better performance, along both metrics, compared to simpler OLS or LASSO approaches, in predicting values in the hold-out

partition. Therefore I fit the same random forest model to the full discontinuity dataset—*including* observations in the treated localities—using the same set of pre-treatment characteristics that the model is trained on in control localities. Figure B.5 plots the observed values of consumption against predicted values. In terms of overall fit, the predicted values correlate with observed values at $\rho = 0.55$ in the pooled sample, $\rho = 0.64$ in control localities, and $\rho = 0.32$ in treated localities. Figure B.6 plots the distribution of the two measures of income.

Figure B.6: Distribution of income measures

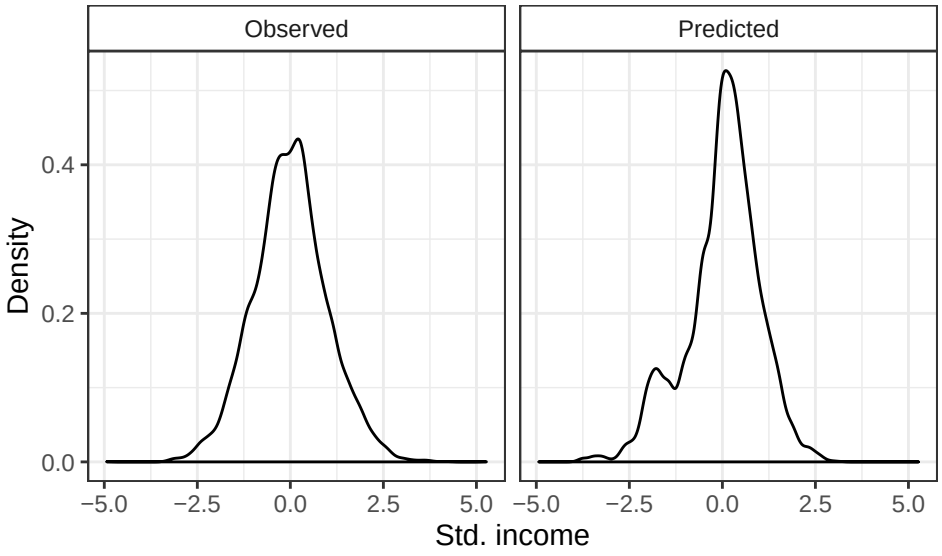


Figure plots density of *observed* standardized log consumption per household member and *predicted* standardized log consumption per household member. Both measures are standardized within-region.

B.1.5 Evidence on compound treatments

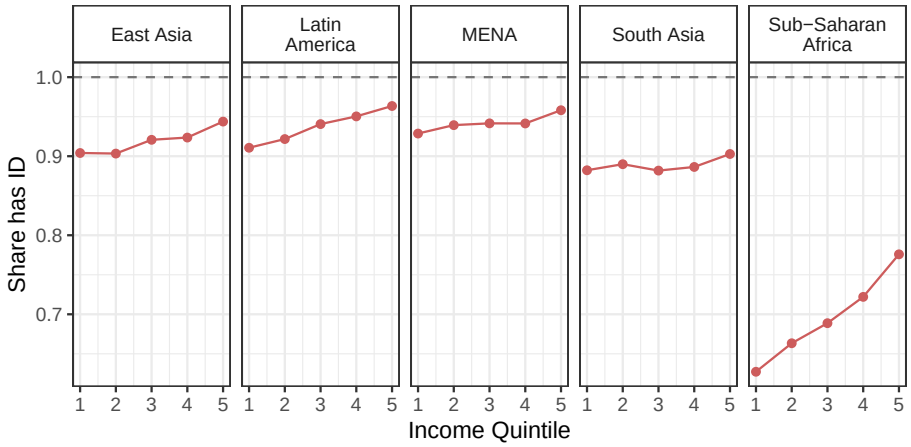
A common threat to inference using population-based thresholds for the deterministic assignment of policies relates to *compound treatments* (Gagliarducci and Nannicini 2013; Eggers et al. 2018). The key threat is that, if multiple policies 'switch on' at the same population threshold then it is hard to conclude that one policy is driving effects rather than the others. In the main text, I provide locality-level estimates using the 1984 census

to suggest the absence of compound treatments in the assignment of local public goods. I provide additional evidence here.

A set of proposed decentralization reforms, proposed in 1967/8 by three independent commissions and later implemented, proposed a four-tiered model of local government, with regional councils, district councils, local councils, and village committees (Mills-Odoi et al. 1967). None of these depended on proximate population thresholds: districts were to contain around 100,000 people (Tordoff 1994), local councils were classified as *urban councils* when they contained a locality with more than 20,000 citizens (Siriboe et al. 1968), and the structure of village councils, which were generally run by traditional authorities, was left to the discretion of regional commissioners with no evidence of population thresholds being used to determine their design (Mawhood 1983). Ayee (1994) reports that the 1988/89 decentralization reforms assigned a *town council* to localities with above 5,000 inhabitants, and an *area council* to localities with below 5,000. Since this assignment used the 1984 census, however, it does not represent a compound treatment when using the 1970 census.

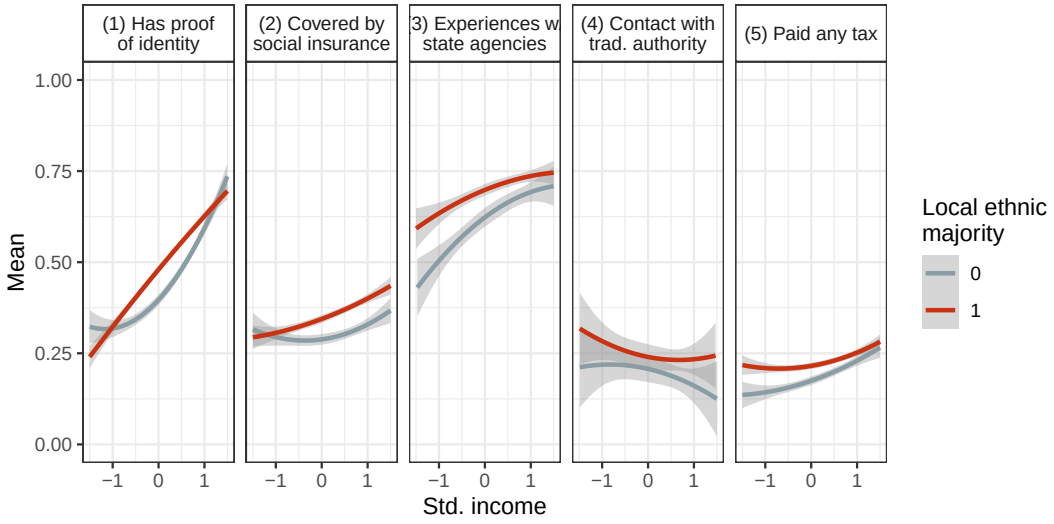
B.2 Additional figures

Figure B.7: Relationship between income and national ID possession



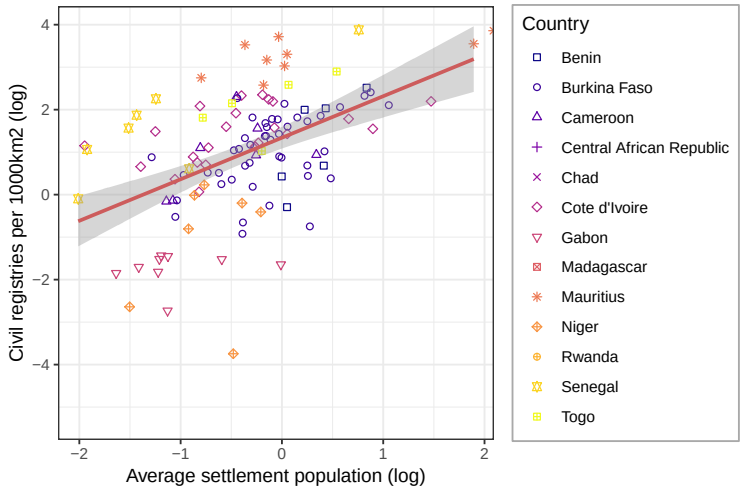
Source: 2018 Global Financial Inclusion database (Findex).

Figure B.8: State-citizen interaction and income disaggregated by local ethnic majority



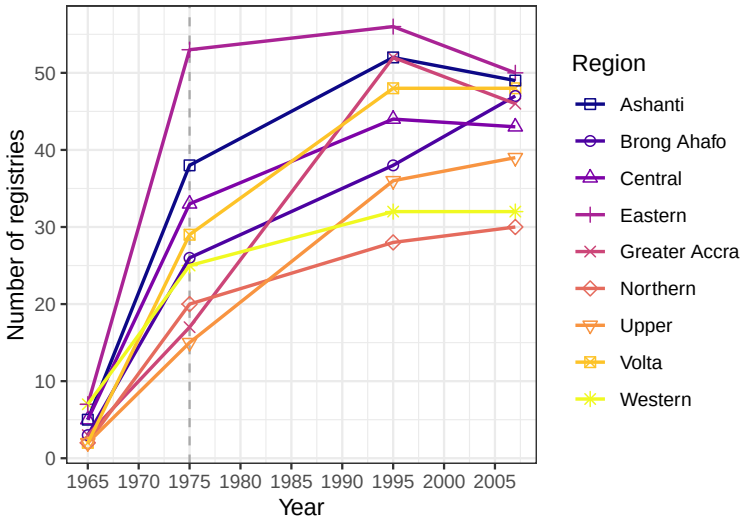
Note: X-axis provides a measure of household income depending on the data source used for each outcome. Panel (1), (2), (3), (5) use standardized (log) consumption per household member from Ghana Living Standards Survey data. Panel (4) uses an index of asset ownership from Afrobarometer.

Figure B.9: Subnational population density and registration infrastructure



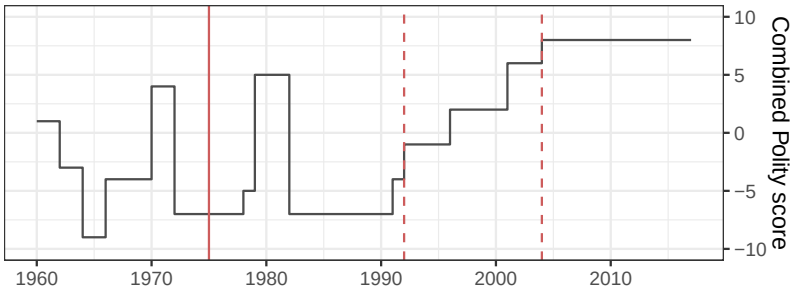
Source: Organisation Commune Africaine Malgache et Mauricienne (1974), World Bank. Y-axis measures standardized log number of registries per 1000 km^2. X-axis measures log average settlement size in each region. Each point corresponds to a different subnational region, with the marker type corresponding to different countries.

Figure B.10: Number of civil registries by region over time

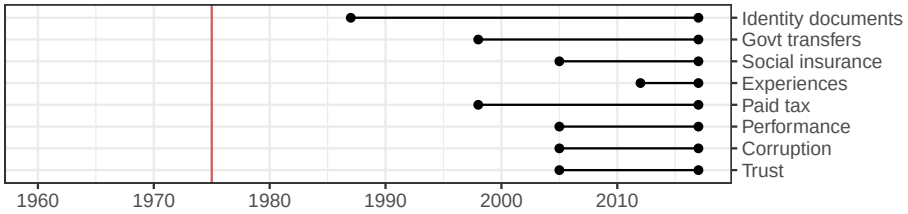


Source: Births and Deaths Registry of Ghana. Regions defined as in 1975.

Figure B.11: Timeline of outcomes and key events over time



(a) Regime transitions over time



(b) Outcomes measured over time

Note: Registries are constructed in 1975; multiparty democratic competition commences in 1992; new social policies begin to be administered in 2004. Top plot provides Polity score of Ghana over time. Bottom plot provides the temporal span for every outcome measure used in the main tables.

Figure B.12: Number of clusters as bandwidth increases

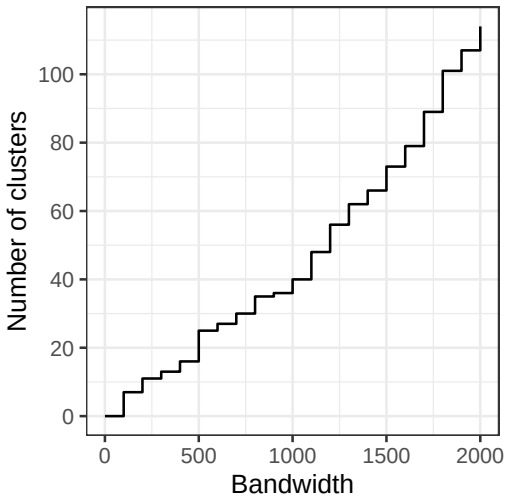
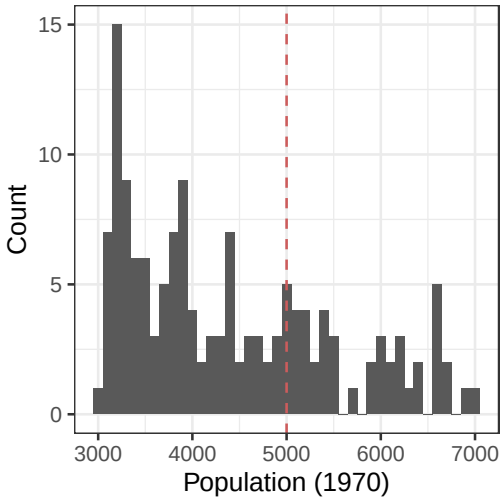
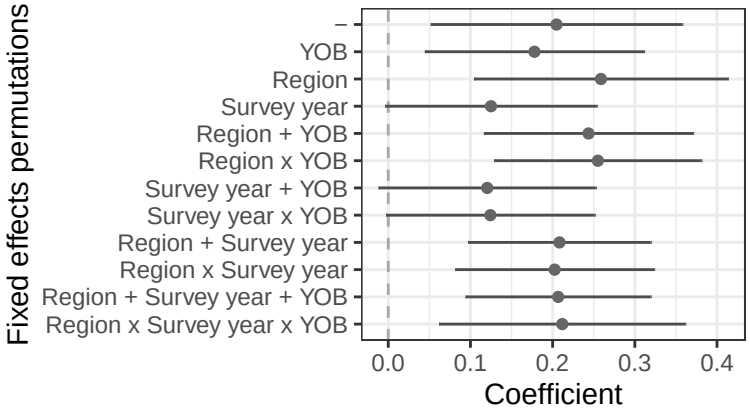


Figure B.13: Density of census localities around population threshold



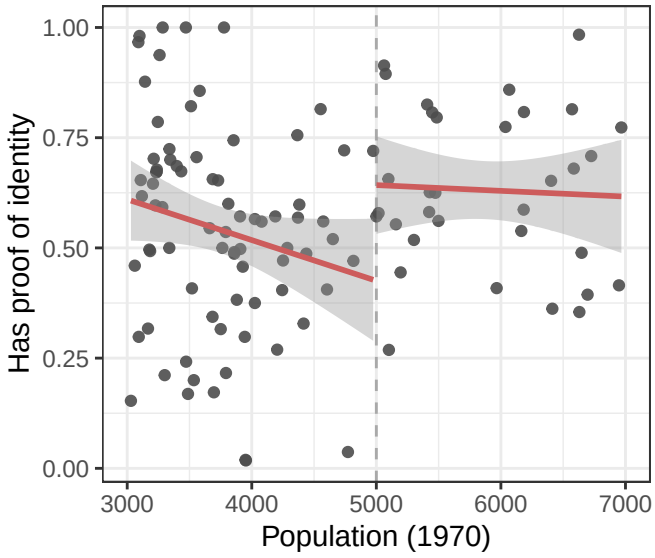
Note: Figure plots the density of localities in the 1970 census with between 3,000 and 7,000 population.

Figure B.14: Estimates with permutations of fixed effects



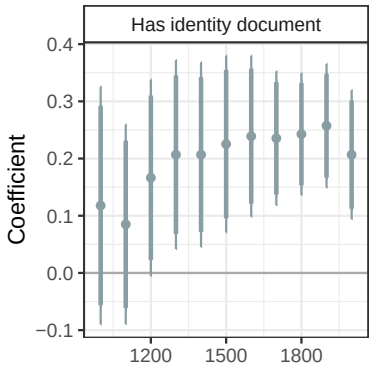
Note: Figure plots estimated treatment effects and 95% confidence intervals corresponding to Table 3.1 while permuting the inclusion of three levels of fixed effects (survey year, region, year of birth) both additively and interactively. Top row does not include any fixed effects in the estimation.

Figure B.15: Plot of treatment effect on possession of identity documents (raw data)



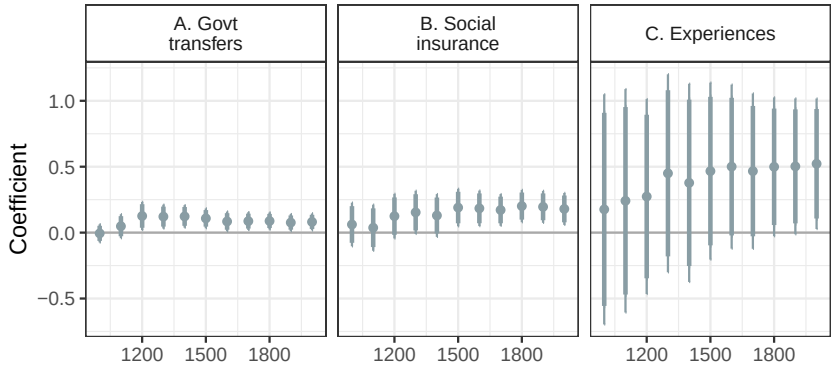
Note: Data aggregated to the 1970 locality-level.

Figure B.16: Varying bandwidth for different outcomes (Equation 3.1)

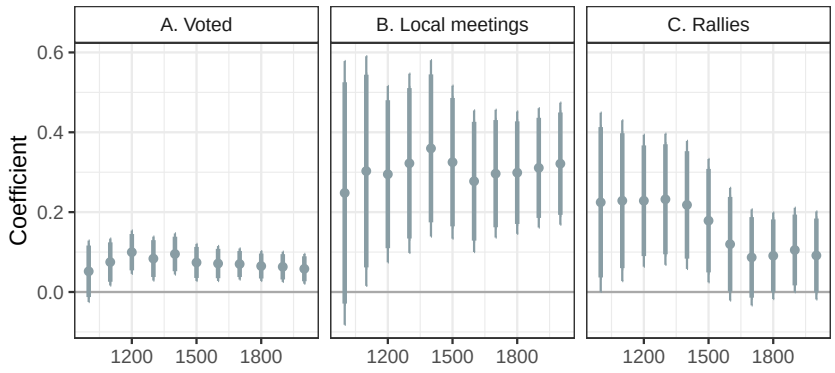


(a) Table 3.1 (Possession of identity documents)

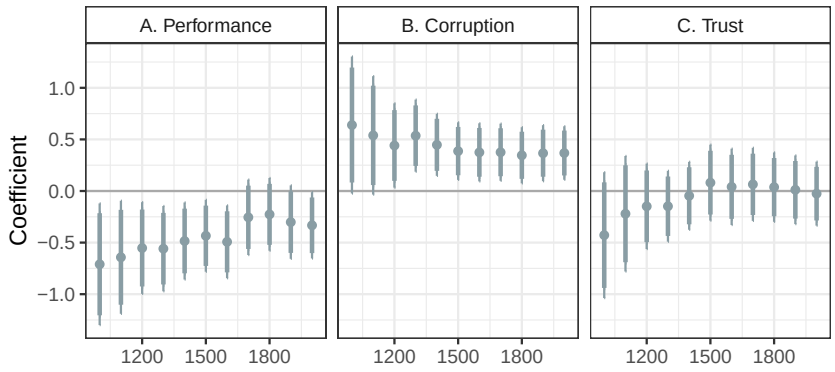
Figure B.17: Varying bandwidth for different outcomes (Equation 3.1)



(a) Table 3.2 (Access to the state)



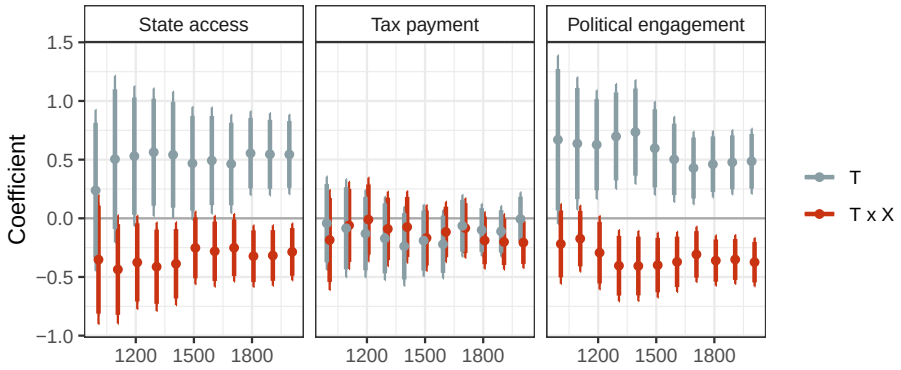
(b) Table 3.3 (Political engagement)



(c) Table 3.4 (Attitudes towards governance)

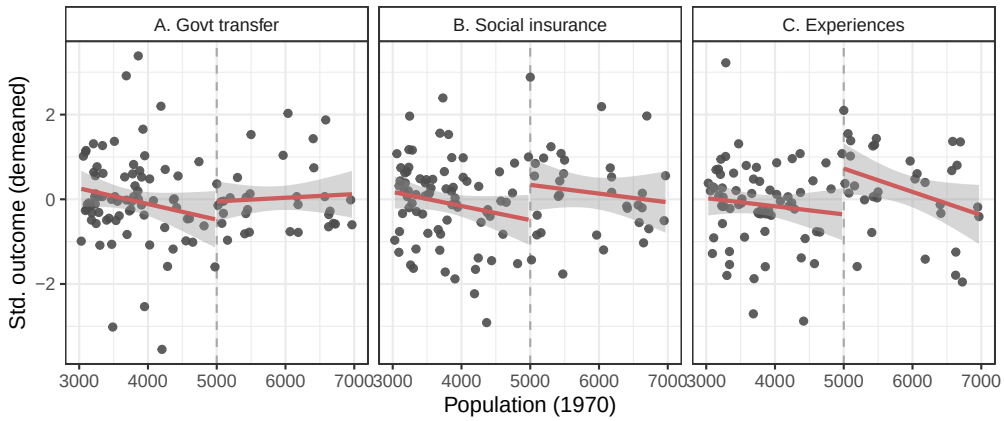
Note: Plots correspond to individual outcomes in the main tables presented in the results section. Point estimates with 90% and 95% confidence intervals plotted for varied bandwidths using equation (3.1). Forcing variable is locality population in 1970 census.

Figure B.18: Varying bandwidth for different outcomes (Equation 3.2)



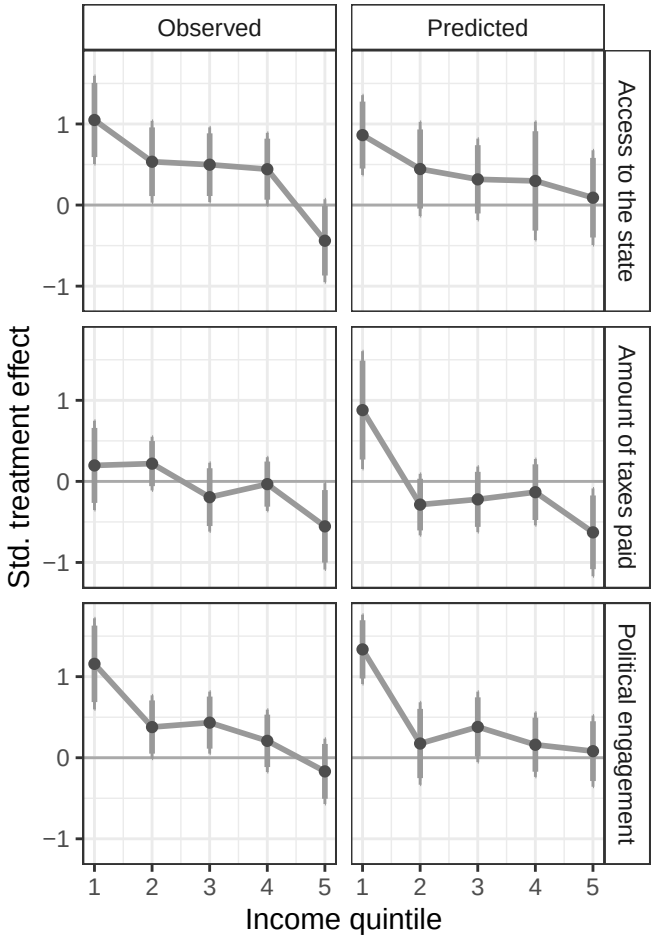
Note: Plots correspond to the marginal effects plots presented in the results section (Figures 3.6, 3.7, 3.8). Point estimates with 90% and 95% confidence intervals plotted for varied bandwidths using equation (3.2). Coefficient T corresponds to β_1^{DD} and coefficient $T \times X$ corresponds to β_3^{DD} using observed income. Forcing variable is locality population in 1970 census.

Figure B.19: Plot of treatment effect on access to the state (Table 3.2)



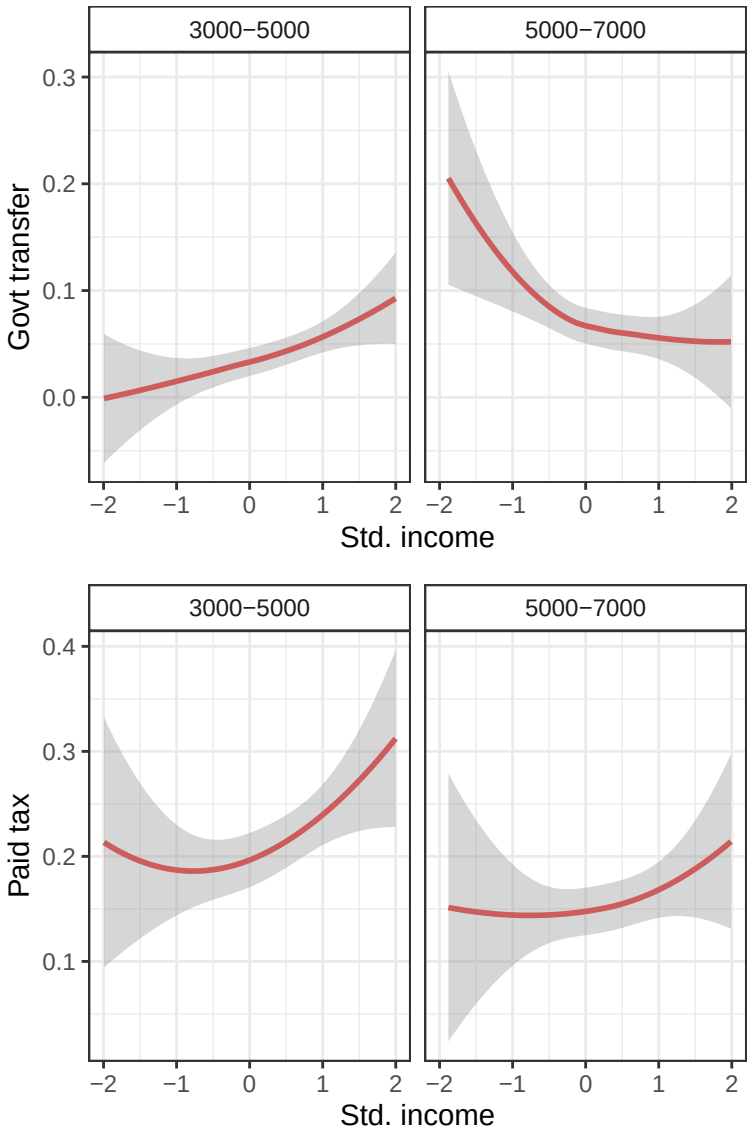
Note: To match the baseline estimating equation, observations are demeaned by region, survey year, and year of birth before being averaged to the 1970 locality-level. Each outcome variable is standardized for comparability.

Figure B.20: Marginal effects plots of treatment effects (quintile-based)



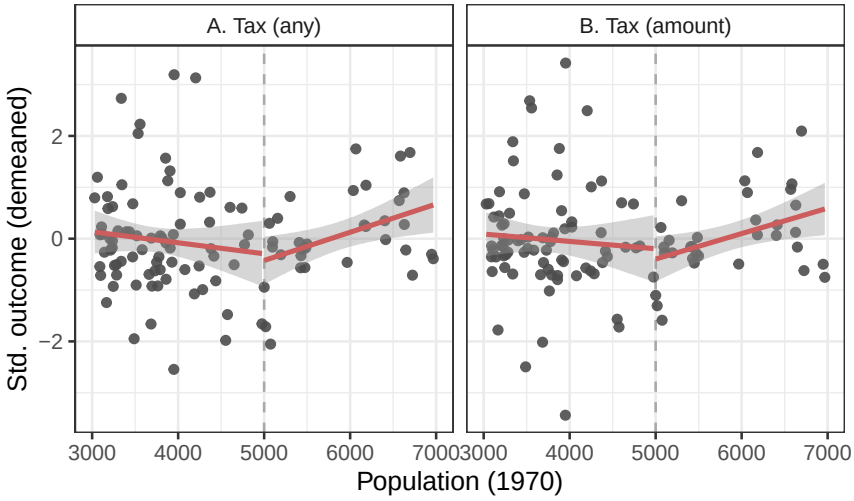
Note: Plots correspond to the marginal effects plots presented in the results section (Figures 3.6, 3.7, 3.8) but instead separately estimate Equation (3.1) within income *quintiles* on the x-axis, either *observed* or *predicted* (see Appendix B.1.4). 90% and 95% confidence intervals for each quintile plotted.

Figure B.21: Descriptive relationship between income and outcomes in treated and control localities



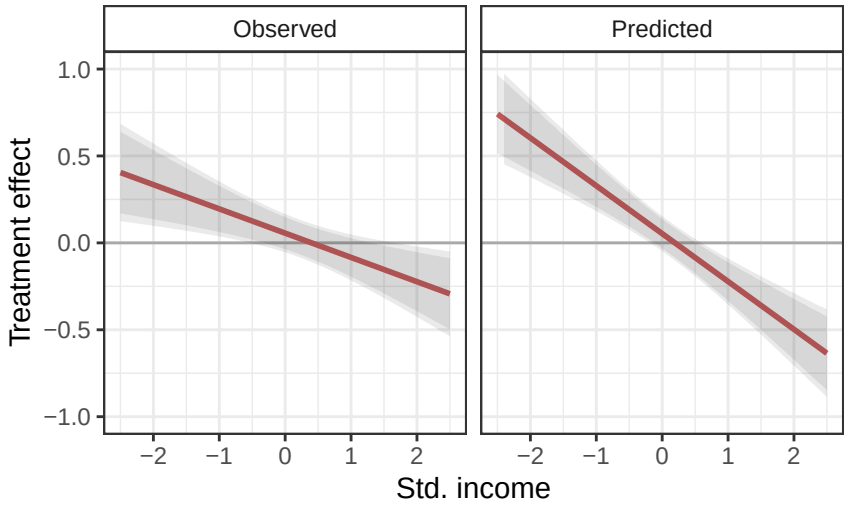
Note: Figure nonparametrically plots relationship between standardized income and outcome measures pooled across treated and control localities.

Figure B.22: Plot of treatment effect on tax payment (Table B.18)



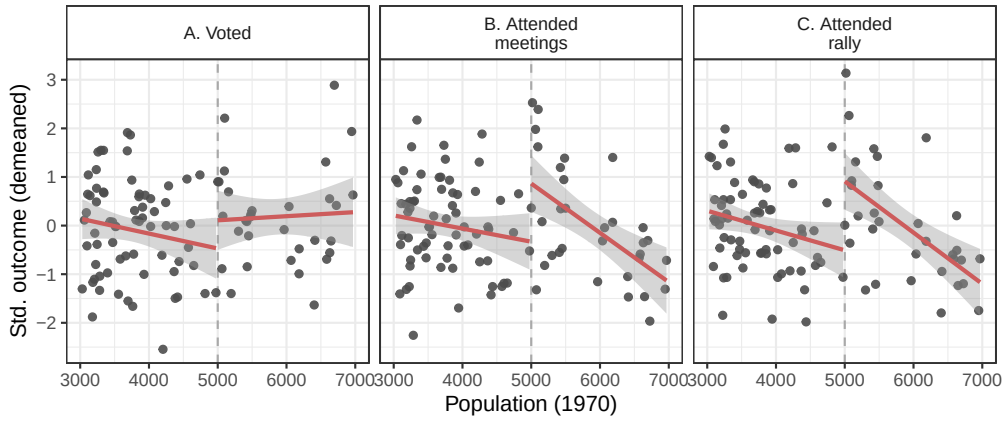
Note: To match the baseline estimating equation, observations are demeaned by region, survey year, and year of birth before being averaged to the 1970 locality-level. Each outcome variable is standardized for comparability.

Figure B.23: Marginal effects plot of tax payment (binary)



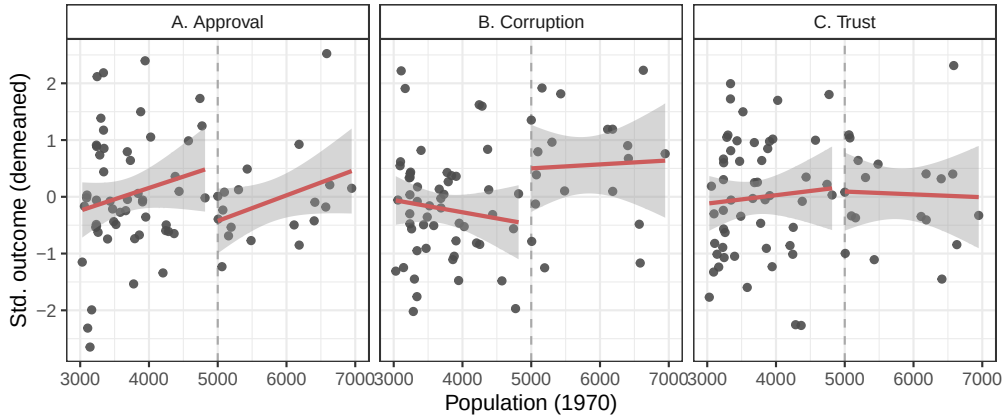
Note: 90% and 95% confidence bands plotted. Outcome is an indicator for whether household paid any taxes (central or local) in last year. Plots vary in whether they use an *observed* or *predicted* measure of income (see Appendix B.1.4).

Figure B.24: Plot of treatment effect on political engagement (Table 3.3)



Note: To match the baseline estimating equation, observations are demeaned by region, survey year, and year of birth before being averaged to the 1970 locality-level. Each outcome variable is standardized for comparability.

Figure B.25: Plot of treatment effect on political approval (Table 3.4)



Note: To match the baseline estimating equation, observations are demeaned by region, survey year, and year of birth before being averaged to the 1970 locality-level. Each outcome variable is standardized for comparability.

B.3 Additional tables

Table B.1: Comparison of 1970 census data with regional distribution of civil registries in 1975

Region	Number of localities with 5,000+ pop. (1)	Number of civil registries (2)	(2) - (1)	Number of localities with 20,000+ pop. (3)
Ashanti	17	19	2	3
Brong-Ahafo	19	21	2	1
Central	22	22	0	3
Eastern	29	33	4	4
Greater Accra	8	14	6	4
Northern	8	10	2	2
Upper	4	6	2	2
Volta	15	15	0	1
Western	14	15	1	3
Total	136	155	19	22

Column (1) is the number of localities in the 1970 census with above 5,000 population by region; Column (2) is the number of urban registries reported by the Births and Deaths Registry as existing in each region in 1975. Column (3) shows that the differences (2) - (1) are largest in regions with the most large localities, which had more than one registry.

Table B.2: Locality descriptive statistics (1970 census)

	Mean	SD	Min	Max
Demographic				
Average household size	10.94	3.22	5.21	20.55
Share male	0.48	0.02	0.38	0.54
Ethnic fractionalization	0.42	0.25	0.02	0.90
Population density	69.22	52.01	4.18	267.26
Population (1948)	2147.29	1013.81	286.00	6773.00
Population (1960)	3650.57	1179.78	508.00	6881.00
Birthplace				
Share born in this locality	0.63	0.17	0.26	0.93
Share born elsewhere in region	0.18	0.10	0.04	0.55
Share born in different region	0.15	0.10	0.02	0.56
Share born outside Ghana	0.04	0.04	0.00	0.22
Share Ghanaian	0.94	0.09	0.38	1.00
Age				
Share 0-5	0.21	0.02	0.16	0.27
Share 6-14	0.26	0.03	0.17	0.31
Share 15+	0.52	0.03	0.45	0.61
Socioeconomic				
Share ever educated	0.40	0.12	0.02	0.69
Share employed (non-agriculture)	0.18	0.07	0.03	0.35
Share employed (agriculture)	0.18	0.08	0.01	0.40

Unit is the locality in 1970. Sample restricted to all localities with a population above 3,000 in 1970 census in Local Councils.

Table B.3: GLSS sample summary statistics

	Mean	SD	Min	Max
Demographic				
Has proof of age	0.69	0.46	0.00	1.00
Age	39.60	17.47	18.00	99.00
Male	0.45	0.50	0.00	1.00
Local ethnic majority	0.75	0.43	0.00	1.00
Christian	0.75	0.43	0.00	1.00
Disabled	0.03	0.17	0.00	1.00
Household head	0.48	0.50	0.00	1.00
Spouse of household head	0.23	0.42	0.00	1.00
Birthplace				
Born here	0.65	0.48	0.00	1.00
Born elsewhere in region	0.14	0.35	0.00	1.00
Born in different region	0.20	0.40	0.00	1.00
Born outside Ghana	0.02	0.13	0.00	1.00
Socioeconomic				
Father educated	0.40	0.49	0.00	1.00
Mother educated	0.28	0.45	0.00	1.00
Income	0.04	0.96	-3.29	5.27
$\widehat{\text{Income}}$	-0.00	0.99	-3.80	3.18

Table provides summary statistics for baseline GLSS sample.

Table B.4: GLSS interactions with government agencies summary statistics

	Interacted (1)	Efficient (2)	Bribed (3)
Passport office	0.56	0.46	0.12
Revenue authority	0.68	0.53	0.08
Driver, Vehicle & License Authority	0.64	0.49	0.10
Post office	0.70	0.54	0.04
Prison service	0.71	0.62	0.06
Immigration service	0.67	0.53	0.09
Electoral Commission	0.89	0.71	0.05
Government health system	0.95	0.80	0.12
Government education system	0.95	0.81	0.10
Births & Deaths Registry	0.74	0.65	0.08
Local government	0.81	0.59	0.07
Police service	0.94	0.70	0.19
Judicial service	0.81	0.65	0.10
Fire service	0.84	0.73	0.06
State electricity company	0.91	0.57	0.14
State owned banks	0.73	0.65	0.05
Social Security and National Insurance Trust	0.54	0.47	0.05
Property and land registration	0.57	0.47	0.06
Registrar General	0.51	0.45	0.05
State water company	0.80	0.63	0.08
State-owned media	0.78	0.69	0.05

Table provides summary statistics for baseline GLSS sample. 'Knows efficiency' is an indicator for the respondent not replying 'Do not know' or 'Not applicable' when answering about the efficiency of interactions with different government agencies. 'Efficient' is an indicator for respondent answering that a given agency is either efficient or very efficient and corresponds to the outcome used in Table 3.2. 'Bribed' is an indicator for respondent answering that they have ever had to pay a bribe to get things done with a given agency.

Table B.5: Balance in locality characteristics (1970 census)

	β_1^{RD} (1)	SE (2)
Demographic		
Average household size	-0.20	(0.82)
Share male	-0.00	(0.01)
Ethnic fractionalization	-0.03	(0.08)
Population density	2.70	(17.49)
Population (1948)	119.71	(304.98)
Population (1960)	90.06	(305.08)
Birthplace		
Share born in this locality	0.03	(0.05)
Share born elsewhere in region	0.01	(0.03)
Share born in different region	-0.03	(0.04)
Share born outside Ghana	-0.01	(0.01)
Share Ghanaian	0.01	(0.02)
Age		
Share 0-5	-0.00	(0.01)
Share 6-14	0.01	(0.01)
Share 15+	-0.01	(0.01)
Socioeconomic		
Share ever educated	0.05	(0.03)
Share employed (non-agriculture)	-0.01	(0.02)
Share employed (agriculture)	-0.00	(0.02)
Index		
First principal component	-0.18	(0.29)
Second principal component	0.27	(0.26)

Table regresses locality-level characteristics from the 1970 census onto Equation (3.1) within a bandwidth of +/- 2000 population. Final two covariates use standardized principal components of the other variables.

Table B.6: Balance in locality characteristics (1984 census)

	β_1^{RD} (1)	SE (2)
Demographic		
Population	-84.17	(411.01)
Average household size	0.00	(0.01)
Share male	-0.01	(0.01)
Main water supply		
Pipe-borne	-0.03	(0.17)
Bore hole	0.23*	(0.12)
Well	-0.10	(0.11)
Dam	-0.03	(0.04)
River	-0.07	(0.12)
Health facility		
Health center	0.01	(0.06)
Hospital	0.06	(0.13)
Health post	-0.10	(0.14)
Mobile clinic	0.04	(0.07)
Maternity home	-0.19	(0.16)
Polyclinic	0.04	(0.12)
Rural health center	0.11	(0.10)
Traditional health center	0.01	(0.03)
Education facility		
Primary school	0.05	(0.06)
Middle school	-0.04	(0.04)
Index		
First principal component	-0.12	(0.32)
Second principal component	0.25	(0.33)

Table regresses locality-level characteristics from the 1984 census onto Equation (3.1) within a bandwidth of +/- 2000 population. Final two covariates use standardized principal components of the other variables. $\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.7: No change in presence of local public goods using GLSS data

	β_1^{RD} (1)	SE (2)
Overall		
Index	0.16	(0.52)
Services		
Index	0.33	(0.58)
Road	0.22	(0.13)
Electricity	-0.16	(0.27)
Water	0.02	(0.25)
Post office	0.16	(0.17)
Bank	0.18	(0.13)
School		
Index	0.01	(0.52)
Primary school	0.05	(0.21)
Middle school	-0.06	(0.27)
Secondary school	0.01	(0.15)
Health		
Index	0.49	(0.52)
Clinic	0.36	(0.22)
Dispensary	0.05	(0.24)
Hospital	-0.05	(0.07)
Pharmacy	0.04	(0.03)

DVs: 'Index' is a standardized index of all outcomes in a given group.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8: Placebo tests using different censuses

	A. Born in locality			B. Born outside locality		
Census	1960	1970	1984	1960	1970	1984
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{60} > 5000)$	0.06			0.05		
	(0.06)			(0.07)		
$\mathbb{1}(\text{Pop}_{70} > 5000)$		0.20***			-0.02	
		(0.06)			(0.07)	
$\mathbb{1}(\text{Pop}_{84} > 5000)$			-0.06			-0.03
			(0.05)			(0.06)
Control Mean	0.45	0.49	0.55	0.49	0.53	0.59
Observations	7401	7401	7401	4165	4165	4165

Outcome is an indicator for respondent possessing proof of age. Each column varies the year of the census data used for the population threshold to determine the treatment variable. Estimated using Equation 3.1.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.9: Balance in GLSS respondent characteristics

	A. Full sample				B. Born in locality			
	All		HH Head		All		HH Head	
	β_1^{RD}	SE	β_1^{RD}	SE	β_1^{RD}	SE	β_1^{RD}	SE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Demographic								
Age	0.42	(1.38)	0.60	(1.66)	0.04	(1.77)	-1.33	(1.99)
Male	-0.00	(0.02)	0.02	(0.04)	0.01	(0.03)	-0.03	(0.05)
Local ethnic majority	0.17	(0.10)	0.12	(0.09)	0.17	(0.11)	0.14	(0.10)
Christian	0.07	(0.08)	0.11*	(0.07)	0.03	(0.10)	0.08	(0.08)
Disabled	0.03**	(0.01)	0.02	(0.02)	0.04***	(0.01)	0.03	(0.02)
Household head	-0.01	(0.02)	0.00	(.)	0.04	(0.03)	0.00	(.)
Spouse of household head	0.02	(0.02)	0.00	(.)	-0.02	(0.03)	0.00	(.)
Birthplace								
Born in locality	-0.05	(0.08)	0.00	(0.08)	0.00	(.)	0.00	(.)
Born elsewhere in region	0.06	(0.06)	-0.01	(0.05)	0.00	(.)	0.00	(.)
Born in different region	-0.01	(0.05)	0.00	(0.05)	0.00	(.)	0.00	(.)
Born outside Ghana	0.00	(0.01)	0.00	(0.01)	0.00	(.)	0.00	(.)
Migration								
Lived elsewhere	0.00	(0.06)	0.01	(0.06)	0.10	(0.08)	0.13	(0.11)
Migration reason: family/marriage	0.08*	(0.05)	0.00	(0.04)	0.00	(.)	0.00	(.)
Migration reason: work/study	-0.02	(0.03)	0.02	(0.04)	0.00	(.)	0.00	(.)
Socioeconomic								
Father educated	0.01	(0.05)	-0.01	(0.05)	0.01	(0.05)	-0.01	(0.05)
Mother educated	0.03	(0.04)	-0.01	(0.04)	0.04	(0.05)	0.00	(0.04)
Income	-0.03	(0.13)	-0.05	(0.12)	0.04	(0.15)	-0.02	(0.14)
$\widehat{\text{Income}}$	-0.04	(0.18)	-0.13	(0.15)	0.05	(0.20)	-0.03	(0.16)

Table regresses respondent-level characteristics from baseline GLSS sample onto Equation (3.1) within a bandwidth of +/- 2000 population. Panel A includes full sample. Panel B subsets to individuals born in the locality of enumeration. Estimate for age excludes year of birth FE.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.10: Balance in AB respondent characteristics

	β_1^{RD} (1)	SE (2)
Demographic		
Age	0.97	(2.66)
Male	0.01	(0.03)
Socioeconomic		
Primary education	-0.03	(0.09)
Secondary education	0.05	(0.06)
Employed	-0.03	(0.10)
Income	0.16	(0.15)
EA-level		
EA Services	0.26	(0.32)
EA Facilities	0.46	(0.31)
EA Sec	-0.06	(0.36)
EA Urban	0.03	(0.18)
Latitude	0.49	(0.30)
Longitude	-0.02	(0.31)

Table regresses respondent-level characteristics from baseline AB sample onto Equation (3.1) within a bandwidth of +/- 2000 population. Estimate for age excludes year of birth FE.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.11: Effects on possessing proof of identity by different samples

	(1)	(2)	(3)	(4)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.21***	0.18***	0.17***	0.14***
	(0.06)	(0.06)	(0.05)	(0.05)
Control Mean	0.47	0.61	0.49	0.63
Born in locality	Yes	Yes	All	All
Sample	All	Head	All	Head
EAs	357	353	360	360
Clusters	114	113	114	114
Observations	7279	3349	11365	5388

Outcome is an indicator for respondent possessing proof of age. 'Born in locality' indicates whether sample is restricted to only respondents born in the locality of enumeration. 'Sample' indicates whether the sample comprises all household members or just household heads. Estimated using Equation 3.1.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.12: Effects on possessing proof of identity by birth cohort

		Decade of birth				
	All	'50s	'60s	'70s	'80s	'90s
	(1)	(2)	(3)	(4)	(5)	(6)
A. Born in locality						
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.20***	0.25**	0.14*	0.26***	0.22***	0.25**
	(0.06)	(0.10)	(0.07)	(0.08)	(0.08)	(0.11)
Control Mean	0.48	0.43	0.42	0.48	0.58	0.65
Clusters	117	112	113	115	113	101
Observations	7401	853	1204	1357	1451	1322
B. Born outside locality						
$\mathbb{1}(\text{Pop}_{70} > 5000)$	-0.02	0.06	0.03	0.16	0.08	0.13
	(0.07)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Control Mean	0.52	0.46	0.50	0.52	0.66	0.83
Clusters	117	106	107	108	106	92
Observations	4165	515	722	801	879	507

Outcome is an indicator for respondent possessing proof of age. Estimated using Equation 3.1.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.13: Effects on possessing proof of age by excluded GLSS survey round

		GLSS round excluded						
	All	-R1 (1987)	-R2 (1989)	-R4 (1998)	-R5 (2005)	-R6 (2012)	-R7 (2017)	R6/R7 (2012/17)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Born in locality								
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.20***	0.28***	0.25***	0.18***	0.27***	0.20***	0.22***	0.23***
	(0.06)	(0.07)	(0.07)	(0.06)	(0.08)	(0.08)	(0.07)	(0.07)
Control Mean	0.48	0.52	0.52	0.57	0.53	0.41	0.44	0.68
Clusters	117	115	117	114	112	108	106	103
Observations	7401	7009	7204	6485	6176	4864	5267	4671
B. Born outside locality								
$\mathbb{1}(\text{Pop}_{70} > 5000)$	-0.02	0.06	-0.02	-0.10	-0.04	-0.01	0.02	-0.06
	(0.07)	(0.09)	(0.09)	(0.10)	(0.10)	(0.12)	(0.10)	(0.10)
Control Mean	0.52	0.58	0.57	0.61	0.59	0.47	0.49	0.74
Clusters	117	115	117	114	113	108	106	104
Observations	4165	3882	3998	3794	3280	2805	3066	2459

Outcome is an indicator for respondent possessing proof of age. Estimated using Equation 3.1. Columns 2-7 sequentially drop one GLSS survey round from the estimation. Column 8 estimates effects just using round 6 and 7.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.14: Disaggregated effects on access to government transfers

	Any		Social Security		Pension		LEAP	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.08**	0.07**	-0.01	-0.01	-0.00	0.01	0.02*	0.01
	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Sample	All	Head	All	Head	All	Head	All	Head
Control mean	0.04	0.04	0.02	0.02	0.01	0.01	0.00	0.01
Survey rounds	[4-7]	[4-7]	[4-7]	[4-7]	[4-7]	[4-7]	[6-7]	[6-7]
EAs	328	328	253	253	253	253	159	159
Clusters	111	111	102	102	102	102	86	86
Observations	9185	4364	6228	2943	6228	2943	4035	1882

DVs are indicators for household receipt of social welfare transfers in least year. 'Any': indicator for any transfer; 'SSNIT': Social Security and National Insurance Trust; 'Pension': state pension; 'LEAP': Livelihood Empowerment Against Poverty cash transfer program.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.15: Effect on interactions with state agencies

	A. Interaction		B. Bribe payment	
	(1)	(2)	(3)	(4)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.38	0.39*	-0.29	-0.36*
	(0.24)	(0.23)	(0.18)	(0.19)
Sample	All	Head	All	Head
Control mean	-0.07	-0.06	0.07	0.08
Survey rounds	[6-7]	[6-7]	[6-7]	[6-7]
EAs	234	234	234	234
Clusters	102	102	102	102
Observations	3167	2119	3167	2119

DVs are all standardized z-score indices: Panel A: extent of interaction with different central government agencies; Panel B: how often respondent had to pay bribes in these interactions.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is observed per capita log total expenditure. '$\widehat{\text{Income}}$' is predicted per capita log total consumption (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.16: Effects on access to local public goods

	A. Education			B. Healthcare		
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.02	0.03	0.03	-0.00	0.00	-0.01
	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.00			-0.01	
		(0.03)			(0.02)	
$\mathbb{1}(\text{Pop}_{70} > 5000) \times \widehat{\text{Income}}$			0.06			-0.02
			(0.05)			(0.02)
Sample	All	All	All	All	All	All
Control mean	0.72	0.72	0.74	0.16	0.16	0.15
Survey rounds	[1-7]	[1-7]	[4-7]	[1-7]	[1-7]	[4-7]
EAs	360	360	331	360	360	331
Clusters	114	114	112	114	114	112
Observations	11115	11113	10090	11093	11091	10067

DVs: Panel A: Respondent has formal education; B: Respondent has been to a health facility in the past year.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is observed per capita log total expenditure. '$\widehat{\text{Income}}$' is predicted per capita log total consumption (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.17: Heterogeneous effects on access to the state

	(1)	(2)	(3)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.43**	0.50***	0.48***
	(0.19)	(0.19)	(0.18)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.28**	
		(0.13)	
$\mathbb{1}(\text{Pop}_{70} > 5000) \times \widehat{\text{Income}}$			-0.21*
			(0.11)
Sample	All	All	All
Survey rounds	[6-7]	[6-7]	[6-7]
EAs	234	234	234
Clusters	102	102	102
Observations	3166	3166	3162

DV: Index of outcomes in Table 3.2.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is observed per capita log total expenditure. '$\widehat{\text{Income}}$' is predicted per capita log total consumption (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.18: Effects on tax payment

	A. Any tax			B. Amount of tax		
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.01	0.02	0.01	-0.12	-0.11	-0.11
	(0.06)	(0.06)	(0.05)	(0.14)	(0.14)	(0.13)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.11**			-0.24**	
		(0.05)			(0.12)	
$\mathbb{1}(\text{Pop}_{70} > 5000) \times \widehat{\text{Income}}$			-0.20***			-0.29***
			(0.04)			(0.10)
Sample	All	All	All	All	All	All
Control mean	0.23	0.23	0.23	0.03	0.03	0.03
Survey rounds	[4-7]	[4-7]	[4-7]	[4-7]	[4-7]	[4-7]
EAs	331	331	331	331	331	331
Clusters	112	112	112	112	112	112
Observations	10326	10326	10301	10274	10274	10249

DVs: Panel A: Indicator for whether respondent paid any taxes (either central or local) in last year; Panel B: Standardized (log+1) amount of tax paid in last year.
$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is observed per capita log total expenditure. '$\widehat{\text{Income}}$' is predicted per capita log total consumption (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.19: Effects on taxation over time

	A. Any tax			B. Amount of tax		
1998-2005	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	-0.23	-0.23	-0.28	-0.24	-0.23	-0.29
	(0.30)	(0.29)	(0.26)	(0.39)	(0.36)	(0.35)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		0.12			0.12	
		(0.24)			(0.33)	
$\mathbb{1}(\text{Pop}_{70} > 5000) \times \widehat{\text{Income}}$			0.03			0.00
			(0.18)			(0.26)
Sample	All	All	All	All	All	All
Survey waves	[4-5]	[4-5]	[4-5]	[4-5]	[4-5]	[4-5]
EAs	97	97	97	97	97	97
Clusters	72	72	72	72	72	72
Control mean	0.09	0.09	0.09	0.27	0.27	0.27
Observations	3314	3314	3301	3314	3314	3301
2012-2017	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	-0.05	-0.06	-0.06	-0.03	-0.04	-0.03
	(0.14)	(0.13)	(0.13)	(0.07)	(0.06)	(0.06)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.07			-0.01	
		(0.13)			(0.06)	
$\mathbb{1}(\text{Pop}_{70} > 5000) \times \widehat{\text{Income}}$			-0.24***			-0.10*
			(0.09)			(0.05)
Sample	All	All	All	All	All	All
Survey waves	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]
EAs	234	234	234	234	234	234
Clusters	102	102	102	102	102	102
Control mean	-0.07	-0.07	-0.07	-0.18	-0.18	-0.18
Observations	6960	6960	6948	6960	6960	6948

DV: Respondent's household has paid any taxes in the past year.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is observed per capita log total expenditure. '$\widehat{\text{Income}}$' is predicted per capita log total consumption (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.20: Effects on beliefs about tax compliance

	A. Wrong		B. Understandable		C. Punishable	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.14**	0.11**	0.14*	0.16*	-0.01	-0.04
	(0.05)	(0.05)	(0.09)	(0.08)	(0.09)	(0.07)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.09*		0.16***		-0.24***
		(0.05)		(0.05)		(0.07)
Control Mean	0.88	0.88	0.17	0.17	0.71	0.71
Survey rounds	[5-6]	[5-6]	[5-6]	[5-6]	[5-6]	[5-6]
EAs	88	88	88	88	88	88
Clusters	53	53	53	53	53	53
Observations	704	704	704	704	704	704

DVs are all indicators. Panel A: Respondent thinks it is wrong to avoid paying taxes. Panel B: Respondent thinks it is wrong but understandable to avoid paying taxes. Panel C: Respondent thinks it is wrong and punishable to avoid paying taxes. Outcomes from Afrobarometer R5/R6.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is a measure of observed income based on asset ownership (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.21: Effects on attitudes towards traditional authorities

	A. Trust		B. Corruption		C. Approve		D. Influence	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.05	0.03	0.12	0.13	0.41**	0.36**	0.66*	0.75*
	(0.18)	(0.17)	(0.14)	(0.13)	(0.18)	(0.17)	(0.36)	(0.38)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.25*		0.10		-0.33*		-0.11
		(0.13)		(0.14)		(0.19)		(0.29)
Control Mean	1.75	1.75	1.24	1.24	2.54	2.54	2.85	2.85
Survey rounds	[4-7]	[4-7]	[4-7]	[4-7]	[6-7]	[6-7]	[4-4]	[4-4]
EAs	102	102	102	102	78	78	24	24
Clusters	58	58	58	58	49	49	21	21
Observations	816	816	816	816	624	624	192	192

DVs: Panel A: How much respondent trusts traditional authorities; Panel B: How much respondent believes traditional authorities engage in corruption; Panel C: How much respondent approves of traditional authorities; Panel D: How influential respondent considers traditional authorities to be.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is a measure of observed income based on asset ownership (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.22: Effects on political engagement

	(1)	(2)	(3)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.40***	0.49***	0.45***
	(0.15)	(0.14)	(0.14)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.39***	
		(0.11)	
$\mathbb{1}(\text{Pop}_{70} > 5000) \times \widehat{\text{Income}}$			-0.30***
			(0.11)
Sample	All	All	All
Survey rounds	[6-7]	[6-7]	[6-7]
EAs	234	234	234
Clusters	102	102	102
Observations	3166	3166	3162

DVs: Panel A: Respondent voted in last election; B: Respondent has attended local meetings in the past three years; Panel C: Respondent has attended political rallies or protests in the past three years.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is observed per capita log total expenditure. '$\widehat{\text{Income}}$' is predicted per capita log total consumption (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.23: Effect on voting driven by difficulty of identity verification

	Not represented		Not worth it		Not registered		Not verified	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	-0.01**	-0.01**	-0.01	-0.00	-0.01	0.00	-0.02*	-0.02**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Sample	All	Head	All	Head	All	Head	All	Head
Control mean	0.01	0.01	0.02	0.02	0.02	0.01	0.02	0.02
Survey rounds	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]
EAs	234	234	234	234	234	234	234	234
Clusters	102	102	102	102	102	102	102	102
Observations	3227	2161	3227	2161	3227	2161	3227	2161

DVs are indicators for stated reasons why respondent did not vote. 'Not represented': respondent did not feel represented by any candidate; 'Not worth it': respondent did not consider voting worthwhile; 'Not registered': respondent was not registered; 'Not verified': respondent could not have their identity verified. Standard errors clustered at the level of the 1970 locality.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.1). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.24: Effects on political allegiances

	A. Vote without feeling pressured			B. Cross party lines		
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.11*	0.11*	0.10*	-0.16**	-0.17**	-0.18**
	(0.06)	(0.06)	(0.06)	(0.08)	(0.08)	(0.07)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.01			0.07	
		(0.03)			(0.05)	
$\mathbb{1}(\text{Pop}_{70} > 5000) \times \widehat{\text{Income}}$			0.03			0.18***
			(0.04)			(0.05)
Sample	All	All	All	All	All	All
Control mean	0.87	0.87	0.87	0.56	0.56	0.56
Survey rounds	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]	[6-7]
EAs	234	234	234	234	234	234
Clusters	102	102	102	102	102	102
Observations	3166	3166	3162	3165	3165	3161

DVs: Panel A: Respondent feels free to vote for candidates without feeling pressured; Panel B: Respondent would ever vote for a candidate not from their party.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is observed per capita log total expenditure. '$\widehat{\text{Income}}$' is predicted per capita log total consumption (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.25: Effects on conceptions of democracy

	(1)	(2)	(3)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.13	0.17	0.15
	(0.13)	(0.13)	(0.13)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.16*	
		(0.09)	
$\mathbb{1}(\text{Pop}_{70} > 5000) \times \widehat{\text{Income}}$			-0.10
			(0.09)
Sample	All	All	All
Control mean	-0.03	-0.03	-0.03
Survey rounds	[6-7]	[6-7]	[6-7]
EAs	234	234	234
Clusters	102	102	102
Observations	3166	3166	3162

DVs: Panel A: Index of how many components respondent considers to be essential for democracy.

$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is observed per capita log total expenditure. '$\widehat{\text{Income}}$' is predicted per capita log total consumption (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B.26: Disaggregated effects on political attitudes

	A. Overall		B. Services		C. Economy	
Performance	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	-0.32**	-0.36**	-0.24	-0.29*	-0.28*	-0.32**
	(0.16)	(0.15)	(0.16)	(0.15)	(0.15)	(0.15)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		0.01		0.01		0.02
		(0.13)		(0.14)		(0.13)
Control Mean	0.05	0.05	0.04	0.04	0.04	0.04
Survey rounds	[3-7]	[3-7]	[3-7]	[3-7]	[3-7]	[3-7]
EAs	174	174	174	174	174	174
Clusters	71	71	71	71	71	71
Observations	1392	1392	1392	1392	1392	1392

	A. Overall		B. Elected		C. Non-elected	
Corruption	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	0.36***	0.38***	0.26*	0.28*	0.37***	0.38***
	(0.13)	(0.13)	(0.14)	(0.14)	(0.13)	(0.14)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.08		-0.14		-0.02
		(0.15)		(0.16)		(0.15)
Control Mean	-0.10	-0.10	-0.08	-0.08	-0.11	-0.11
Survey rounds	[3-7]	[3-7]	[3-7]	[3-7]	[3-7]	[3-7]
EAs	174	174	174	174	174	174
Clusters	71	71	71	71	71	71
Observations	1392	1392	1392	1392	1392	1392

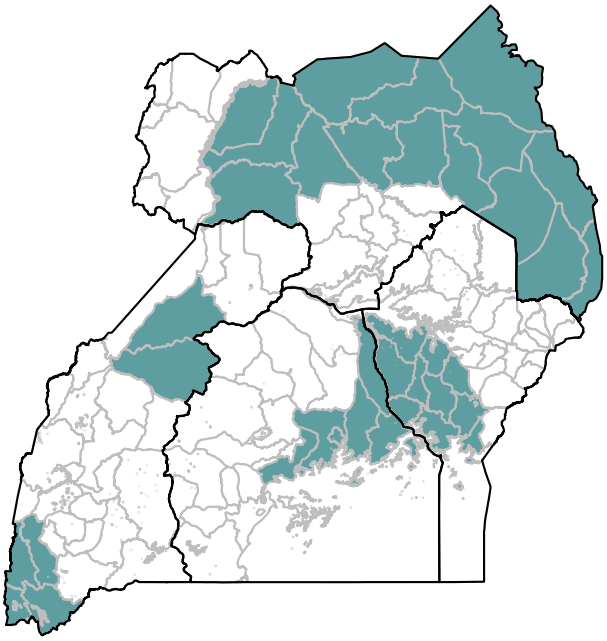
	A. Overall		B. Elected		C. Non-elected	
Trust	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}(\text{Pop}_{70} > 5000)$	-0.03	-0.02	-0.04	-0.04	0.03	0.05
	(0.16)	(0.17)	(0.16)	(0.16)	(0.15)	(0.16)
$\mathbb{1}(\text{Pop}_{70} > 5000) \times$ Income		-0.10		-0.06		-0.10
		(0.13)		(0.15)		(0.10)
Control Mean	0.02	0.02	0.02	0.02	-0.01	-0.01
Survey rounds	[3-7]	[3-7]	[3-7]	[3-7]	[3-7]	[3-7]
EAs	174	174	174	174	174	174
Clusters	71	71	71	71	71	71
Observations	1392	1392	1392	1392	1392	1392

DVs are standardized disaggregated versions of the outcomes in Table 3.4. Panel A disaggregates approval measure into approval towards services and economic issues. Panel B disaggregates perceived corruption measure into perceived corruption among elected versus non-elected institutions. Panel C disaggregates trust measure into trust towards elected versus non-elected institutions.
$\mathbb{1}(\text{Pop}_{70} > 5000)$ is an indicator for population of l being above 5,000 in 1970 census. 'Income' is a measure of observed income based on asset ownership (see Appendix B.1.4). All specifications are estimated using OLS within a bandwidth of +/- 2000 population using Equation (3.2). Standard errors clustered at the level of the locality in 1970. * p < 0.1, ** p < 0.05, *** p < 0.01.

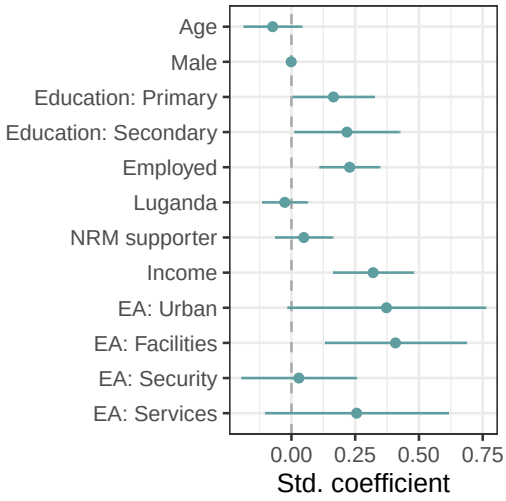
C | Appendix to Chapter 4

C.1 Additional figures

Figure C.1: Early vs late-rollout districts



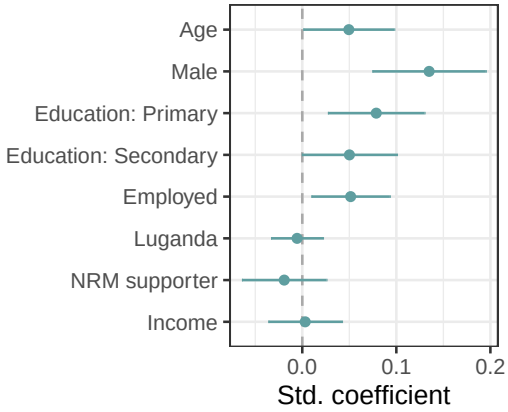
(a) Spatial distribution of treated districts



(b) Cross-sectional differences between respondents in treated and control districts

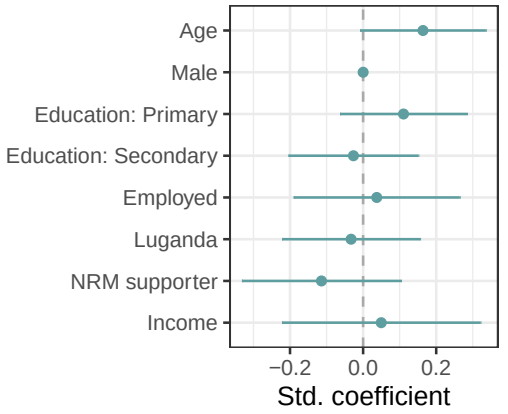
Notes: Districts and regions are as defined pre-rollout in 2014. *Treated* districts are those shaded districts where the card issuance process had commenced prior to survey enumeration. Figure C.1b uses pooled Afrobarometer data to compare respondents' individual/EA-level covariates in treated districts relative to those in control. Specification includes region fixed effects and clusters standard errors at the district-level.

Figure C.2: Imbalances in who interacts with the state



Note: Figure uses pooled Afrobarometer data to regress individual-level covariates onto a standardized index of respondents' extent of interaction with the state in the last year. Specification includes enumeration area fixed effects and clusters standard errors at the district-level.

Figure C.3: Placebo difference-in-differences outcomes



Note: Figure estimates treatment effects of β in Equation (1) using individual-level covariates on the left hand side. Specification includes district fixed effects, survey round fixed effects, and clusters standard errors at the district-level.

C.2 Additional tables

Table C.1: Placebo effects on prior political engagement

	A. Voted		B. Rallies		C. Campaigned	
	(1)	(2)	(3)	(4)	(5)	(6)
Rollout (β)	-0.036	-0.021	0.012	0.026	-0.034	-0.022
	[0.030]	[0.028]	[0.027]	[0.026]	[0.028]	[0.027]
Outcome mean	0.71	0.71	0.53	0.53	0.23	0.23
Outcome range	[0-1]	[0-1]	[0-1]	[0-1]	[0-1]	[0-1]
Region-round FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Observations	8447	8447	5992	5992	5992	5992
Clusters	105	105	105	105	105	105

DVs: (A) Respondent voted in last election; (B) Respondent attended campaign events in the last election; (C) Respondent worked on a campaign in the last election.
All specifications are estimated using OLS using Equation (4.1). *Rollout* is defined as respondent being in a district where, in 2015, biometric card issuance had commenced by the time of survey enumeration. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the district-level in parentheses.

Table C.2: Effects on economic perceptions and expectations

	A. Index		B. Current		C. -12m		D. +12m	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rollout	0.14**	0.15**	0.01	-0.00	0.11*	0.15**	0.19***	0.19***
	[0.07]	[0.07]	[0.07]	[0.07]	[0.06]	[0.06]	[0.06]	[0.06]
Region-round FE	✓	✓	✓	✓	✓	✓	✓	✓
Region-group FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓
Observations	8447	8447	8447	8447	8447	8447	8447	8447
Clusters	105	105	105	105	105	105	105	105

DVs: (A) Standardized index; (B) Current economic situation of the country; (C) Economic situation compared to 12 months ago; (D) Expected economic situation in 12 months' time.
All specifications are estimated using OLS using Equation (4.1). *Rollout* is defined as respondent being in a district where, in 2015, biometric card issuance had commenced by the time of survey enumeration. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the district-level in parentheses.

Table C.3: Effects on perceptions of freedom

	A. Index		B. Speech		C. Join		D. Vote		E. Careful	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Rollout	0.00	0.01	0.04	0.04	0.02	0.03	0.03	0.03	-0.08	-0.07
	[0.07]	[0.07]	[0.07]	[0.07]	[0.03]	[0.04]	[0.03]	[0.03]	[0.06]	[0.07]
Region-round FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region-group FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓		✓		✓
Observations	8447	8447	8447	8447	8447	8447	8447	8447	8447	8447
Clusters	105	105	105	105	105	105	105	105	105	105

DVs: (A) Standardized index; (B) How free to say what you think; (C) How free to join political organizations; (D) How free to vote; (E) How careful is it necessary to be when talking about current affairs.

All specifications are estimated using OLS using Equation (4.1). *Rollout* is defined as respondent being in a district where, in 2015, biometric card issuance had commenced by the time of survey enumeration. * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors clustered at the district-level in parentheses.

Table C.4: Supplementary effects on monitoring

	A. Avoid taxes		B. Avoid punishment	
	(1)	(2)	(3)	(4)
Rollout (β)	-0.04	-0.05	0.02	0.01
	[0.11]	[0.10]	[0.07]	[0.08]
Region-round FE	✓	✓	✓	✓
Controls		✓		✓
Observations	4816	4816	8447	8447
Clusters	103	103	105	105

DVs are standardized. (A) How difficult to avoid paying taxes; (B) How infrequently citizens avoid being punished after breaking the law.

All specifications are estimated using OLS using Equation (4.1). *Rollout* is defined as respondent being in a district where, in 2015, biometric card issuance had commenced by the time of survey enumeration. * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors clustered at the district-level in parentheses.

Table C.5: Heterogeneous effects

	A. Authority				B. Economic signal				C. Monitoring signal			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Rollout	0.27***	0.28***	0.33***	0.26***	0.14**	0.08	0.18*	0.15**	0.00	0.08	0.15	0.00
	[0.07]	[0.08]	[0.11]	[0.07]	[0.07]	[0.07]	[0.10]	[0.07]	[0.07]	[0.07]	[0.10]	[0.06]
Rollout × X_1		-0.03				0.08				-0.17***		
		[0.06]				[0.06]				[0.04]		
Rollout × X_2			-0.05				-0.02				-0.10**	
			[0.06]				[0.06]				[0.05]	
Rollout × X_3				0.03				-0.03				-0.05
				[0.06]				[0.06]				[0.04]
Region-round FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region-group FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5992	5992	5992	5992	8447	8447	8447	8447	8447	8447	8447	8447
Clusters	105	105	105	105	105	105	105	105	105	105	105	105

Interaction variables are all indicators: X_1: NRM supporter; X_2: above median income; X_3: has secondary education. DVs: (A) Standardized index of outcomes from Table 4.2; (B) Standardized index of outcomes from Table C.2; (C) Standardized index of outcomes from Table C.3.

All specifications are estimated using OLS using Equation (4.1). *Rollout* is defined as respondent being in a district where, in 2015, biometric card issuance had commenced by the time of survey enumeration. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the district-level in parentheses.

Table C.6: Effects on national versus ethnic identification

	A. National vs ethnic		B. More national		C. Only national	
	(1)	(2)	(3)	(4)	(5)	(6)
Rollout	0.10	0.06	0.01	0.02	0.04	0.03
	[0.07]	[0.07]	[0.03]	[0.03]	[0.03]	[0.03]
Outcome mean	3.29	3.29	0.32	0.32	0.20	0.20
Outcome range	[1-5]	[1-5]	[0-1]	[0-1]	[0-1]	[0-1]
Region-round FE	✓	✓	✓	✓	✓	✓
Region-group FE	✓	✓	✓	✓	✓	✓
Controls		✓		✓		✓
Observations	8447	8447	8447	8447	8447	8447
Clusters	105	105	105	105	105	105

DVs: (A) Continuous scale of national vs ethnic identity; (B) Indicator for participant affiliating themselves with the nation more than their ethnic group; (C) Indicator for participant only affiliating themselves with the nation.

All specifications are estimated using OLS using Equation (4.1). *Rollout* is defined as respondent being in a district where, in 2015, biometric card issuance had commenced by the time of survey enumeration. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the district-level in parentheses.

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