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Identifying the Rich: Registration, Taxation, and Access to the State in Tanzania

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How do states build their informational capacity? This article argues that distributive politics conditions how the state's capacity develops. I study civil registration, where citizens comply with the state's informational demands in exchange for documentary proof of identity, which may simultaneously facilitate access to public resources and exposure to taxation. Though the rich are particularly threatened by taxation, the narrow benefits of registration induce their compliance over that of the poor. I leverage a set of reforms in early postindependence Tanzania which provide quasi-random variation in citizens' registration status and show that registration promotes access to narrow-based resources, rather than broad-based ones, while increasing tax payment. In turn, citizens' decisions to comply reflect the economically stratified local incidence of these net benefits. The results suggest how nominally universal state-building schemes can have regressive effects on the state's coverage.

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" (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 taxation and the delivery of 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), 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 article, I consider how these parameters are set, articulate their implications for the state's coverage, and take advantage of a rich empirical setting in postindependence Tanzania to provide quasi-experimental evidence supporting the argument.

I focus on the case of identity registration schemes, perhaps the most ubiquitous mechanism through which states solicit information from their citizens. In complying with state demands for information, citizens trade off potential increases in both their access to public resources and their exposure to taxation. When bureaucratic capacity is moderate and democratic political competition is weak, state-building schemes to register citizens have especially economically stratified results. Moderate bureaucratic capacity renders the state selectively capable of regulating access to narrow-based public resources on the basis of registration status. Limited competition, by weakening redistributive pressures, dampens the concerns of higher economic status citizens about registering with the state. Together, the benefits of compliance become nonuniform even absent variation in citizens' logistical and financial costs. Rendering this incidence self-enforcing, to the extent that the state fiscally benefits by taxing those citizens with more economic resources, it faces only weak incentives to induce the registration of relatively poorer citizens.

Such seemingly administrative state-building schemes then have particularly regressive implications: rather than universalizing access, registration technologies can undergird and entrench economic inequalities in state–citizen interaction. The first empirical prediction of the theoretical framework is that supplying information to the state through registration should facilitate access to narrow-based public resources of particular value to higher economic status citizens, in exchange for increased 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

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for major confounders, such as rurality, income, and education, which also independently affect access to the state. Consequently, no credibly causally identified evidence appears to exist on its effects.

I study civil registration in Tanzania in the state-building period following independence in 1961, which offers an appropriate but challenging case.[1] 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 relatively wealthier citizens electing to supply 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 trade-offs they faced in registering with the state (Harris 1965; Hunter 2015; Kjekshus 1974), and how these were especially salient for wealthier citizens due to the progressive incidence of taxation (Huang 1976).

To evaluate the theory's empirical implications, I leverage a series of legal reforms—common across the region during this period—that sought to induce registration in a set of districts in the mid-1960s. 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. Then, leveraging reform exposure as an instrument for individuals' registration status—and therefore possession of identity documents—I provide evidence on its consequences for access to public resources and exposure to taxation.

The results point to effects on access to resources of particular relevance for relatively wealthier citizens, such as higher education and state employment, with no effect on access to broad-based public goods, such as primary education. In turn, registration increases citizens' direct tax payment. For the compliers induced to be registered by the reforms, registration creates substantial variation in access to a relatively narrow set of resources. Next, consistent with the stratified incidence of costs and benefits conditioning citizens' decisions, I establish three results. First, fewer citizens complied with the reform in localities where overall levels of tax collection were higher *or* where the local incidence of taxation was particularly progressive.[2] Second, the local presence of narrow-based public goods (secondary schools), but not broad-based public goods (primary schools), positively predicts compliance. Third, indicative of citizens' evaluation of these returns, individuals in targeted localities 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 article 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 2017; Soifer 2013), and its aggregate variation affects a range of societal outcomes (Brambor et al. 2020; D'Arcy and Nistotskaya 2017; Lee and Zhang 2017). If this capacity has such wide-ranging benefits, its persistently low levels across many developing country settings render it important—but mostly overlooked—to understand how it is built.[3] Because I argue the development of this capacity has distributive implications, its even expansion constitutes a political challenge as much as an administrative one. This political challenge implies that our understanding of the benefits of aggregate increases in informational capacity likely masks important heterogeneity in *who* benefits from a stronger state. The results in this article suggest a central role for economic inequality in conditioning how states invest in this capacity and to whom the benefits accrue.

Second, it relates to work on the distributive consequences of policy implementation. 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 truncated, however, compliance with state demands often benefits the rich at the expense of the poor (Bastagli 2009; Ferguson 1999; Holland 2017). The results here show how variation in citizens' compliance decisions can lead even nominally universal policy instruments to have regressive implications.[4] This is substantively important because a billion people, half of them in sub-Saharan Africa, lack proof of legal identity in spite of sustained donor-led efforts (Gelb and Metz 2018). 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.

[1] 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).

[2] This result aligns with prior work finding that states struggle to invest in fiscal capacity—because of the redistributive threat of taxation—when economic inequality is higher (Hollenbach and Silva 2019)

[3] Some recent work considers the endogenous determinants of investments in capacity more broadly. For example, Sánchez-Talanquer (2020) considers elites' decisions to register land, Christensen and Garfias (2021) consider electoral incentives to invest in capacity-building interventions, and Garfias and Sellars (2021) consider how centralizing investments are undermined by the weakening of local elites.

[4] Civil registration reforms are typically considered progressive interventions: goal 16.9 of the United Nations Sustainable Development Goals, for example, aims to "By 2030, provide legal identity for all, including birth registration."

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REGISTRATION AND THE STATE

The informational capacity of the state is typically tied to its 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 an array of public administration and governance challenges (Banerjee 1997; Ting 2017). Given the high coercive costs associated with census enumeration (Soifer 2013), perhaps the most common method for states to collect information about their citizens is through identification and registration schemes (Breckenridge and Szreter 2012; Caplan and Torpey 2001).[5] I conceptualize citizens' enrollment in such schemes as an *informational transaction*: citizens provide information about themselves, or their child, to the state in exchange for documentary evidence of their identity. Registration, therefore, reflects an instrumental decision rather than coercive imposition (Cohn and Dirks 1988).

Citizens' Decisions to Supply Information to the State

Citizens weigh the costs and benefits of registering with the state before deciding whether to comply with the state's demands for information. These costs are both direct and indirect. Direct costs represent the short-run financial and time costs of registration, which are often significant in predominantly rural countries (Hunter and Brill 2016; Makannah 1981). Further, even when registration carries low de jure financial costs, high logistical costs necessitated by bureaucratic interaction and brokerage impose particular burdens on poorer citizens (Gupta 2012).

The indirect costs of registration comprise the often-uncertain downstream consequences of becoming *legible* to the state (Scott 1998). Following Scott and a rich historical literature (Breckenridge and Szreter 2012), 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. For example, the administration of centralized direct taxes often imposes high informational demands on the state which are 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 preclude its future use for extraction (Longman 2018).[6]

The benefits of registration are defined by how compliance affects access to resources, whether in the short term or the long term. International organizations point to a wide array of sectors and services whereby access potentially depends on civil 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.[7]

Distributive Incidence of the Returns to Registration

Whether, and from whom, states are able to solicit information is conditioned by the extent of bureaucratic capacity and political competition together shaping citizens' expectations about the consequences of registering with the state. In a wide set of cases common to developing country contexts, where both bureaucratic capacity and democratic political competition are constrained, the incidence of these returns is especially economically stratified.[8]

Bureaucratic Capacity

Akin to gatekeeping (Cooper 2002), citizens' expected instrumental benefits of registration are shaped by the targeting and enforcement of exclusionary eligibility requirements in accessing particular goods, sectors, and services. The extent of the state's bureaucratic capacity constrains its ability to control access to public resources and, hence, generate this demand for registration. Where such capacity is negligible, states are unable to regulate access to limited public resources and, hence, few citizens are incentivized to comply (Powell 1981). At high levels of capacity, states can generate broad-based demand by conditioning access to a wide set of resources on the basis of registration status (Harbitz and Boekle-Giufrida 2009).

[5] As one Tanzanian newspaper wrote, "A census is a herculean exercise that entails long-term planning and high financial and manpower resources." Comparatively, 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*, August 31, 1982). Survey data from sub-Saharan Africa suggest that a third of all citizens had attempted to obtain identity documents from the state in the prior year (Afrobarometer 2019).

[6] Recent examples discussed by Clark (2018), including from India, Pakistan, and Uganda, illustrate how foundational identity system data are 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 (Longman 2018). More indirectly, if citizens instrumentally benefit from registration schemes, they may become more willing to comply with demands for taxation (Levi 1988).

[7] A review by DLA Piper (2016) 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.

[8] This stylized framework makes important simplifications relating to citizens' economic status (higher or lower) and the basis of public resources (broad or narrow). While empirically justified by the cases considered in this article, these simplifications are potentially reductive in settings where economic status is less bifurcated or where the private provision of equivalent resources renders it challenging to define whether public resources are broad- or narrow-based.

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In intermediate cases, states can only selectively regulate access to public resources.[9] Controlling access to *broad-based* areas of service delivery imposes high 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 the bureaucrats responsible for assessing eligibility. By comparison, it is less bureaucratically taxing to regulate access to more *narrow-based* areas of service delivery, such as higher education levels, which are both demanded by fewer citizens and likely to be located in areas where the state possesses preexisting bureaucratic infrastructure. Limitations in this capacity imply that the instrumental benefits of registration are more likely to center on areas of service delivery relevant for a narrow set of relatively wealthier 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 democratic political competition render their decision particularly fraught. Because political leaders then have both some capacity to tax enrolled citizens and incentives to redistribute their fiscal resources, they cannot commit *against* redistributing the extracted taxes of wealthier citizens toward the poorer majority (Kasara and Suryanarayan 2015). Further, dampening the benefits of registration, strong democratic competition might disincentivize political leaders from the enforcement of policies restricting access to public resources (Gottlieb 2021; Holland 2017). Last, to the extent that high competition implies frequent political turnover, in highly stratified settings, this might amplify citizens' fears regarding the indirect costs, as well as sustained longer-term benefits, of complying with state demands (Padró i Miquel 2007).

When bureaucratic capacity is moderate and democratic political competition is weak, efforts to register citizens then have economically regressive consequences for the state's informational coverage. First, the state's limited extent of bureaucratic capacity renders the expected future benefits of registration more credible with respect to narrow-based public resources than broad-based ones. Second, in spite of the increased exposure to taxation they expect from complying with the state's demands, limited competition mitigates the threat of the redistribution of wealthier citizens' resources to the poorer majority.

This distributive incidence has two implications. First, states more easily structure informational transactions with higher economic status citizens to induce their compliance even ignoring the fact that these citizens face lower direct costs of registration due to their potential proximity to registration infrastructure, financial resources, or greater prior experience with the bureaucracy. Second, this incidence is incentive-compatible, and hence self-enforcing, to the extent that states fiscally benefit from inducing the taxation of wealthier citizens to a greater extent than poorer citizens.[10] Because expanding its informational coverage would either require the costly regulation of access to broad-based public resources, or resource-intensive investment in infrastructure to reduce the direct costs of registration facing poorer citizens, constrained states then face limited incentives to broaden their coverage given the returns from doing so.

Applicability of the Theoretical Framework

We should only expect these stratified implications in settings where economic status meaningfully predicts access to, and exclusion from, public resources. Importantly, this does not preclude contexts where distribution is often characterized as centering on non-economic divides. While a large literature is premised upon 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 distribution (Alesina, Michalopoulos, and Papaioannou 2016; Baldwin and Huber 2010; Huber and Suryanarayan 2016).

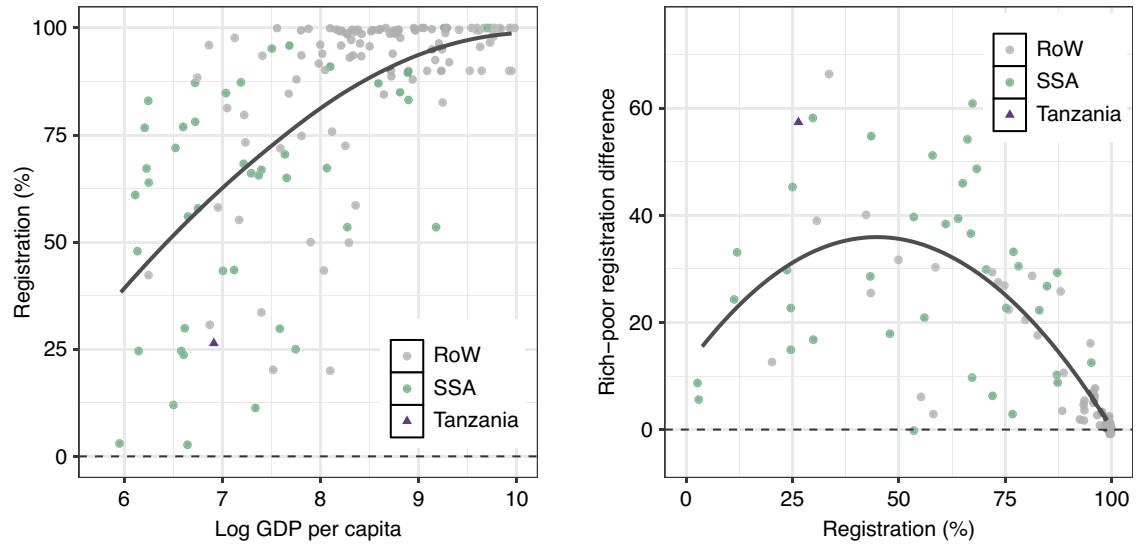
Figure A1 in the Supplementary Material, using cross-national data, demonstrates the strength of the relationship between exclusion from public resources according to social group status and exclusion according to economic status. As such, while non-economic cleavages can surely exert theoretically distinct effects on state-citizen interaction,[11] 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 toward this end. In settings where fiscal incentives are sufficiently absent, such as rentier states, we should expect that such efforts have limited effects on the distribution of access to the state.

[9] Breckenridge (2014), in his account of biometric identity systems in South Africa, underscores the tremendous costs of administering identity registration systems in constrained capacity settings when citizens face incentives to avoid the state's extractive reach.

[10] Importantly, this is consistent with states still benefiting to some extent from inducing the registration of relatively poorer citizens. As bureaucratic capacity increases, the threshold value of citizens' income where the benefits of inducing their registration (through taxation) exceeds the costs (through regulating access) ought to decrease. For sufficiently high capacity states, these benefits likely exceed the minimal costs even absent strong distributive pressures. In such settings, robust states might additionally benefit from registration schemes for broader purposes of social control (Caplan and Torpey 2001).

[11] 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, increases citizens' indirect costs of registration.

FIGURE 1. Income, Registration, and Inequality



(a) Income and registration levels (b) Registration levels and inequality

Note: *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. RoW: rest of world; SSA: sub-Saharan Africa. *Source:* World Bank and UNICEF.

Empirical Implications

Economic inequalities in registration, access, and taxation are observed across a wide set of countries.[12] The theoretical framework implies that, moderated by capacity and competition, inequality in citizens' registration is not simply epiphenomenal to these other inequalities in state–citizen interaction, but plays a constitutive role in entrenching and propagating them. Consistent cross-national evidence in Figure 1a documents the overall positive cross-country relationship between levels of economic development and civil registration rates. Figure 1b, however, illustrates how *levels* of registration mask important nonlinearities in the *economic 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.[13] While suggestively consistent with the selective benefits of compliance 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 simply faced lower direct costs of registering with the state.

There are two key empirical implications of the theoretical framework which underlie this initial regressivity 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.

TANZANIA

The early postindependence period in Tanzania offers a useful case for testing these implications. Before providing quasi-experimental evidence in support of these implications, in this section, I first draw out features of the case salient to the theoretical framework and provide qualitative evidence on citizens' decisions to register with the state.

Salient Features of the Tanzanian Case

First, with regard to its capacity, Figure A3 in the Supplementary Material 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.[14] This classification of moderate capacity is supported by the state's partial regulation of access to

[12] While I focus throughout on sub-Saharan Africa, existing work on Latin America underscores that such inequalities are observed across many regions (Hunter and Brill 2016).

[13] Indicative of the extent of their correlation, Figure A2 in the Supplementary Material replicates this inverted U-shaped relationship with respect to either levels of economic development or measures of state capacity on the *X*-axis.

[14] This measure reflects important temporal variation in capacity, which was relatively low in the immediate postindependence period but significantly developed over the subsequent decades as the state exerted increased control over society.

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public resources. With respect to education, the Tanganyika African National Union (TANU) regime was able to significantly control access to higher levels of education, whereas efforts to expand the provision of primary education were only weakly regulated and overseen (Court 1976; Resnick 1968). With respect to social security, the government exerted tight control over access to the formal sector, such as valuable state jobs and pensions, particularly in more urban localities (Tripp 1989).

Further, scholars underscore the state's inability to effectively coerce its population: as Hyden (1980) notes, the TANU regime initially "relied more on exhortation and persuasion than on compulsion" (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. 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 citizens—such as to formalize urban workers (Diouf and Fredericks 2014; Tripp 1989) or employ tax field units (Bienen 1970)—were strongly resisted 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 limited within-party competition while banning opposition parties (Bienen 1970; Cliffe 1967). These low levels of political competition have persisted, with TANU and its successor party remaining dominant since independence. Importantly, in spite of this lack of meaningful political competition, TANU's socialist orientation rendered it highly redistributive in its pursuit of a rural agrarian mode of development (Hyden 1980). Lee (1965) shows that individuals in the highest income bracket were subject to effective local tax rates twice as high as those in lower income brackets, whereas Huang (1976) shows that high income citizens faced effective direct tax rates on their income of around 30%, whereas those in the lowest income groups faced no direct taxation by the central state. This progressive tax incidence contrasted with most other developing country settings at the time (Coulson 1982).

Third, economic status has been enduringly predictive of access to public resources. Inheriting a relatively fragmented distribution of ethnic groups upon independence, TANU was able to implement nation-building initiatives which further reduced the salience of ethnicity as a cleavage structuring distribution (Glickman 1969; Mueller 1981). In its place, economic status became more strongly associated with distribution, with relatively high levels of economic inequality and much of the country's wealth concentrated in very few districts (Coulson 1982; Tordoff 1967). Figure A1 in the Supplementary Material highlights that socioeconomic 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 initially limited inflows from international donors, the state had clear incentives to extract taxation from its citizens (Due 1963).

Together, these features suggest that Tanzania is an appropriate, but nontrivial, case in which to examine the stratified consequences of states' initial efforts to register their citizens. On the one hand, its moderate bureaucratic capacity afforded it some control over the regulation of access to public resources and policy implementation was conditioned by citizens' willingness to comply. On the other hand, in spite of limited political competition, the orientation of the TANU regime posed a redistributive threat to economic elites.

Evidence on Citizens' Registration Calculus

Government efforts to register the population in Tanzania began in 1894 and, as described in the research design below, intensified in the postindependence period (Kuczynski 1948). These efforts were rendered deeply incomplete—even today, under a fifth of the population possesses a birth certificate. A key reason for this failure is that citizens were acutely aware of the trade-offs they faced in supplying information to the state: as Kjekshus (1974) concludes, "the Tanzanian citizen responds to a series of rational calculations of benefits and sacrifices connected with registration" (133). The key risk associated with registration during this period was that of taxation. The strength of the registration–taxation link inhibited birth registration in the colonial period, with the same officials often responsible for both registration and taxation (Walters 2016; Wood 2016). 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, 31). Citizens' fears were sufficiently extreme that the Vice President was forced to publicly declare that voter registration would *not* induce taxation, but low registration totals suggest that his promise was often 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 (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, whereas Fjeldstad and Therkildsen (2008) report that such taxes accounted for nearly all of Tanzania's local revenues. Undermining effective administration, 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" (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

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claim making on its bearer" (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 the poor with the reality that often "birth certificates and the benefits they are supposed to provide remain largely aspirational" (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,[15] whereas 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 access to more recently developed national identity cards (ITU 2015). An additional set of potential uses relates to access to economic formality. 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). How binding these uses are in practice remains an empirical question.

Early postindependence Tanzania is, therefore, rendered a useful case by its moderate bureaucratic capacity, weakly competitive politics, and significant economic cleavages. In turn, qualitative evidence underscores that citizens carefully considered the trade-offs they faced in registering with the state, with descriptive evidence contrasting the salient threat of taxation against the relatively limited benefits of registration. This motivates the two key empirical predictions to be quantitatively tested in the context of government efforts to register the population: that registering with the state generates narrow benefits in access; and that citizens' decisions to register are accordingly conditioned by the stratified local balance of these returns.

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' decisions to comply with the reform.

Data Sources

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 4 million people. Second, I use data on around 30,000 respondents from the National Panel Survey (NPS), a nationally representative household consumption survey.[16] Due to its much larger sample size, I draw outcomes from the census sample where possible. Descriptive statistics for the baseline census sample are provided in Table A1 in the Supplementary Material and for the NPS sample in Table A2 in the Supplementary Material.

Dependent Variables

The first outcomes for evaluating the consequences of registration relate to access to public goods. I focus on access to education and social security since prior scholarship on Tanzania makes clear the extent to which the central government exerted selective control over each sector (Coulson 1982; Court 1976; Resnick 1968; Tripp 1989). Using the census sample, I divide access to education into indicators for individuals having any primary, secondary, or university education. I divide access to social security into indicators for individuals having access to public national health insurance, a pension indicative of prior private sector employment, and a pension indicative of prior government employment.[17]

The theoretical framework requires classifying outcomes, within these sectors, based on their particular utility for higher economic status citizens. To do this, I compute the partial correlation coefficient ρ(Wealth, DV) between a measure of economic status and each dependent variable.[18] Consistent with intuition and the qualitative evidence discussed above, this exercise suggests that we should expect effects of registration on access to higher levels of education more than basic education, and effects on access to formal sector social security through employment-linked pensions relatively more than access to redistributive health insurance funds.

Second, 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 taxes, and taxes on income and property to the central government. Theoretically, the link between citizens' identity registration and their tax exposure is likely to be

[15] One reason for this relates to the necessity of verifying the identity of individuals taking school leaving examinations (Suthar et al. 2019).

[16] 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) because Round 1 does not include a question on registration status and Round 3 comprises an identical sample to Round 2.

[17] Private sector pensions are defined as those households with access to the National Social Security Fund or the Parastatal Pension Fund, whereas government pensions are defined as those households with access to the Public Service Pension Fund, Government Employee Provident Fund, or Local Authority Pension Fund.

[18] More formally, I standardize and regress the outcome variable DV onto a standardized measure of economic status and the controls in Equation 1, with ρ corresponding to the coefficient on the economic status variable. These measures are based on indexes of asset ownership recorded in both the census and NPS samples.

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stronger for centrally administered taxes, where states face more acute informational challenges, relative to locally administered taxes (Kiser and Sacks 2009). Consistent with this, the ρ coefficients suggest that we should most strongly expect effects of registration on the payment of central government taxes.

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. Additionally, I observe the district and year of birth of each individual, which enables their assignment to the reform described below.

To investigate the second implication of the theoretical framework, I examine heterogeneity in citizens' decisions to register as a function of the local net returns to doing so. First, I consider local variation in the *levels* of taxation using district-level data from Jensen and Mkama (1968): both the share of individuals paying taxes, which captures the extensive margin; and the amount of tax collected per taxpayer, capturing the intensive margin. To proxy for local redistributive pressures, I use data on the *incidence* of taxation from Lee (1965) measured by the difference in a district's tax rate faced by individuals in the highest tax bracket compared with those in the lowest.[19] Second, I consider local variation in the presence of public goods using geolocated administrative data for primary and secondary schools which were founded prior to the mid-1960s.[20] Third, to assess the economic characteristics of registrants, I use data from the NPS sample relating to the educational attainment of a given respondent's parents.

Estimation

To gain intuition for the empirical strategy, consider the following "naive" estimating equation:

$$y_{itd} = \beta^{OLS}\text{Registered}_{itd} + \eta_d + \mu_t + \gamma X_i + \epsilon_{itd}, \quad (1)$$

where y_{itd} is an outcome for individual i born in year t in place d; Registered_{itd} is an indicator for whether i is registered, and hence possesses a birth certificate; η_d are the place of birth fixed effects; μ_t are the year of birth fixed effects; X_i are the flexible controls for individual-level covariates, such as gender; and standard errors are clustered at the place of birth level. The fixed effects control for both time-invariant characteristics of individuals born in the same place and for temporal changes that affect individuals born across areas equally in a given year.

For β^{OLS} to identify the causal effect of registration, 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 locality, wealthier citizens might have easier access to registration *and* other public resources, or might be more easily coerced both to register *and* pay taxes. Or, indicative of reverse causality, citizens already enjoying preferential access to resources might 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 a given administrative area.[21]

This legal reform was first applied in 1966 to make birth registration obligatory for citizens born after a given date in a set of localities comprising the "major towns" (Wood 1971). 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 and increased threats of punishment for noncompliance. Appendix A.1 of the Supplementary Material provides additional information on the reform. Intended as a demand-side shock for registration with the state, the reform was targeted at areas which contained just 5% of the Tanzanian population but a far higher share of its directly taxable wealth (Jensen and Mkama 1968; Tordoff 1967). 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).

As states wrangled with the informational voids they had inherited upon independence, similar government efforts were common across the region during this period (Breckenridge and Szreter 2012; Dalberto and Banégas 2021).[22] Several former British colonies, such as Botswana, Ghana, Kenya, Sierra Leone, and Zambia, selectively targeted reforms to induce the registration of citizens living in urban, wealthier areas (Makannah 1981; Mehta and Assie 1979; United Nations 1985). Across former French colonies, such as Senegal and Côte d'Ivoire, states often enforced existing legal requirements to register with the state

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

[20] Indicative of variation in the ease of their access and regulation, the data suggest that the average district had 12.4 primary schools in 1965 but just 0.5 secondary schools.

[21] 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).

[22] Figure A4 in the Supplementary Material plots the rapid increase in the share of African countries with formal institutions to register citizens in this period.

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as a function of geographical proximity to urban registration centers (Barré 2021; Brass 1968; Cooper 2002). After this initial period, efforts to register citizens tended to stagnate (Padmanabha 1993).[23]

Identification Strategy

I leverage citizens' exposure to the 1966 reforms in Tanzania 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: individuals born in a "treated" area should be more likely to be registered than those born in a "control" area. By itself, this comparison is confounded by the general differences between treated areas and the rest of the country. As such, I combine this spatial variation with *temporal* variation: individuals born after the reform in one of the treated areas 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 (vs. before) the reform in places which were (vs. were not) treated by the reform. If the reform induced registration as intended, we should observe a larger difference in registration among cohorts born after versus before the reform in treated areas than in control areas. The baseline equation I estimate is the following:

$$\text{Registered}_{itd} = \beta^{FS}\text{Reform}_{itd} + \eta_d + \mu_t + \gamma X_i + \epsilon_{itd}, \tag{2}$$

where exposure to the reform, Reform_{itd}, is an indicator variable for whether i was born after the reform in one of the treated areas, and the other variables are as defined above for Equation 1. 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 as a first stage to predict registration, $\widehat{\text{Registered}}_{itd}$:

$$y_{itd} = \beta^{IV}\widehat{\text{Registered}}_{itd} + \eta_d + \mu_t + \gamma X_i + \epsilon_{itd}, \tag{3}$$

where y_{itd} is a given outcome variable and the other variables are as per Equation 1 and Equation 2. Throughout, I define the place of birth fixed effects, η_d, to correspond to administrative units as existing prior to the reform (as discussed below) and cluster at this level. Following recent work on instrumental variables inference, I report p-values based on a clustered bootstrap (Lal et al. 2021; Young 2022).[24]

β^{IV} in Equation 3 estimates the local average treatment effect: the causal effect of registration over time among compliers induced to be registered by the reform. Then, to probe the *determinants* of registration, I examine heterogeneity in the first stage relationship. I do this by interacting the reform indicator in Equation 2 with the independent variables discussed above to evaluate how the local balance of these benefits and costs conditioned citizens' compliance decisions.

Identification Assumptions

β^{IV} in Equation 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 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 areas (where the reform was applied) relative to control (where it was not).[25]

I maximize the plausibility of this assumption in two ways. First, through sample restrictions. In the baseline specification, I restrict the sample only to comprise cohorts born within 10 years of the reform and restrict the set of control birthplace areas. Control areas are restricted to be those which shared the same "parent district" as treated areas as defined prior to the reform. For example, Arusha town was treated with the reform and was nested within the larger Arusha district at the time of the reform. Treated areas as observed today, therefore, comprise the modern administrative district corresponding to Arusha town (Arusha Urban), whereas control areas comprise those districts corresponding to Arusha district (Arusha Rural and Meru districts).[26] For parsimony, I refer to these areas, for example, Arusha town and

[23] Suggestively consistent with the challenge of broadly inducing citizens' registration, as well as the limited fiscal incentives the state faced to expand its informational coverage fully, Appendix A.2 of the Supplementary Material discusses the gradual expansion of these compulsory registration orders to more rural districts in Tanzania over time. I show that these subsequent orders, which took four decades before all citizens across the country had a nominal legal obligation to register with the state, were largely ineffective.

[24] Specifically, I use the studentized clustered bootstrap as per Canay, Santos, and Shaikh (2021), although results are robust to instead resampling the non-studentized distribution of bootstrap coefficients (Young 2022).

[25] Additionally, the identification of β^{FS} rests on the stable unit treatment value assignment assumption that individual i's exposure to the reform does not affect j's registration status. The robustness tests in Panel B of Table A6 in the Supplementary Material, which include household-level fixed effects, suggest that this no-interference assumption is plausibly satisfied.

[26] Figure A5 in the Supplementary Material plots the location of these districts and Table A3 in the Supplementary Material maps relevant administrative units from the reform period to modern districts as observed in the outcome data sources as respondents' districts of birth. The fixed effects used in the estimations reflect pre-reform administrative units (i.e., Arusha town or Arusha district, using the above example) rather than the corresponding modern administrative units (i.e., Arusha Urban, Arusha Rural, and Meru) to avoid potential issues arising from the use of posttreatment fixed effects.

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Arusha district, as distinct "districts" below since they correspond to modern districts. 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. These time trends absorb either region-specific, or district-specific, trends in registration over time.

Supportive of the plausibility of the identifying assumption, Figure A6 in the Supplementary Material provides the visual evidence of parallel registration rates among individuals born in the period leading up to the reform. To more formally test this, in the analysis below, I include specifications where I add lead terms to Equation 2 reflecting i's treatment status in years $t+1$, $t+2$, and so on. I find no effects on these lead terms, which would otherwise provide evidence against the parallel trends assumption by suggesting nonparallel pre-trends.

If β^{FS} identifies the effect of exposure to the 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 A7 in the Supplementary Material. 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.3 of the Supplementary Material provides supporting evidence toward the plausibility of this. In brief, a quantitative analysis of legislation during this period provides no evidence of other policies targeted specifically at the treated areas. Further, while the Arusha Declaration during this period heralded Tanzania's transition to a rural model of development, (1) the sample restrictions exclude the most rural areas; (2) its immediate implications were relatively limited; and (3) it is hard to account for *Arusha* affecting outcomes in ways consistent with the observed results.

RESULTS

Table 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. In columns 1–3, I use the baseline specification of Equation 2, whereas in columns 4–6, I add 5 years of treatment lead terms to examine pre-trends, as discussed in the previous section. I linearly add the 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.

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

TABLE 1. Effect of Reform on Registration

	(1)	(2)	(3)	(4)	(5)	(6)
Reform (β^{FS})	0.06***	0.05***	0.04***	0.05***	0.05***	0.03**
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
$Reform_{t+1}$				0.02	0.02	0.02
				(0.01)	(0.01)	(0.01)
$Reform_{t+2}$				−0.01	−0.01	−0.01
				(0.02)	(0.02)	(0.02)
$Reform_{t+3}$				−0.00	−0.00	−0.01
				(0.01)	(0.01)	(0.01)
$Reform_{t+4}$				−0.01	−0.01	−0.01
				(0.01)	(0.01)	(0.01)
$Reform_{t+5}$				0.00	0.00	−0.00
				(0.01)	(0.01)	(0.01)
Time trends	None	Region	District	None	Region	District
F-statistic	29.8	140.7	12.9	17.4	24.1	6.9
Outcome mean	0.13	0.13	0.13	0.13	0.13	0.13
No. of obs.	193,648	193,648	193,648	193,648	193,648	193,648

Note: DV: respondent has a birth certificate. The sample is restricted to cohorts born within 10 years of the reform in the *treated* or *control* districts. The specifications are estimated using OLS including the district of birth and year of birth fixed effects and gender-district of birth controls. Exposure to reform is an indicator for being born after the reform in a *treated* district. Standard errors clustered at the district of birth level are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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TABLE 2. Effects on Access to the State

	I. Education			II. Social security		
	Pri.	Sec.	Uni.	HI	Priv.	State
	(1)	(2)	(3)	(4)	(5)	(6)
Registered (β^{OLS})	0.11***	0.34***	0.10***	0.11***	0.12***	0.11***
	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
$\widehat{\text{Registered}}$ (β^{IV})	0.08	0.72***	0.17**	0.21	0.15*	0.18**
	(0.35)	(0.13)	(0.06)	(0.13)	(0.07)	(0.08)
	[0.83]	[0.00]	[0.01]	[0.17]	[0.07]	[0.04]
DV Mean	0.80	0.15	0.03	0.09	0.05	0.04
FS *F*-statistic	29.8	29.8	29.8	29.8	29.8	29.8
ρ(Wealth, DV)	0.12	0.42	0.29	0.17	0.33	0.26
No. of obs.	193,648	193,648	193,648	193,648	193,648	193,648

Note: DVs are all indicators. (1) has any primary education; (2) has any secondary education; (3) has any university education; (4) in a household accessing the National Health Insurance Fund; (5) in a household accessing a private pension; (6) in a household accessing a state pension. The sample is restricted to cohorts born within 10 years of the reform in the *treated* or *control* districts. β^{OLS} is estimated using Equation 1; β^{IV} is estimated using Equation 3. All specifications include the district of birth and year of birth fixed effects and gender-district of birth controls. Standard errors clustered at the district of birth level are reported in parentheses; bootstrapped *p*-values are reported in square brackets. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

represents an effect size of nearly 50% compared with the outcome mean of 13%. The treatment effect is significant at the 1% level and associated with an *F*-statistic of 30, which suggests that it can be considered a strong instrument. The addition of substantially more demanding time trends only marginally decreases the coefficient estimate. There is little evidence, in columns 4–6, of nonparallel 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 A4 in the Supplementary Material 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 control variables; (D) modifying the set of control districts; (E) permuting the fixed effects and clustering units; and (F) estimating the same first-stage relationship instead using the NPS dataset. Second, Table A6 in the Supplementary Material provides alternative estimation strategies, using either (A) local linear regression or (B) household-level fixed effects. Third, in Figure A7 in the Supplementary Material, I estimate a district-level jackknife of the first-stage coefficient to test for the presence of outlier districts driving the first-stage relationship (Young 2022). β^{FS} remains stably estimated across these tests.

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} coefficient obtained by estimating Equation 1, whereas the second reports β^{IV}, the primary coefficient of interest, obtained by estimating Equation 3. The ρ coefficient is reported at the bottom of each table to guide interpretation.[27]

Access to the State

In Table 2, I report the effects of registration on access to the state. Panel I reports outcomes relating to access to education, whereas panel II reports outcomes relating to access to social security. 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 pp more likely to possess primary education, 34 pp more likely to possess secondary education, and 10 pp more likely to have university education. Examining the ρ vector shows that economic status correlates most strongly with access to postprimary education. The causal β^{IV} estimates, accordingly, point to more selective benefits of registration on access to the state. These 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 postprimary education, whether for secondary education or university, with effect sizes even larger than the β^{OLS} estimates.[28]

[27] Figure A8 in the Supplementary Material plots descriptive trends in the various outcome measures across treated and control districts across cohorts.

[28] Figure A9 in the Supplementary Material provides the estimates of β^{IV} for access to education by specific *grade* of schooling. The figure, consistent with Table 2, shows the null effects of registration on access to all levels of primary education (P1–P7), but significantly positive

TABLE 3. Effects on Exposure to Taxation

	All	Fees	Local	Central
	(1)	(2)	(3)	(4)
Registered (β^{OLS})	0.18***	–0.00	0.05**	0.18***
	(0.03)	(0.02)	(0.02)	(0.04)
$\widehat{\text{Registered}}$ (β^{IV})	0.47*	0.20	0.02	0.45*
	(0.28)	(0.22)	(0.21)	(0.28)
	[0.10]	[0.62]	[0.91]	[0.08]
DV Mean	0.19	0.05	0.05	0.13
FS *F*-statistic	17.5	17.5	17.5	17.5
ρ(Wealth, DV)	0.36	0.04	0.28	0.42
No. of obs.	1,571	1,571	1,571	1,571

Note: 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 local taxes in the last year; (4) in a household which paid taxes to the central government in the last year. The sample is restricted to cohorts born within 10 years of the reform in the *treated* or *control* districts. β^{OLS} is estimated using Equation 1; β^{IV} is estimated using Equation 3. All specifications include the district of birth and year of birth fixed effects and gender-district of birth controls. Standard errors clustered at the district of birth level are reported in parentheses; bootstrapped *p*-values are reported in square brackets. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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), 12 pp more likely to have access to a private sector pension, and 11 pp more likely to have access to a state pension. The ρ vector suggests less pronounced differences compared with access to education, but that economic status 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 noisier than for the other two outcomes.[29]

Taxation

In Table 3, using the baseline NPS sample, I examine the extent to which registration induces increased exposure to taxation among the compliers. The β^{OLS} estimates suggest that registration is associated with an 18 pp increase in the probability of making payments to the state. The ρ vector suggests that economic status more strongly predicts the payment of central rather than local taxes or fees to the government. While somewhat noisily estimated given the much 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 formal taxes to the central government (column 4). In Table A10 in the Supplementary Material, I estimate effects without restricting the set of cohorts included in the sample, which provides more precise, but very similar, point estimates.

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 relatively wealthier citizens. 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 implication of the 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 toward 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

coefficients on access to secondary education (S1–S4), advanced secondary education (S5 and S6), and university education. Table A8 in the Supplementary Material provides estimates relating to literacy to support the effects on access to education. The estimates suggest no effect on literacy in Kiswahili, the typical language of instruction for primary education, but strongly positive effects on literacy in English, typically taught at higher levels of education.

[29] Table A9 in the Supplementary Material disaggregates the effects on social security access. The estimates show that effects on access to private pensions are driven by increased access to the National Social Security Fund (NSSF), which provides social security funds for individuals primarily with formal sector employment, and effects on access to state pensions are driven by increased access to the Public Service Pension Fund (PSPF) and Government Employee Provident Fund (GEPF), each of which indicate prior employment by the central state.

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TABLE 4. Heterogeneity in Compliance with Reforms

		I. Taxation				II. Local public goods		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform	0.060***	0.053***	0.054***	0.054***	0.056***	0.062***	0.051***	0.059***
	(0.011)	(0.006)	(0.005)	(0.007)	(0.006)	(0.018)	(0.005)	(0.012)
Reform × Share paying tax		−0.011**			−0.013***			
		(0.005)			(0.003)			
Reform × Tax per capita			−0.028*		−0.022**			
			(0.015)		(0.009)			
Reform × $(\tau^{\text{Max}}-\tau^{\text{Min}})$				−0.008**	−0.002			
				(0.004)	(0.003)			
Reform × Primary schools						0.022		0.020
						(0.028)		(0.020)
Reform × Secondary school							0.016***	0.016***
							(0.004)	(0.004)
DV mean	0.13	0.13	0.13	0.14	0.14	0.13	0.13	0.13
No. of obs.	193,648	193,648	193,648	160,588	160,588	193,648	193,648	193,648

Note: DV: respondent has a birth certificate. Panel I examines heterogeneity using local measures of taxation; Panel II using local measures of public goods. The sample is restricted to cohorts born within 10 years of the reform in the *treated* or *control* districts. District-level measures of tax levels are from Jensen and Mkama (1968); tax incidence is from Lee (1965); and local public goods are from administrative data. Regressions control for the interaction of *Reform* with standardized district income per capita and population density. All specifications are estimated using OLS including the district of birth and year of birth fixed effects and gender-district of birth controls. Standard errors clustered at the district of birth level are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

the threat of taxation at the time of the reform. In panel I of Table 4, I estimate Equation 2 while interacting the reform indicator with the local measures of taxation introduced above.[30] The results show that a 1-standard-deviation increase in the *levels* of taxation, whether on the extensive (column 2) or intensive (column 3) margins, reduces the extent of citizens' compliance with the reform. Further, consistent with wealthier citizens' concerns over the threat of redistribution, column 4 demonstrates that citizens' compliance was lower in districts where the local *incidence* of taxation $(\tau^{\text{Max}}-\tau^{\text{Min}})$ was most progressive.[31] Estimating these interactions simultaneously suggests that the extensive and intensive margins may have been more important in conditioning compliance than the local incidence of taxation (column 5). Together, the increase in citizens' registration induced by the reform is primarily driven by changes in localities where citizens, and particularly wealthier citizens, faced a relatively lower threat of taxation.

Second, to provide evidence on how variation in the expected benefits of registration affected compliance, in panel II of Table 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 estimates of β^{IV} in panel I of Table 2, I construct a measure of the local presence of *narrow-based* public goods in a district, using secondary schools, and *broad-based* public goods, using primary schools.[32] 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 column 8.

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 Angrist and Fernandez-Val (2013), in Table 5, I assess descriptive characteristics of these compliers compared to the overall sample. I consider gender and the educational attainment of individuals' *parents*, which offers a pretreatment proxy for the extent to which a given individual is born into a family with higher economic status.

This exercise suggests that the reform induced the registration of boys marginally more than girls: compliers are 6% more likely to be male than among the broader sample.[33] Comparisons using parental educational

[30] 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.

[31] This result aligns with prior work finding that economic elites become more likely to undermine the state's fiscal capacities as economic inequality increases (Hollenbach and Silva 2019).

[32] 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.

[33] Prior work on the gendered dimensions of civil registration demonstrates variation in the timing of registration (Harbers 2020) but

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TABLE 5. Complier Characteristics

Variable	Sample mean (1)	Complier mean (2)	Ratio (3)
Male	0.47	0.50	1.06
Parent has primary education	0.60	0.67	1.13
Parent has secondary education	0.09	0.17	2.02
Parent has university education	0.02	0.08	3.54

Note: This table computes covariate means for baseline NPS sample (column 1) and compliers (column 2). Column 3 is column 2 divided by column 1.

attainment, however, show striking differences: compliers are only 13% 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 education. The complier characteristics are consistent with the reform inducing individuals born into relatively wealthier families in these districts, for whom the returns to enrollment were positive in expectation, to be registered.

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

CONCLUSION

A growing literature highlights the informational foundations of state capacity. In this article, 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 toward areas of particular utility for wealthier citizens. In turn, these citizens are induced to comply and 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 postindependence 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, the results provide novel evidence regarding the dynamics of state-building efforts to build informational capacity. 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 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 the nature of the research design limits this evidence to one setting, the theoretical framework, existence of similar government efforts across a range of countries, and broader ubiquity of economic inequalities in registration, state access, and taxation, suggest that such dynamics are likely to have relevance elsewhere. Both theory and empirics suggest that a truncated state-building equilibrium limits the equalizing potential of such technologies to empower citizens to make demands upon the state. This equilibrium is just as much *political* in origin as it is administrative or bureaucratic: the theoretical framework implies that states often face only weak incentives to expand their informational coverage more broadly. Such expansions carry high resource costs—whether through regulating access to broad-based public resources or through investing in infrastructure to reduce citizens' costs of enrollment—but also might risk disrupting the willingness of higher economic status citizens to comply with the state's informational demands if they fear the redistribution of their resources.

Such technologies, instead, risk entrenching and propagating preexisting economic inequalities in state–citizen interaction when compliance, and its benefits, cannot be taken for granted. While much of the existing literature has tended to focus on the aggregate consequences of states' capacities for societal outcomes, this suggests that the interaction between economic inequality and state-building processes might help to explain the otherwise puzzling underinvestment in states' capacities over time. Further, given that variation in the informational transactions struck between state and citizen undergirds these inequalities, an increased focus on the local political economy of citizens' decisions to comply with state-building efforts is needed to understand variation in both the levels and *distribution* of informational capacity across societies. To render the state's informational coverage more even, it is not enough to decrease citizens' direct costs of registering with the state: rather, citizens must receive sufficient incentives to make the decision worthwhile. As many developing countries rapidly introduce novel biometric technologies for the identification of citizens, an increased focus on these

provides relatively limited evidence on other gender biases in registration decisions (Bhatia et al. 2019).

distributive dynamics is crucial for understanding the future political economic impacts of efforts to register the world's poor.

SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit https://doi.org/10.1017/S0003055423000394.

DATA AVAILABILITY STATEMENT

Research documentation and data that support the findings of this study are openly available in the American Political Science Review Dataverse at https://doi.org/10.7910/DVN/OM6ZUR.

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CONFLICT OF INTEREST

The author declares no ethical issues or conflicts of interest in this research.

ETHICAL STANDARDS

The author affirms this research did not involve human subjects.

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