

Barriers to Formalization: Evidence from Six Randomized Experiments on Transaction Costs, Public Services, and Taxation in the Global South

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June 8, 2026

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Abstract

We present six harmonized RCTs to assess whether removing bureaucratic hurdles can encourage individuals to engage formally with the state in six countries in the Global South. Recent work has argued that the trade-offs inherent to formalization are more acceptable to individuals when formalization is tied to publicly-derived benefits, such as access to legal recourse in disputes, public services, or public utilities. Yet, even in these cases, bureaucratic barriers to formalization might be insurmountable. So far, disconnected research on different bureaucratic procedures has led to contrasting conclusions about how entry costs affect formalization. Our interventions involved in-person assistance to reduce the upfront transaction costs of dealing with the bureaucracy in three types of policy domain: titling property, registration of small businesses, and access to public utilities (i.e., municipal water) and public services (i.e., garbage collection). A meta-analysis shows that the average effect of these interventions on individuals' formalization, tax payment, and access to services is indistinguishable from zero. We also find substantial heterogeneity in individuals' intent to undertake and complete the bureaucratic process. Across policy domains, individuals are more willing to bear formalization's downstream costs when the benefits are individual. Our results also suggest that local bureaucratic incentives must be aligned for demand-side interventions to work.

1 Introduction

Bureaucracies impose substantial burdens on citizens, particularly in low- and medium-growth economies (World Bank 2025). Decades ago, scholars and policy makers alerted governments that the costs of navigating complex bureaucratic procedures are often so prohibitive that informality becomes a practical alternative for many, particularly low-income individuals (de Soto 1989; Perry et al. 2007). In response, governments have repeatedly sought to facilitate citizen-state interactions by making it easier and faster to comply with regulations and access to public services. Notable examples include land titling programs, which offer a simplified procedure and subsidized titling fees, “one-stop-shop” registrations to simplify the small firm start-up process, campaigns to ease access to public utilities, among other initiatives. However, as a recent World Bank report shows, there is often a mismatch between governments’ rules and regulations and how easy they are to follow. The upfront costs of dealing with bureaucracies are still considered a reason why many citizens forgo engaging with the state, particularly in low-growth contexts (World Bank 2025). Easing such barriers might help citizens comply with regulations and access services, which might allow governments to collect more taxes and fund higher quality public services and, possibly, spur economic development. But is it the case that making it easier for citizens to engage with the state triggers such a causal chain?

Using randomized controlled trials (RCTs) in six countries, we investigate whether equipping citizens to interact with the bureaucracy leads to greater citizen engagement with the state, increased tax payments, and increased access to state services. We focus on urban contexts where citizens typically do not engage directly with bureaucracies (Heller, Swaminathan, and Varshney 2023; Kruks-Wisner 2018). Instead, they tend to employ intermediaries, such as local politicians, organizations with informal ties to political parties, community development associations, and brokers, to address a wide range of problems, from property disputes to securing forbearance to operate in the informal economy, living in informal settlements or gaining access to services (Auerbach et al. 2018). In these contexts, governments often fail to provide information and assistance to citizens to navigate regulations and access services (Joshi, Prichard, and Heady 2014; Henn et al. 2026; World Bank

2025). This problem is prevalent and particularly affects low-income households and micro-businesses in which illiteracy, lack of accounting skills, poor information, and mistrust of government prevail (Joshi, Prichard, and Heady 2014). To address this problem, scholars and policymakers have expressed the need for governments and non-governmental organizations to deploy outreach campaigns and expand taxpayer services (Joshi, Prichard, and Heady 2014; World Bank 2025).

Our empirical investigation is guided by the expectation that the effect of reducing the costs of dealing with the bureaucracy on citizen-state interactions is theoretically unclear. If the benefits of interacting with the state are few, the strategy of avoiding direct engagement with the state to evade taxes will not be altered by a reduction in bureaucratic hurdles. However, this view overlooks how individuals might benefit from engaging with the state. Completing a bureaucratic process, such as registering a firm, acquiring a title, or claiming access to utilities, enhances citizens' visibility –or legibility– to the state (Scott 1998; Lee and Zhang 2017), a feature that can be advantageous in some contexts. For example, individuals actively seek to make their property legible to the state, recognizing that a title can provide legal recourse in disputes (Bowles 2023; Ferree et al. 2023; Sánchez-Talanquer 2020). More puzzling examples include squatters who violate property laws but pay taxes to compel the state to recognize their occupation (Martin 2014), slum residents who pay water bills to claim residence in a settlement (Anand 2017), and unlicensed street vendors who pay taxes or market fees to access essential public services, such as waste removal or electricity (Holland and Hummel 2022). These examples suggest that people might be willing to accept the taxes and fees associated with engaging with the state. However, it is challenging to generalize from existing scholarship because most studies have focused on one type of activity at a time. Moreover, except for small-firm registrations, few studies on citizen-state interactions use experimental research designs, with notable exceptions, such as Henn, Paler, Prichard, Samii, and Sánchez de la Sierra (2026).

We designed six RCTs to equip citizens to engage with a bureaucracy across three types of policy domains: property titling, small business registration, and formal access to utilities (i.e., buying water from registered government vendors) and services (public garbage removal). In what follows, we will use formalization processes as a shorthand for these bureaucratic processes. The interventions used in-person visits

where subjects received information and personalized assistance to navigate the relevant bureaucratic process. The information included procedural guidance, as well as information about upfront payments and relevant taxes or fees. In addition, the intervention included information on the potential benefits of completing the process, emphasizing publicly-derived benefits. The assistance component eliminated hurdles by helping people understand the process, fill in the relevant forms, identify relevant documentation, and locate where to submit the paperwork. We estimate the effects of the interventions on individuals' intention to undertake the process, whether they completed it, and, ultimately, their payment of the relevant taxes and access to services.

Our meta-analysis of our RCTs offers little support for the premise that equipping citizens with information and assistance to engage with the state leads to successful completion of bureaucratic processes, increased tax payments or more access to services. Even when the initial frictions to interact with a bureaucracy become negligible, households and small business owners continue their informal activities. These null findings are consistent with empirical work that finds that facilitating the registration of small firms is generally ineffective (Bruhn and McKenzie 2014; Floridi, Demena, and Wagner 2020; Ulyssea 2020). We contribute to this strand of literature with additional evidence of the null effect of reducing hurdles to registration among informal businesses and evidence showing that this extends to other domains like land registration and public utility services.

Despite the null findings on the completion of processes and tax payments, our results suggest that lowering the costs to deal with a bureaucracy increases the reported intention to engage with the state. However, the meta-analysis estimate is not precisely estimated, and we find significant variation in the impact of the intervention between studies. This variation challenges the view that people generally prefer to keep their transactions undetected by the state (Brambor et al. 2020; Lee and Zhang 2017; Scott 2009).

We find that in contexts where the benefits of engaging with the bureaucracy are individual (for example, access to social security, property titles, and water connections), the intervention had a positive and significant effect on people's intent to undertake the process. On the other hand, in contexts where the benefits were collective, we find no effects. These results are compatible with Ostrom's semi-

nal work, which offers reasons to expect individuals to free-ride and evade taxation when collective goods are in question (Ostrom 1990). This information is critical for policy-makers who consider it imperative to close the gap between the regulations and the actual ease of regulatory compliance, demonstrating that connecting compliance with individual benefits can persuade citizens to become legible to the state, even in contexts where intermediaries have a strong presence, trust in government is low, and perceptions of corruption abound.

Our findings also provide deeper insights into a substantial source of the discrepancy between regulations and the actual ease of compliance. Policy-makers agree that governments must simplify their processes and avoid redundant and unnecessary steps. Why are efficiency gains so difficult to attain? Our RCTs underscore the lesson that to make progress in answering the question, the state should not be considered as a unitary actor. In our RCTs focused on land titling and water service provision, local bureaucrats undermined individuals' efforts to comply with regulations despite the support of higher-level governments for titling policies and water connection campaigns. These patterns are consistent with previous work showing that bureaucracies in low-growth contexts have a history of producing arbitrary outcomes (Gupta 2012), adding burdensome steps in processes that are supposed to be inexpensive and straightforward (Bowles 2023), and demanding informal payments (Henn et al. 2026). Therefore, focusing on optimizing the processes in the books is not enough. To make progress in this area, the economic and political incentives of state agents down the chain of command must align with the government's goal of promoting citizen-state interactions that lead to more tax revenue or fees, and increase access to services or legal protections.

Our study follows the multi-site collaborative model in (Dunning et al. 2019).¹ Independent groups of researchers conducted RCTs in Brazil, Colombia, the Democratic Republic of the Congo (DRC), India, Nigeria, and Malawi. The researchers coordinated the research question, a common treatment arm intervention, and measurement of relevant variables from the outset.² The interventions are tailored to the

¹The six RCTs are part of the Metaketa Initiative, organized by the Evidence in Governance and Politics (EGAP) network, which promotes cumulative learning through collaboration among researchers to conduct multi-site RCTs.

²In addition, each RCT includes a study-specific arm, which allows researchers to compare the effectiveness of the common treatment arm.

bureaucratic processes under study. To increase transparency and avoid publication bias, the six studies and the meta-analysis have registered pre-analysis plans; in addition, data and replication code are publicly available, and third-party researchers reviewed all code and data to avoid errors. Following our pre-analysis plan, we also report a meta-analytical specification curve analysis in the supplementary materials, which shows that our results are robust across methodological and analytical specifications.

The rest of this article is organized as follows. Section 2 reviews the academic literature that informed our interventions. Section 3 includes information about the contexts of our interventions and describes the common treatment arm. Section 4 describes the internal validity of the experiments. Section 5 presents our results. Section 6 discusses different interpretations of our results. Finally, Section 7 concludes with the implications of our findings.

2 Literature review

Understanding why people engage in informal activities, keeping an “uncertain, undocumented, and irregular relationship to the state” (Gottlieb 2024), is a challenge that extends beyond particular policy domains. Unregistered businesses, workers without formal contracts, property owners without a title, people occupying land in informal settlements, and consumers of products and services offered by informal vendors, among many others, share several features: their activities are outside the state’s legal and regulatory frameworks, which makes them difficult to monitor and tax. On the other hand, they are excluded from relevant legal protections and state benefits or services. Encouraging compliance with rules and regulations could increase state revenue and access to protections and services (de Soto 1989; Perry et al. 2007). Hence, examining the barriers that people might face to engage with and access the state is essential.

Various strands of the literature suggest that people’s decision to undergo a formalization process depends on the upfront bureaucratic costs, as well as the downstream costs and benefits associated with the formal activity. Research in this area suggests that entry barriers and costly regulation matter more in people’s decisions to

be informal than the desire to evade taxes (Joshi, Prichard, and Heady 2014). However, studies reach different conclusions about how crucial it is to reduce individual-level constraints associated with upfront costs, such as lack of information, time, financial resources, and problems of capacity (illiteracy, limited skills). Moreover, the literature has focused more on the market-based benefits of formalization than on state-derived ones, leaving a gap in our understanding of the types of positive inducements that matter to small businesses and households in low- and middle-income countries, where mistrust of government and complex social and economic institutions prevail (Auerbach et al. 2018; Joshi, Prichard, and Heady 2014).

Previous research on small business registration has documented that system-wide reforms aimed at simplifying the process of registering a small firm and reducing the financial costs of business licenses are insufficient (Bruhn and McKenzie 2014; Floridi, Demena, and Wagner 2020). One explanation for the limited success of regulatory reforms is that, even after simplification, hurdles such as lack of information or the perception that the process is arduous remain (IFC 2013; World Bank 2025). Often, intended beneficiaries in the informal economy are not aware of simplification programs targeted at them, or do not understand the requirements of formalization (Joshi, Prichard, and Heady 2014).

A more recent wave of firm formalization studies evaluated randomized interventions that remove hurdles small business owners face in formalizing. Three review articles document mixed results, at best (Bruhn and McKenzie 2014; Ulyssea 2020; Floridi, Demena, and Wagner 2020). Interventions that address the lack of information seem not to promote formalization (Benhassine et al. 2018; De Giorgi and Rahman 2013). Combining information with an offer to reimburse registration costs does not work either (Andrade, Bruhn, and McKenzie 2014; Mel, McKenzie, and Woodruff 2013). However, interventions that bundle information with personalized face-to-face assistance to navigate the formalization process produced positive effects on the order of 2 to 16 percentage points (Benhassine et al. 2018; Campos, Goldstein, and McKenzie 2015; Galiani, Meléndez, and Ahumada 2017).³ That personalized as-

³A closer look at firm formalization studies that evaluated a bundle of information and in-person assistance offers additional support for the argument that face-to-face personalized interaction is essential. Benhassine et al. (2018) found that the bundled intervention increased firm registration by 9.6 percentage points (std error=2.3) in Benin. In contrast, a treatment arm in which subjects received information without personalized assistance produced null effects (Benhassine et al. 2018).

sistance works is consistent with social science research showing that removing hurdles, even seemingly minor ones, can be a powerful strategy for changing behavior (Blair, Littman, and Paluck 2019).⁴

Another interpretation of the limited effects of business formalization interventions is that many informal firms are too small to perceive the market benefits of formalization, including access to credit, government contracts, advertisement, marketing, and increased profits (Floridi, Demena, and Wagner 2021).⁵ If small firms expect downstream costs from formalization to outweigh benefits, eliminating entry costs will not sway them.

Nevertheless, this view overlooks the possibility that state-derived benefits, instead of offering market gains, might make formalization appealing. For example, subsidized social security might benefit small entrepreneurs (Besley and Persson 2009, 2013), even if the markets do not reward business registration. Unlicensed street vendors benefit from access to essential public services, such as waste removal or electricity (Holland and Hummel 2022). In these examples, formalization increases legibility, which entails financial costs (for example, social security contributions and market fees), but it also enables people to claim access to state resources. Despite its relevance, few RCTs have evaluated whether linking small firm formalization with state-derived benefits persuades small firms to register their business.⁶

In the land and property formalization literature, there is greater agreement that upfront costs constitute a central barrier. Financial costs and the time and effort

Campos et al. (2015) find that, while information and personalized assistance increased tax registration by 2 percentage points (std. error=1.2) in Malawi, an intervention that added group-based information sessions in a bank did not work (Campos, Goldstein, and McKenzie 2015). Similarly, the bundled intervention in Colombia increased firm registration by 5.5 percentage points (std. error=1.4). However, when the intervention delivered the same information and assistance in a group setting, registration rates did not increase (Galiani, Meléndez, and Ahumada 2017).

⁴Table S7 in the appendix includes more information about prior field experiments on firm registration.

⁵Also see: Benhassine et al. 2018; Demenet, Razafindrakoto, and Roubaud 2016; McCaig and Nanowski 2019; McKenzie and Seynabou Sakho 2010; Rand and Torm 2012.

⁶Experimental work, which focuses on the behavior of firms and citizens already on the tax rolls, offers insights into how public services could be incorporated into interventions to promote formalization. For example, connecting taxes to the National Health Service in the U.K. increases the timely payment of taxes (Hallsworth et al. 2017). The few experimental studies that evaluate the impact of random provision of public services on tax payments show positive results in Argentina (Carrillo, Castro, and Scartascini 2021) and Mexico (Gonzalez-Navarro and Quintana-Domeque 2015). However, we do not know if individuals operating in the informal sector respond similarly to individuals already on the tax rolls.

required to acquire a title dissuade people from undergoing an administrative process, which might linger (Manara and Regan 2025). Indeed, a few field experiments that randomly assign vouchers to cover title fees consistently find a latent demand for titles (Ali et al. 2016; Manara and Regan 2025).

If property owners are willing to register their assets, it must be that the downstream benefits of formalization outweigh the costs. Consistent with this expectation, a meta-analysis of land formalization policies shows substantial productivity and income gains from land formalization, although the effects are stronger in Latin America and Asia than in Africa (Lawry et al. 2017). However, the mechanism that connects formalization and productivity is tenure recognition, not increased access to credit markets (Lawry et al. 2017). More secure tenure strengthens investment incentives, leading to better market outcomes.

Registering land or property in state records makes the ownership more legible to the state and others, which can help people defend their property and assets (Ferree et al. 2023; Sánchez-Talanquer 2020). Along these lines, scholars have argued that people will be more willing to make their assets legible to the state and pay taxes when the state upholds the rule of law and protects property rights (Besley and Persson 2009, 2013; Levi 1989).⁷

Does this mean that formalization will not appeal to property owners in weak states or in cases where customary tenure arrangements exist? The literature is more divided on this question. Some studies suggest that property titles are valuable even in weak states because their legibility provides some legal recourse in case of a dispute. On the other hand, legibility can increase tenure recognition even when the state does not grant the property title, such as in some customary tenure arrangements (Ferree et al. 2023). Hence, it remains an open question whether state protection of property rights is a prerequisite for formalization to be appealing.

This review of the literature suggests that further research is necessary to understand the barriers to formalization fully. Few studies, if any, have systematically examined the effects of reducing upfront costs across policy domains. However, prior work builds (sometimes implicitly) on a cost-benefit framework, suggesting that the

⁷Some scholars have argued that incumbent governments may prefer not to invest in fiscal infrastructure that increases legibility because doing so decreases opportunities for forbearance (Christensen and Garfias 2021; Holland 2016, 2017), and taxation may be electorally costly (Christensen and Garfias 2021).

underpinnings of informality are similar across types of informality. Moreover, although, intuitively, downstream benefits should outweigh the costs for formalization to be appealing, there is much to be explored about the role of publicly-derived benefits and the types of inducements that might be appealing to people in contexts of rapid urbanization and weak governance (Joshi, Prichard, and Heady 2014).

3 Design

We designed our multi-site study to test the general claim that upfront transaction costs and unclear state-derived benefits are obstacles to formalization. The six studies were coordinated such that the bundled intervention in all cases included two core components: information and in-person assistance. On the other hand, each of our studies tailored certain aspects of the research design to the different bureaucratic processes under study. We included information about the processes and information about specific benefits derived from government services that could enhance the bundled treatment. The in-person assistance component aimed to reduce in a substantive way the barriers to complete the bureaucratic process. With this bundled intervention, we aimed to alter subjects' calculations of the costs and benefits of formalization.

Because our studies are not fully harmonized, the diversity in research designs complicates the interpretation of meta-analytical quantities (Slough and Tyson 2023). However, allowing for research design diversity may be preferable to full harmonization in some circumstances (Wilke and Samii 2025). In particular, when research aims to shed light on the effects of a treatment in contexts that have not been studied, diversity in research design is preferable because it improves learning about the component of treatment effects that is common in contexts (Wilke and Samii 2025). In our case, previous single-case experiments on small firms' formalization found that information and in-person assistance produce the desired effects, although the magnitude of the effects ranged from 2 to 16 percentage points. Building on these results, we aimed to test if this type of intervention produces similar effects for different types of bureaucratic procedures. Therefore, we opted for tailoring the research design to the site and type of informality.

3.1 Context of the experiments

We selected sites where governments had taken steps to make it easier for individuals to undergo a certain bureaucratic process. However, frictions and hurdles seemed to prevent people from doing so. Our RCTs took place in low-income neighborhoods in Rio de Janeiro (Brazil) and Bogotá (Colombia), public markets in Lagos (Nigeria), neighborhoods in Kananga (DRC) and Zomba (Malawi), and slums in Mumbai (India). Table 1 provides information about the samples in each site.

Critical elements of a fiscal-contract-like mindset are missing among respondents from these sites. As Table 2 shows, a majority of respondents in our control groups expressed more agreement with the statement that people “should only pay their taxes if they agree with the government and its policies” than “citizens should always pay their taxes.” Furthermore, most of them believed that the government would not respond to the needs of their communities if they experienced an adverse shock, mostly because the government would be unwilling to do so. The exception is Malawi, where most respondents report that their government lacks the resources to solve community problems. Furthermore, our respondents think that their government would steal or waste 15% (Brazil) to 82% (Colombia) of its budget. Hence, with some variation, most people in our studies do not think paying taxes is an obligation and are skeptical of their governments. In this way, these sites are pertinent but hard cases for efforts to facilitate formalization.

Governments in our sites have tried to simplify and reduce the upfront costs of various bureaucratic procedures. In Brazil, Colombia, and Nigeria, governments simplified the processes to register small businesses, reduced application fees, and, in some cases, reduced tax rates to encourage small firm formalization. In India, a landmark judgment of the Bombay High Court removed a significant barrier to increasing municipal water connections by decreeing that the government cannot deny access to the municipal water supply to any slum. In Malawi, the provincial government offered a tax forgiveness plan to encourage residents to pay property taxes. The provincial government of the DRC offered a simplified procedure and subsidized prices for land titles as part of our evaluation. Despite these efforts, most of the activities under study remain informal. At endline, in our control groups, business registration rates were 16% in Brazil, 10% in Colombia, and 20% in Nigeria;

Table 1: Description of the six samples

	Sample	Sampling Frame	Sample Size
Brazil	Informal micro-entrepreneurs below the retirement age with annual incomes below BR\$81,000.	Census of informal micro-entrepreneurs conducted by research team.	866
Colombia	Open-door businesses.	Enumeration of open-door businesses conducted by research team.	562
Nigeria	Vendors in public marketplaces (excludes mobile vendors and vendors who sell foodstuffs).	Market lists compiled by LIRS, informal sector tax stations, and other sources covering 37 Local Community Development Areas and 20 Local Government Areas in Lagos.	641
DRC	Households eligible to purchase a property title.	Neighborhoods in Kananga defined for a recent tax collection campaign using a satellite map of the city.	824
Malawi	Owner-occupied households (renters were excluded because they are not required to pay the city fee).	Enumeration of owner-occupied households in approx. 80 neighborhoods in Zomba city conducted by research team. In each neighborhood, 78 percent of owner-occupied households were selected into the sample.	1617
India	Clusters of 50 to 200 households.	Enumeration, conducted by research team, of cluster-slums that have been in continuous existence for at least three years, are located reasonably close (1 km) to a municipal water pipe, and have a majority of residents lacking a municipal water connection. Slums in which the implementing NGO had previously worked extensively were excluded from the sampling frame.	6854

Table 2: Citizens’ tax morale and perceptions of state capacity, governments’ resources, and officials’ wastefulness and corruption

	India	Brazil	Colombia	DRC	Nigeria	Malawi
Tax attitudes	-0.45 (1.89)	-0.14 (1.38)	0.04 (1.51)	1.14 (1.27)	0.18 (1.56)	-0.47 (1.83)
Perceptions of state capacity	0.17 (0.38)	0.11 (0.32)	0.12 (0.33)	0.26 (0.44)	0.45 (0.50)	0.3 (0.46)
Perceptions of government lacking resources	0.17 (0.38)	0.15 (0.36)	0.32 (0.47)	0.16 (0.37)	0.2 (0.4)	0.58 (0.50)
Perceptions of waste and corruption	0.62 (0.23)	0.15 (0.19)	0.82 (0.22)	0.63 (0.37)	0.48 (0.30)	0.52 (0.23)

Notes: **Tax attitudes** measures respondents’ willingness to pay taxes on a scale from -2 (very strong preference for the phrase “citizens should only pay their taxes if they agree with the government or its actions”) to 2 (very strong preference for “citizens should always pay their taxes even if they disagree with the government or its actions”). **Perceptions of state capacity** measures the percentage of respondents who report that they think their government would solve a hypothetical community-specific problem caused by an adverse weather shock. **Perceptions of lack of resources** is the percentage of respondents who report that they think the government would not solve a hypothetical community-specific problem because it lacks the resources, rather than the willingness, to solve it. **Perceptions of waste and corruption** is the percentage of the government budget respondents think is stolen or wasted by public officials. The table reports means, with standard deviations in parentheses.

3% of households in India had a municipal water connection, suggesting that most people get their water from private (and usually informal) vendors; 13% of residents paid their city fee in Malawi; and no property owners obtained a title to their land in the DRC.

According to the 2025 *Business Ready* report (World Bank 2025), even when regimes are simplified or legal barriers removed, entry costs continue to be a problem in our sites. Our preliminary fieldwork suggested that people lacked information about the requirements to register their firm, acquire a property title, or gain access to public services and utilities. In addition, people widely perceived bureaucratic processes to be onerous.

3.2 The interventions: commonalities and variations

The common treatment arm in the six RCTs was a bundled treatment designed to remove transaction-cost-type barriers that impede those who wish to register their firm, acquire a property title, or access public services from doing so. As a reminder, for presentation purposes, we refer to these bureaucratic processes as different types of formalization. The first component of the intervention was at least one in-person visit in which the individuals received information about (1) the process they needed to follow to formalize and (2) the costs and benefits of formalization. Second, during the visits, individuals were offered assistance navigating the formalization process.

Our interventions aimed to substantially reduce up front costs. To be sure, in some cases, the entry hurdles required more involved assistance than in other cases. At a minimum, the assistance component offered help in identifying the required documentation, filling out the forms, and locating the nearest office to submit the paperwork. Additionally, the intervention offered on-site help to eligible subjects to file their forms online in Brazil and India. Malawi’s intervention included information about a tax forgiveness plan, which could remove a significant obstacle to formally accessing public services after missing years of tax payments. In the DRC, the intervention offered on-site eligibility assessments and discounted rates for obtaining legal titles. In India, where the process was more complicated compared to the other sites, the intervention included assistance to form groups of 5-10 neighboring households who could apply jointly for a water connection. The intervention also helped with ancillary requirements, such as proof-of-residency certification, acquiring documents concerning the legal status of the slum, and obtaining approval of a licensed plumber. In all cases, interventions offered additional help via phone consultations or additional visits. Table 3 presents a summary of the interventions and provides the context-specific details for each study.

The benefits of formalization underscored in the interventions varied by site, although all interventions described benefits derived from government services or policies. The intervention in Brazil focused on the Individual Microentrepreneur Program (MEI, in Portuguese), which gives microentrepreneurs subsidized access to social security. In Colombia, the intervention mentioned that only formal firms can

sell goods and services to government agencies.⁸ Nigeria’s intervention highlighted access to secondary education, as well as the possibility of applying for government loans. In the DRC, the intervention focused on the legal protections that come with property titles. Malawi’s intervention highlighted waste collection as one of the various public services that city rates fund. The intervention in India underscored the benefits of having access to more affordable and higher quality water compared to the water purchased from informal vendors.

On the costs of formalization, interventions offered information about registration fees, if applicable, and, in most cases, tax liability. At the time of the studies, there was no registration fee in Brazil, Malawi, or Nigeria. However, market vendors in Nigeria had to pay income taxes for the year to receive the e-TCC (formal electronic tax card), which amounts to approximately 2.08% of the monthly minimum wage. The monthly flat-rate fee contribution in Brazil, which combines a social security contribution and all state and municipal taxes, was equivalent to 5% of the monthly minimum wage. The city rate in Malawi was approximately 8% of the monthly minimum wage (the city rate could be higher depending on the household size). The fee for obtaining a business license in Colombia was approximately 7.4% of the monthly minimum wage, with an exemption for business owners under 35 years of age. Most small businesses in the Colombia study would fall under a simplified fiscal plan. They do not pay value-added taxes, and they pay a fraction of the income tax in flexible installments, plus labor and business taxes. The collective application fee in India was approximately 7 US dollars. The more substantive costs are the installation of pipes and water meters, for which applicants were responsible. Depending on the group’s size, these costs amounted to approximately 12% to 24% of the monthly minimum wage in Mumbai. The official prices for land titles in the DRC varied depending on their legal weight. The price for a *Certificat d’Enregistrement* was 1.6 to 3 times the monthly minimum wage, the *Contrat de Location* 1 to 1.6 times the monthly minimum wage, and the *Acte de Vente Notarié* 0.5 to 0.8 times the monthly minimum wage. The property tax was 6.6% of the monthly minimum wage salary for most property owners in the sample.

The research teams worked with local organizations or local governments to de-

⁸The intervention also mentioned benefits such as registration of a unique name, access to cheaper credit, and lower risk of penalties for operating without a business license.

liver the treatment. Table 3 includes information about implementing partners in each site. In all cases, nongovernmental organization (NGO) workers, consultants, and government officials had experience working in the respective sites. In Nigeria, where multiple languages are used, the NGO workers offered the information in the vendor’s native language. In the DRC, independent enumerators accompanied government surveyors during their home visits to make sure that surveyors followed the study’s protocol. Most research teams used printed materials to reinforce the interventions’ message. In Brazil, the consultants also used a 5-minute video delivered through a tablet. Visits tended to last 10 to 40 minutes, depending on the subject’s interest.

While all interventions followed the coordinated core components of the common treatment arm, additional modifications helped teams adapt their design to local conditions. For example, as part of the experimental design in Malawi, the Zomba City Council provided all residents in the study at baseline with waste collection by placing dumpsters in areas that were generally accessible to the residents and collecting waste from those dumpsters. The aim was to demonstrate to skeptical residents that the government could provide reliable waste collection. Instead of a control group that received no intervention in Nigeria, the experiment included a placebo intervention conducted among the control group. The reason for this modification was a low re-contact rate between the baseline survey and treatment delivery.⁹ Finally, treatment was clustered by slums in India. The rest of the sites randomized the intervention at the business or household level.

The sample size of the studies varies from 562 small businesses in Colombia to 6,854 households in India. Pooling all sites, the overall sample size is 11,364 households/small businesses. Table 1 contains information about each study’s sampling frame and sample.

3.3 Ethical considerations

Careful consideration of ethics informed the design and implementation of each of the six studies. Risks of harm to participants were evaluated on a site-by-site basis and

⁹In the placebo intervention, enumerators provided information on recognizing and dealing with stress.

Table 3: Description of the six coordinated RCTs

Brazil	Colombia	DRC	Malawi	Nigeria	India
Type of formalization:					
Firm registration	Firm registration	Property title	Payment of city rates (property taxes)	Market vendors registration	Formal access to public service (tubed water)
Location:					
Rio de Janeiro	Soacha	Kananga	Zomba	Lagos	Mumbai
Implementing agency:					
Research team	Local NGO	Provincial government	Zomba City government	Centre for Public Policy Alternatives (non-partisan think tank)	YUVA and Pani Haq Samiti (local NGO)
Common treatment arm-Information about process:					
Consulting session about MEI program	Consulting session about firm registration	Consulting session, and on-site appraisal	Consulting session about the process	Consulting session about e-TCC	Consulting session about the process
Common treatment arm- Information about costs and benefits:					
yes	yes	yes	yes	yes	yes
Common treatment arm- Subsidies/assistance:					
Offer to formalize on site	Assistance to complete paperwork	Assistance to complete paperwork/ discounted rates for obtaining legal title	Information about where to pay and about the tax forgiveness plan	Offer of assistance to fill in paperwork	Assistance navigating the bureaucratic process
Pure control condition:					
yes	yes	yes	yes	no (placebo group)	yes
Unit of randomization:					
Individual	Individual	Household	Household	Individual	Slum clusters
Method of randomization:					
Randomization done remotely by research team					
Clustered randomization:					
no	no	no	no	no	yes
Block randomization:					
yes	yes	no	yes	yes	yes
Time between intervention and measurement of outcomes:					
intent to formalize					
7-8 months	<2 months	>1 month	1 month	NA	13 months
formalization					
1 year	2-3.5 months	30 months	1 month	4 months	13 months
tax payment:					
2 months after formalization	2-3.5 months	11 months after title was received	9-12 months	4 months	13 months

deemed minimal. Although in some cases formalization required participants to pay fees, our interventions were designed to lower transaction costs and help participants obtain public benefits. We considered the possibility that formalization would expose subjects to government officials, but we concluded instead that researchers' and NGO involvement would help participants obtain elusive benefits and reduce the risk of shake-downs by officials. The collection of data from participants was conducted with informed consent, with safeguards for personally identifying information. Finally, as to ethical concerns that our intervention might change political or economic outcomes in the host countries, the scale of the interventions was too small to affect appreciably the supply or distribution of public goods.

3.4 Outcomes

Formalization is a process that people can initiate, but it is possible (and in some cases likely) that the government could knowingly or unknowingly block the process. Therefore, first we measure whether subjects assigned to the common treatment arm had an intention to formalize. In the study in Brazil, drawing from administrative data, subjects who contacted the implementing partner or the field team at any point after treatment to ask for help or for more information about the MEI program (as well as subjects who formalized without assistance) are coded as having attempted to formalize. In Colombia, subjects who reported having plans to register their business, procured additional information, or visited the Chamber of Commerce, where the registration takes place, are coded as having attempted to formalize. Subjects are coded as having attempted to formalize if they submitted an official application for a water connection in India and, in Malawi, if they reported in the midline survey that they planned to pay their city rate. Finally, in the DRC study, subjects who scheduled and received a home visit from a government surveyor are coded as having attempted to formalize. Intent to formalize is a binary outcome.¹⁰

Formalization takes place when individuals complete the bureaucratic process. A firm is coded as formal if it is registered as MEI in Brazil, registered with the

¹⁰In Nigeria, an unfortunate error in the flow of the endline survey impeded the measurement of intent to formalize. Outcomes were only collected among respondents who had never heard of the e-TCC. Among that sample, there is no difference in intent to formalize between treatment and control.

Chamber of Commerce in Colombia, and registered in the tax system in Nigeria. The studies in Brazil and Colombia measure formalization with administrative data, while the study in Nigeria measures it with an endline survey. In India, respondents are considered formal if they have a water connection for which they are required to regularly pay fees to the Brihanmumbai Municipal Corporation. In Malawi, the study uses administrative and survey data to learn whether respondents paid the city rate in the tax period during which the service delivery in the study wards started. Finally, in the DRC, the formalization process is completed when a subject acquires a property title. Formalization is considered a binary outcome.

All studies measure whether subjects pay the most relevant tax after the interventions. In Brazil, the study measures from administrative records the payment of the flat-rate fee that combines a social security contribution and all industry sector taxes two months following formalization. In Colombia, the study measures whether a respondent paid the business tax. In the DRC, the study measures payments of property taxes. In Malawi, the study measures continued payment of the city rate (after the intervention and waste collection). In Nigeria, the study measures payment of personal income tax. In India, the study measures payment of water consumption fees. The last rows of Table 3 include information about the time between assignment to treatment and the measurement of outcomes, by study.

Our pre-analysis plan also included two additional sets of downstream outcomes that could have been affected by subjects' completing the process of formalization. One set includes subjects' access to public services. The other set includes tax morale, perceptions of tax compliance, and willingness to pay taxes. Since these are outcomes that would be affected later in the causal chain, we report the corresponding analysis in the SI file.

4 Integrity of the experiments

For each study, we evaluate the baseline covariate balance produced by randomization with an omnibus test using the Xbalance package in R (Hansen and Bowers 2008). Table S1 in the supplementary materials shows that in five of the studies we cannot reject the null of balance between treatment and control (placebo) groups. In India,

we narrowly reject the null of balance (p -value=0.04). To account for this imbalance, and following our pre-analysis plan, in Tables S4 and S5 we present our results with and without baseline covariates.¹¹

Regression is used to check whether treatment assignment affected the profile of subjects who completed the endline survey. We regressed attrition on treatment and block indicators, where $attrition_i = 1$ if the person did not participate in the endline survey, and $attrition_i = 0$ if she did. We do not find asymmetric attrition between treatment and control (p -value=0.463) in five of the six RCTs. Table S2 in the supplementary materials shows this test country by country. Only in Nigeria do we find evidence of asymmetric attrition. Following our pre-analysis plan, we estimate extreme value bounds for the Nigeria study, and the corresponding meta-analysis estimates, to account for the asymmetric attrition. Figures S1, S2, and S3 show that our meta-analysis results are unchanged.

Table S3 in the supplementary materials shows that in three of the six studies (Colombia, Nigeria, and the DRC) there were no deviations from the randomization protocol. That is, all subjects assigned to treatment were treated, and no subjects assigned to control were treated. In Brazil, 12% of the subjects assigned to treatment did not receive the treatment. In the Malawi study, according to the canvasser-reported compliance, 1% of the subjects assigned to treatment did not receive treatment.¹² In India, where treatment assignment was conducted at the cluster level, 4 of the 152 slum-clusters were assigned to treatment but failed to receive it.

4.1 Statistical Analysis

Our analysis follows closely our registered pre-analysis plan. We estimate the effect of the common treatment intervention by comparing subjects assigned to treatment with subjects assigned to control, irrespective of whether or not they received the treatment. That is, we calculate the intent-to-treat (ITT) effect. For each study, we estimate the ITT with an ordinary least squares regression of the following form:

¹¹For the study in India, we confirmed that the randomization procedure was performed in accordance with the pre-analysis plan. We found imbalance in one of the 13 baseline covariates we used in the omnibus test. Therefore, we attribute the imbalance to random chance.

¹²The study in Malawi also measured compliance as reported by subjects in the endline survey. Of subjects assigned to treatment, 52% reported that they did not recall the information campaign or brochure, whereas 10% of the subjects assigned to the control condition did.

$$Y_{i,b} = \alpha + \beta_1 Treatment_{i,b} + \theta_b + \epsilon_{i,b} \quad (1)$$

where i is the business (or household), b is the block (for the studies that use block randomization), and $Treatment$ is an indicator for random assignment to the treatment arm. We control for any strata θ used in the individual studies to perform block randomization, and we weight observations by the inverse probability of treatment.¹³ Robust standard errors are clustered at the level of randomization.

To aggregate the results from all studies, we use a pre-registered random effects model (Borenstein 2009). We report two-sided hypotheses tests. To control the false discovery rate, we adjust p -values using the Benjamini-Hochberg procedure within categories of outcomes as described in our pre-analysis plan.¹⁴

As indicated in our pre-analysis plan, in addition to our main specification, Table S5 reports models where we add baseline covariates to equation (1) to increase the precision of our statistical inferences. We use an adaptive LASSO procedure to identify prognostic covariates to be included in our specification (Cooper, Paluck, and Annan 2019). We also examine the robustness of our results to alternative model specifications that take into account study-specific analytical choices. Figures S2 to S4 present a type of specification curve for the meta-analysis results, while Figures S5 to S7 depict results across specifications by country. In each figure, the top panel depicts the estimated ITT effect with its confidence intervals, while the bottom panel describes the model specification associated with each point estimate.

5 Results

Our pre-registered meta-analysis shows that the common treatment arm led to a 9 percentage points (pp) increase in intent to formalize (s.e.= 6 pp), and a 3 pp increase in formalization (s.e.= 2 pp). Although these estimates are in the expected direction, the effects are neither large substantively nor statistically distinguishable from zero

¹³We follow Gerber and Green 2012 on how to analyze block randomized RCTs when the probability of treatment varies by block.

¹⁴Our pre-analysis plan considers the outcomes of intent to formalize and formalization as part of the same category. Tax payment and an index of willingness to pay taxes (reported in Table S6) are another category of outcomes.

at the 0.05 level. Given the weak average effects on formalization, it is unsurprising that the treatment and control groups are almost identical in terms of tax payment at endline, with a difference of less than 1 pp between them (s.e.= 1 pp). Table S6 shows consistently null results for subjects' willingness to pay taxes, for which we find a tightly estimated zero, attitudes towards taxation, and perceptions of tax evasion. In that same table, we report that the interventions did not increase access to public services in general; nor did we find an effect on access to the public service most related to the intervention.

Still, we find significant heterogeneity across sites for the outcomes of intent to formalize and formalization. As shown in the last column of Table 4, the p -values of the Q-statistic measuring heterogeneity across sites are significant at the 0.01 level for intent to formalize and formalization. For tax payments, we do not find significant heterogeneity (p -value=0.19).

Table 4: Meta-analysis of the effects of the interventions on intent to formalize, formalization, and tax payment

Outcome	Estimate	SE	p -value	p -value BH correction	Heterogeneity test (p -value)
Intent to Formalize	9.4	5.7	0.176	0.176	76.306 (0)
Formalization	2.9	1.8	0.166	0.176	22.4719 (0.0004)
Tax Payment	0.7	0.8	0.430	0.859	7.3238 (0.1977)

Notes: **Intent to formalize** measures whether subjects initiated the process of formalization or declared interest in initiating it. **Formalization** measures whether subjects finished the formalization process. **Tax payment** measures whether subjects paid the relevant tax or fee. The table shows the results from random effects models. Columns 2 and 3 present the estimates and Robust SE clustered at the randomization level (in percentage points). P -values in columns 3 and 4 are from two-tailed t -tests. Column 4 presents adjusted p -values after applying the Benjamini-Hochberg correction within categories of outcomes. As pre-specified, intent to formalize and formalization are in one category of outcomes. Tax payment and an index of willingness to pay taxes (reported in Table S6) are in another category.

The country-by-country analysis of the common treatment arm's effects is informative because it shows where the process of formalization breaks down at each site.

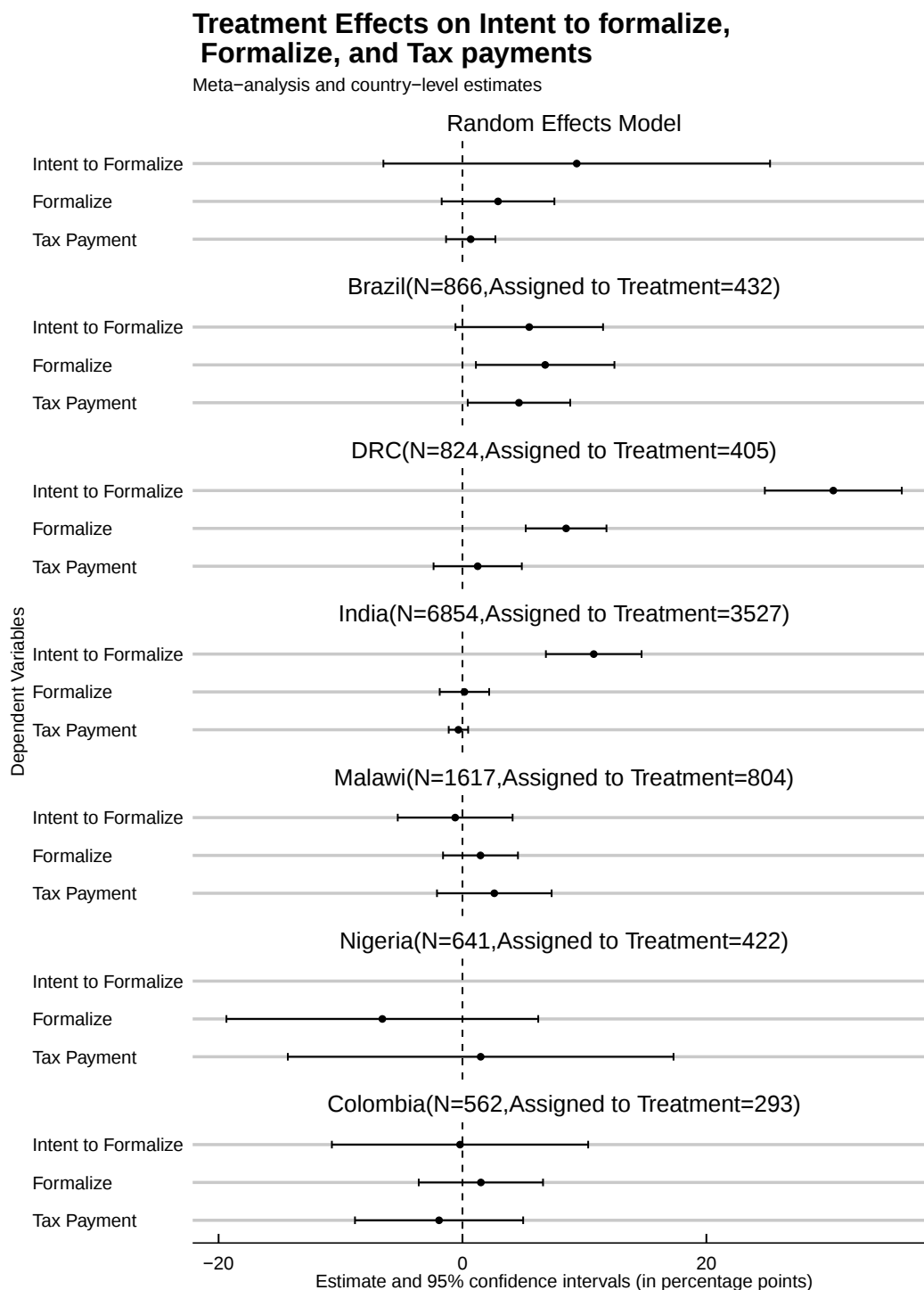
Figure 1 and Table S4 in the SI file report these results. For the outcome of whether individuals initiated the process or declared interest in formalizing, we find that the common treatment arm had null effects in Colombia (-0.2 pp, s.e.=5 pp) and Malawi (-0.6 pp, s.e.=2 pp). In Brazil, India, and the DRC, the common treatment arm increased subjects' intent to formalize by 5 pp (s.e.=3 pp), 11 pp (s.e.=2 pp), and 30 pp (s.e.=3 pp), respectively.

For the outcome of whether individuals finalize the formalization process, we find that the common treatment arm had weak and statistically insignificant effects in Colombia (1.5 pp, s.e.=2.5 pp), Malawi (1.4 pp, s.e.=1.6 pp), Nigeria (-7 pp, s.e.=6.5 pp), and India (0.16 pp, s.e.=1 pp). For Colombia and Malawi, this null effect makes sense, considering that the interventions did not increase subjects' willingness to formalize. However, in India, despite an intervention that successfully assisted subjects in applying and meeting the initial requirements of the formalization process, the treatment group was not more likely than the control group to gain access to water connections. Among the other two RCTs where there was a positive effect on intent to formalize, we find an increase in formalization in Brazil (7 pp, s.e.=3 pp) and in the DRC (8.5 pp, s.e.=1.7 pp). In the DRC, however, there is considerable slippage between individuals who seek to regularize their property and those who manage to complete the process and acquire a property title.

Since we find that in four of six studies (Colombia, Nigeria, Malawi, and India) people in the treatment group do not formalize at higher rates than the control group, it is not surprising that we do not find an increase in tax payments in these cases. In the two sites where we find a positive effect on formalization (the DRC and Brazil), we find an effect on tax payments only in Brazil (5 pp, s.e.=2 pp). In the DRC, we find that the common treatment arm scarcely increased tax payments (1.2 pp, s.e.=2 pp). In sum, there is little evidence that our interventions increased tax collection.

In our pre-analysis plan, we specified that in addition to the main specifications, we would report models with LASSO-selected covariates, which could increase the precision of our estimates. Table S5 in the Supplementary Material section shows that our results remain unchanged. Consistent with the results in Table 4, we find that the common treatment arm did not affect downstream outcomes, including subjects' access to public services, tax morale, perceptions of tax compliance, and

Figure 1: Effects of interventions on intent to formalize, formalization, and tax payment.



Notes: **Intent to formalize** measures whether subjects initiated the process of formalization or declared interest in initiating it. **Formalization** measures whether subjects finished the formalization process. **Tax payment** measures whether subjects paid the relevant tax or fee. The figure shows the results of a random effects model in the top panel, and from OLS models with block fixed effects and inverse probability weighting in the country-level estimates. Table S4 in the SI file reports the full set of results.

willingness to pay taxes. Table S6 reports these findings. Finally, Figures S2, S3, and S4 show the meta-analytical specification curve analysis, in which we find null effects across all models that covered combinations of methodological and analytical decisions.¹⁵

6 Discussion

Overall, the meta-analysis results show that equipping individuals with information and assistance to engage with the bureaucracy did not produce the desired effect of increasing formalization, tax payments, and access to services. The heterogeneity in the treatment effects for the outcomes of intent to formalize and formalization suggests that there are two paths to these null results, each with important theoretical and policy implications.

In one of the paths, the barriers to successful citizen-state interactions come from individuals' cost-benefit calculations. It is possible that the variation across our studies in the public benefits tied to formalization accounts for the interventions' varying effects. That said, we did not include in each RCT a between-subjects experiment to test the differential effects of various public services. Future research could build on our study to examine various dimensions of public benefits that seem to be relevant. The dimension we consider most relevant is whether benefits are individual versus collective. Prior work has argued that individual benefits generate more interest in formalization because they circumvent collective action problems (Besley and Persson 2013, Levi 1989). In line with these expectations, subsidized social security in Brazil, water connections in India, and property titles in the DRC generated interest from our subjects. In fact, the bundled intervention in the DRC also produced positive effects in areas governed by an alternative customary land system (Balán et al. 2022), suggesting that customary tenure systems are not a perfect substitute for formal property rights.¹⁶ In contrast, when governments offer

¹⁵We also report specification curves for site-by-site models in Figures S5, S6, and S7. These specification curves show that the only two models with divergent results are those in which we specify extreme value bounds for the study in Nigeria. As pre-registered, we include such models to deal with the systematic attrition in that RCT. Nevertheless, as figures S2, S3, and S4 show, the meta-analysis results are robust to specifying extreme value bounds in the Nigeria study.

¹⁶This result is at odds with prior work, which has argued that customary tenure systems are a

collective benefits, such as neighborhood trash collection in Malawi, subjects seemed unpersuaded. Future research could follow up on these insights to investigate the types of positive inducements that enable or undermine formalization policies in the Global South.

An alternative interpretation of the heterogeneity that is less compatible with our results is that contextual factors, such as individuals' perceptions of state capacity and government corruption, account for the varying effects on the intent to engage with the state. Suppose people were unwilling to make their transactions legible to governments that lack the capacity or willingness to solve community problems or that are characterized by widespread corruption. In that case, we should not have found an effect in India or Brazil, where most respondents report that their government is unwilling to solve community-specific problems, or in the DRC, where the average respondent reports that government officials would steal or waste more than 60% of its budget. Conversely, we should have found an effect in Nigeria, which has the highest percentage of respondents who report that their government would solve a community problem, or in Malawi, where the RCT included free waste collection to demonstrate that the government can indeed provide this service.

Another interpretation, for which we find little empirical support, is that differences between the treatment effects across our studies are an artifact of differences in how intent to formalize was measured in each study. In particular, one concern is that the two studies (Colombia and Malawi) in which we find null effects used self-reported data to measure intent to formalize. Although we acknowledge that respondents may inflate their intention to formalize due to social desirability bias, we do not see evidence that such measurement error was systematically related to treatment. For example, if subjects assigned to the treatment group were less truthful in their reports than people assigned to the control group, then we would have seen a substantial positive difference in outcomes instead of the effects reported in Table 4.

Another research design variation is whether government agents or NGOs delivered the treatment. We find that some government-led interventions promoted subjects' intention to formalize (as in the DRC study), but some failed to do so (as

substitute (Deininger and Feder 2001).

in the Malawi study). Similarly, we do not see a pattern of success or failure among NGO-led interventions.

Finally, one last research design variation to consider is that the costs of formalization are different across studies. Was it the case that the intervention increased subjects' intention to formalize only in the studies where the costs of formalization were lower? Interestingly, the answer to this question is no. Of the three studies where the intervention led to an increase in intention to formalize, two of them had the highest up front costs (Please see section 3.2 for more information about these costs).

Another path to the null results includes barriers originating from state agents. The slippage between individuals' intent to formalize and actual formalization sheds light on this point. Bureaucratic obstacles limited the effectiveness of the common treatment arm in two of our studies. In the DRC, despite initial support for the land titling campaign, relevant bureaucracies blocked the process in two ways. First, the land ministry restricted the eligibility criteria after the launch of the campaign. Second, the legal and cadastral offices were skeptical of the campaign because the simplified formalization process reduced their discretion at certain steps in the titling process.¹⁷ As a result, the intervention took nearly 15 months longer than expected.

In India, the intervention increased the number of inspections by water officials, which is an intermediate step in the process.¹⁸ But burdensome and sometimes *ad hoc* additional requirements impeded water connections. Follow-up interviews with NGO workers suggest that ward-level politicians, BMC engineers, and licensed plumbers obstructed the water connections, even though higher officials wanted to provide them. There are three possible reasons for the response from local bureaucratic and political agents. First, water connections entail more work for these agents. Second, connecting households to municipal water would reduce the market share of water mafias, which operate private commercial tanker businesses and sell water in slums at exorbitant rates (Björkman 2015). As a consequence of increased access to municipal water, local-level bureaucrats would lose the extra income they get from the water mafias, as one NGO worker explained (Gaikwad, Nellis, and Thomas 2022). Third,

¹⁷Less discretion meant that many “administrative fees” to various government officials in the titling and cadastral offices effectively went unpaid (Balán et al. 2022).

¹⁸See Gaikwad, Nellis, and Thomas 2022 for details on the analysis of the effects of the bundled intervention on the number of inspections by water officials.

local bureaucrats' biases against migrants might have made them reluctant to expand public services to settlement dwellers.

The bureaucratic interference observed in the India and the DRC sites is compatible with recent work showing that local governments may allow, and even benefit from, certain types of informalities (Holland 2016, 2017; Feierherd 2020). This suggests that governments need to overcome agency costs (Levi 1989) to align local bureaucrats' incentives to the government's objectives of increasing property registration and connection to municipal water in the DRC and India, respectively.

7 Conclusion

In many low- and middle-income countries, most transactions between people occur out of the state's sight, unable to benefit from its regulation and protections. Informality has been associated with weak fiscal and state capacity. Individuals are assumed to be unwilling to formalize because that would make their transactions legible to the state and taxable; states are assumed to be incapable of persuading people to shoulder the burden of taxation. We conducted six randomized controlled trials that aimed to reduce up front transaction costs and make salient the downstream state-derived benefits of different bureaucratic procedures. The cases included property titling, small firm registration, and payment for public utilities/services (instead of resorting to alternatives in the informal market). Meta-analysis of our six studies shows that, on average, the interventions did not significantly increase formalization, tax payments or access to services. The link at which the causal chain broke in our studies is informative. Three interventions moved subjects' intention to formalize. However, in two of these studies, state agents prevented subjects from completing the process. In the rest of the studies, subjects were not responsive to the interventions.

Our results help reconcile mixed findings across disciplines on whether reducing entry costs is essential and whether people might benefit from formalization. Although we find mixed results, we uncover a latent demand for individual benefits, such as subsidized social security, a property title, and water connections, which seem to make engaging with the state appealing despite the downstream costs associated with more legibility. Indeed, in each of the bureaucratic procedures we

examined, including the small firm registration, the interventions that successfully moved people's willingness to undertake the formalization process offered individual (rather than collective) benefits. Therefore, in contexts of weak governance and rapid urbanization, equipping citizens with information and assistance to engage with the bureaucracy and offering individual positive inducements may be necessary, although perhaps not cost-effective, to produce the desired effects.

Although it is encouraging that some interventions generated interest in formalization even where people perceive that public corruption is widespread and the government is indifferent to community needs, our studies reveal that barriers frequently originate from within the state. As we have seen, interventions may only produce the desired result if all layers of a bureaucracy are structured to facilitate the completion of bureaucratic processes rather than block them. Corruption and interest group politics within state bureaucracies may prevent them from implementing formalization policies, even when individuals are willing to cooperate. Hence, to close the gap between the regulations on the books and the ease of regulatory compliance, bureaucratic incentives must be taken into account.

Our multi-site study makes it clear that extrapolating results from one case to others can be misleading. Even among cases where consensus seemed to exist, such as informal small firms' reluctance to register with tax authorities and people's demand for property titles, we found nuanced results. The broader insight from our study is that *both* government-imposed entry costs and people's cost-benefit calculations contribute to the perpetuation of informal activities. In that sense, we place our findings within wider considerations about how the political economy of these countries constrains choices and public policies, as well as offering opportunities. Future research could extend beyond untitled properties and informal firms to explore how transaction costs and publicly derived benefits influence the various ways in which people's lives operate outside of the state's regulations and protections.

8 Competing interest information for all authors

All authors declare that they have no competing interests.

9 Data sharing plans

Replication files will be available on <http://osf.io>, upon publication.

10 Funding information

Funding for the coordinated studies and the meta-analysis was provided by UK Foreign, Commonwealth and Development Office (formerly the Department for International Development). Grant no. 205133.

11 Acknowledgements

We are grateful to Jaclyn Leaver for support of this research, and to Shikhar Singh for excellent research assistance. We thank Miram Bruhn, Cyrus Samii, Jasper Cooper, Tara Slough, James Robinson, Annette Brown, Pia Raffler, Fredrik Savje, Peter Aronow, Alexander Coppock, Daniel Nielson, Graeme Blair, Markus Taussig, Renard Sexton, Edmund Malesky, Jeremy Weinstein, Lucy Martin, and Rebecca Wolfe for useful feedback.

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12 Supplementary Materials

12.1 Additional Tables

Table S1. Omnibus test of balance in baseline characteristics by country

Table S2. Test of asymmetric attrition in survey data by country

Table S3. Compliance with treatment assignment by country

Table S4. The effect of the common treatment arm on intent to formalize, formalization, and tax payment

Table S5. The effect of the common treatment arm on intent to formalize, formalization, and tax payment (covariate adjustment using LASSO)

Table S6. Random effects meta-analysis of interventions on downstream outcomes

Table S7. Summary of prior experimental studies

12.2 Additional Figures

Figure S1. Distribution of Primary Outcomes

Figure S2. Robustness of findings across specifications: intent to formalize (meta-analysis)

Figure S3. Robustness of findings across specifications: formalization (meta-analysis)

Figure S4. Robustness of findings across specifications: tax payment (meta-analysis)

Figure S5. Robustness of findings across specifications: intent to formalize (country-by-country)

Figure S6. Robustness of findings across specifications: formalization (country-by-country)

Figure S7. Robustness of findings across specifications: tax payment (country-by-country)

12.3 Pre-analysis plan

Table S1: Omnibus test of balance in baseline characteristics by country

Country	Chi-square	p-value
Brazil	29.52	0.39
Colombia	4.47	0.61
DRC	27.49	0.12
India	22.79	0.04
Malawi	16.76	0.67
Nigeria	21.39	0.56

Notes: This table presents the Chi-square and associated p-value from omnibus tests using Hansen and Bowers's (2008) xbalance package in R.

Table S2: Test of asymmetric attrition in survey data by country

	India	Brazil	Colombia	DRC	Nigeria	Malawi
Treatment	−0.0013 (0.0176)	0.0221 (0.0300)	0.0409 (0.0411)	−0.0375 (0.0372)	0.0960* (0.0459)	−0.0043 (0.0155)
R ²	0.0848	0.0215	0.0409	0.5340	0.6267	0.0609
Adj. R ²	0.0778	0.0018	0.0110	0.3077	0.0586	0.0107
Num. obs.	6854	866	562	824	639	1617
RMSE	0.4398	0.4409	0.6829	0.4111	0.3942	0.4414

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Notes: **Attrition** refers to respondents who were interviewed at baseline, but not at endline. **Treatment** takes the value of one for subjects assigned to treatment, and zero for assignment to control.

Table S3: Compliance with treatment assignment by country

Country	Z=0 and untreated	Z=1 but untreated	Z=1 and treated	Z=0 but treated	Total
Brazil	434	52	380	0	866
Colombia	269	0	293	0	562
DRC	419	0	405	0	824
Malawi	813	9	795	0	1617
Nigeria	219	0	422	0	641
India (cluster level)	75	4	73	0	152

Notes: **Z** refers to assignment to experimental conditions. Z=1 indicates assignment to treatment, and Z=0 indicates assignment to control.

Table S4: The effect of the common treatment arm on intent to formalize, formalization, and tax payment (in percentage points)

	Intent to formalize					
	India	Brazil	Colombia	DRC	Nigeria	Malawi
Treatment	10.7638*** (1.9850)	5.4755 (3.0824)	-0.2009 (5.3429)	30.4099*** (2.8617)		-0.5992 (2.4007)
R ²	0.0711	0.0554	0.0966	0.5188		0.2870
Adj. R ²	0.0612	0.0331	0.0495	0.2851		0.2398
Num. obs.	4802	738	344	824		1322
RMSE	44.0028	41.8499	68.8983	32.4765		61.7097

	Formalization					
	India	Brazil	Colombia	DRC	Nigeria	Malawi
Treatment	0.1604 (1.0265)	6.7814* (2.8928)	1.5089 (2.5942)	8.4980*** (1.6869)	-6.5714 (6.5068)	1.4770 (1.5661)
R ²	0.0848	0.0511	0.0332	0.3882	0.6836	0.3094
Adj. R ²	0.0750	0.0287	0.0029	0.0912	0.0348	0.2673
Num. obs.	4802	738	562	824	480	1428
RMSE	18.7505	39.3879	43.3224	20.7417	39.0286	41.4513

	Tax payment					
	India	Brazil	Colombia	DRC	Nigeria	Malawi
Treatment	-0.3325 (0.4031)	4.6318 (2.1407)	-1.9193 (3.5061)	1.2473 (1.8437)	1.4925 (8.0472)	2.6111 (2.3963)
R ²	0.0291	0.0330	0.0895	0.3235	0.7275	0.2993
Adj. R ²	0.0186	0.0082	0.0419	-0.0049	0.1372	0.2523
Num. obs.	4802	681	343	824	479	1306
RMSE	12.0402	27.8196	46.0244	21.8111	45.1787	60.8510

Notes: **Intent to formalize** measures whether subjects initiated the process of formalization or declared interest in initiating it. The Nigeria RCT does not have a measure of intent to formalize because of an administrative error in the flow of the endline survey. **Formalization** measures whether subjects finished the formalization process. **Tax payment** measures whether subjects paid the relevant tax or fee. The table shows the results from OLS models with block fixed effects and inverse probability weighting. P-values are from two-tailed t-tests, after applying the Benjamini-Hochberg correction. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Table S5: The effect of the common treatment arm on intent to formalize, formalization, and tax payment (covariate adjustment using LASSO)

	Intent to formalize				
	India	Brazil	DRC	Nigeria	Malawi
Treatment	10.5463*** (1.9172)	4.7648* (2.3517)	29.6349*** (2.8858)		−1.0068 (2.2718)
R ²	0.0849	0.4527	0.5234		0.3806
Adj. R ²	0.0735	0.4343	0.2906		0.3304
Num. obs.	4802	738	824		1322
RMSE	43.7122	32.0128	32.3499		57.9169

	Formalization				
	India	Brazil	DRC	Nigeria	Malawi
Treatment	−0.0427 (0.9889)	6.2144* (2.5710)	7.0167*** (1.6072)	−6.5714 (6.5068)	1.3638 (1.1964)
R ²	0.0922	0.2601	0.4161	0.6836	0.6011
Adj. R ²	0.0803	0.2363	0.1279	0.0348	0.5724
Num. obs.	4802	738	824	480	1428
RMSE	18.6963	34.9261	20.3182	39.0286	31.6661

	Tax payment				
	India	Brazil	DRC	Nigeria	Malawi
Treatment	−0.3607 (0.4048)	4.4565* (2.1169)	1.2473 (1.8437)	1.6999 (8.0410)	2.3895 (2.2940)
R ²	0.0307	0.0590	0.3235	0.7324	0.3778
Adj. R ²	0.0192	0.0304	−0.0049	0.1414	0.3250
Num. obs.	4802	681	824	479	1306
RMSE	12.0365	27.5053	21.8111	45.0695	57.8180

Notes: Please see notes in Table S4. We do not include Colombia in this table because the study did not collect a baseline survey. Therefore, we do not have a large set of baseline covariates to apply the LASSO selection method. Unadjusted p-values are from two-tailed t-tests. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Table S6: Random effects meta-analysis of downstream outcomes

Outcome	Countries	Estimate	SE	P value
Closest public service	All	1.499	1.853	0.712
Access to public services	All	-0.018	0.046	0.712
Tax morale	All	0.072	0.053	0.233
Perceptions of tax non-compliance	All	-0.021	0.022	0.395
Willingness to pay taxes	All	0.001	0.008	0.925

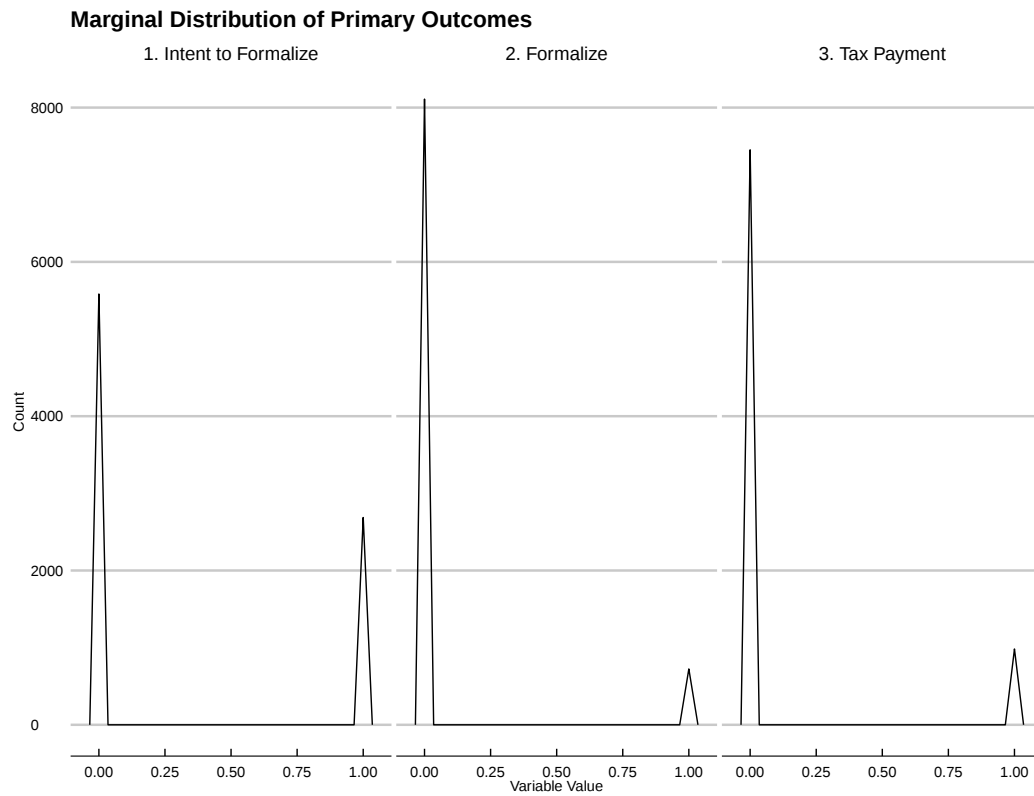
Notes: **Closest public service** refers to access to a public pension in Brazil, access to a public pension and healthcare in Colombia, access to a primary school (within a 15-minute walk) in Nigeria, access to tubed water in India, self-reported use of government-provided dumpsters as the method of waste disposal in Malawi, and perceptions of legal certainty over land ownership in DRC. **Access to public services** is an index that summarizes access to a series of public services. **Tax morale** measures agreement or disagreement with the phrase: “Citizens should only pay their taxes if they agree with the government or its actions.” **Perceptions of tax non-compliance** is an index composed from three measures: perceptions about tax compliance in respondent’s region, perceptions about likelihood of being caught when evading taxes, and perceptions about likelihood of being punished for tax evasion. **Willingness to pay taxes** is an index that takes higher values when respondents are willing to pay more taxes. This index is constructed from a battery of questions that includes a series of taxes relevant for each site. The table reports results from random effects models. P-values are from two-tailed t-tests, after applying the Benjamini-Hochberg correction within categories of outcomes, as described in the pre-analysis plan.

Table S7: Summary of prior experimental studies on small business registration

Set	Study	Intervention	Setting	Time between intervention and outcome	Findings in percentage points (std. error)
Set 1	De Giorgi and Rahman 2013	Information	Bangladesh	1 year	-0.6 (0.7)
	Benhassine et al. 2018	Information	Benin	2 years	1 (0.6)
	Andrade et al. 2016	Information	Brazil	1 year	-1.2 (1.7)
Set 2	de Mel et al. 2013	Information + reimbursement of registration costs	Sri Lanka	1 month	-0.9 (1.6)
	Andrade et al. 2016	Information + reimbursement of registration costs	Brazil	1 year	-2.8 (1.5)
Set 3	Campos et al. 2015	information + assistance in person and in a group setting	Malawi	4 months	1.6 (1.1)
	Galiani et al. 2017	information + assistance in a group setting	Colombia	1 year	0.3 (0.9)
Set 4	Campos et al. 2015	information + in-person assistance	Malawi	4 months	2 (1.2)
	Galiani et al. 2017	information + in-person assistance	Colombia	1 year	5.5 (1.4)
	Benhassine et al. 2018	information + in-person assistance	Benin	2 years	9.6 (2.3)
	Benhassine et al. 2018	information + in-person assistance + help to open a bank account	Benin	2 years	13 (1.4)
	Benhassine et al. 2018	information + in-person assistance + help to open a bank account + help to prepare tax returns	Benin	2 years	16.3 (1.3)
Set 5	de Mel et al. 2013	information + cash transfer (aprox. half a month's profit)	Sri Lanka	1 month	13 (3.8)
	de Mel et al. 2013	information + cash transfer (aprox. one month's profit)	Sri Lanka	1 month	10 (3.5)
	de Mel et al. 2013	information + cash transfer (aprox. two month's profit)	Sri Lanka	1 month	27 (4.7)

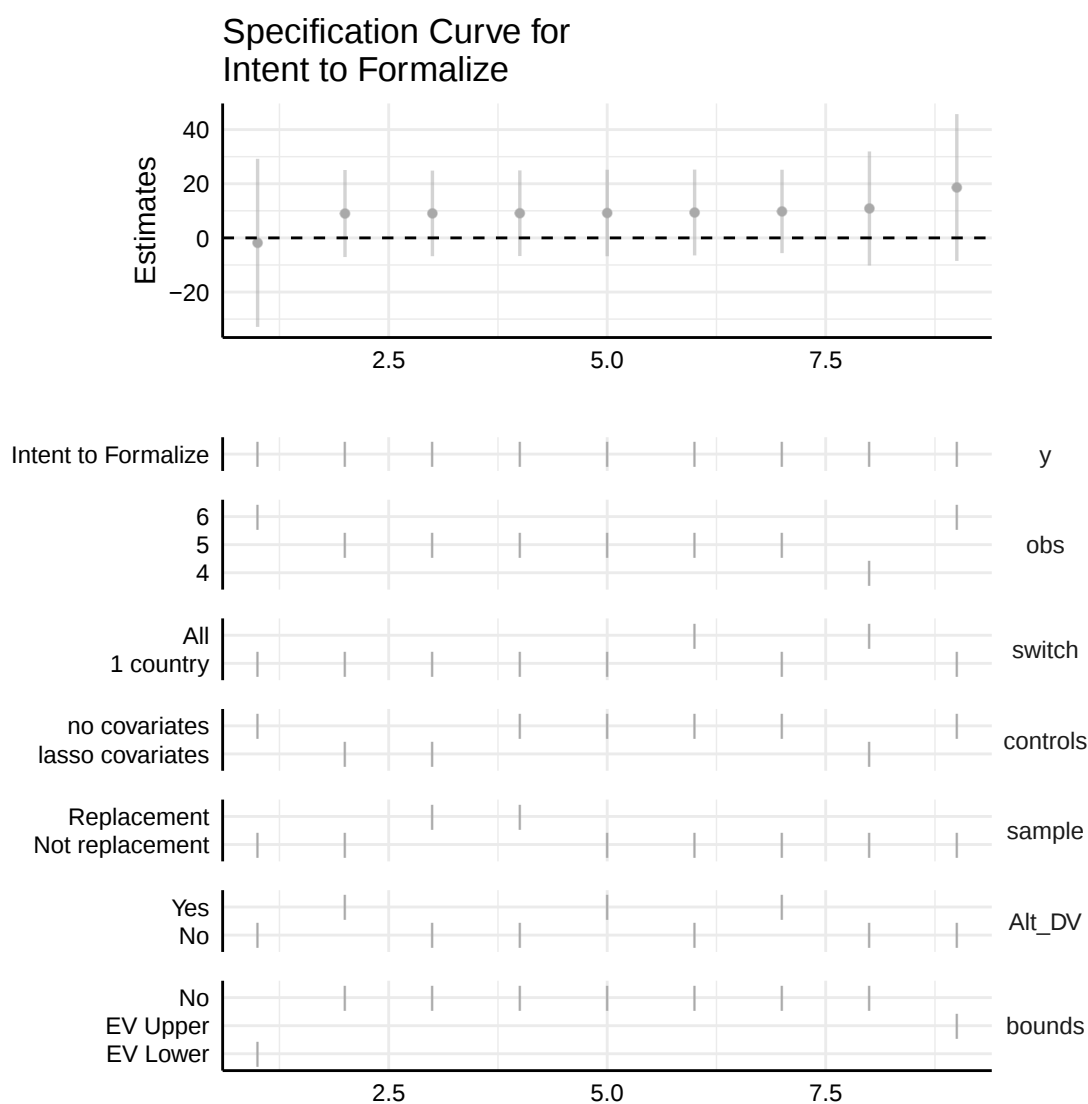
Notes: Benhassine et al. (2018) also report effects by number of months since intervention. The magnitude of the effect seems constant after 3-4 months, approximately. In de Mel et al (2013), firms had additional time to register (one month). In Galiani et al. (2017), the effects disappear two years after the intervention.

Figure S1. Distribution of Primary Outcomes



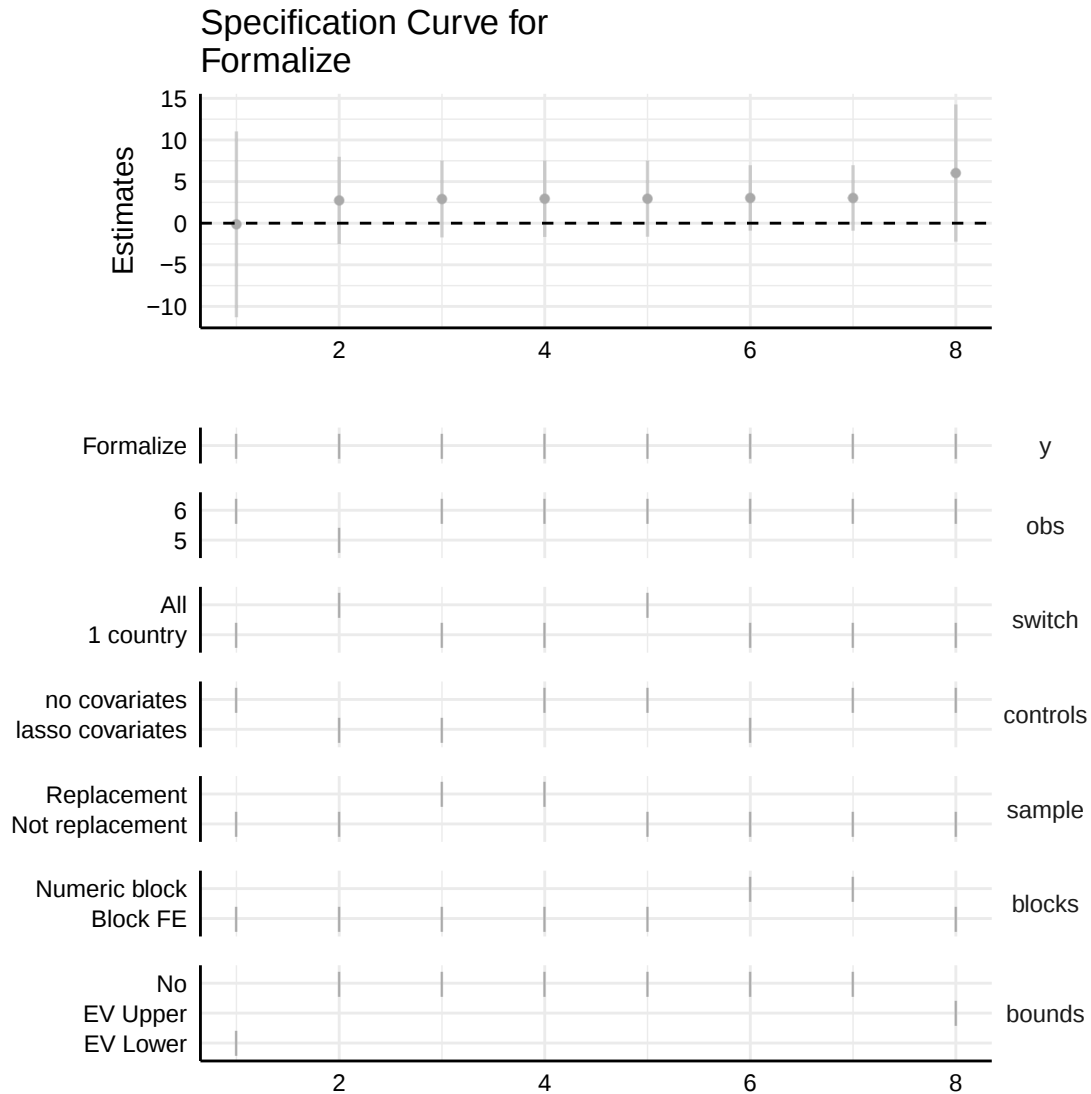
Notes: This figure presents the marginal distribution of the three primary outcomes: intent to formalize, formalization and tax payment. These three outcomes are binary.

Figure S2. Robustness of findings across specifications: intent to formalize (meta-analysis)



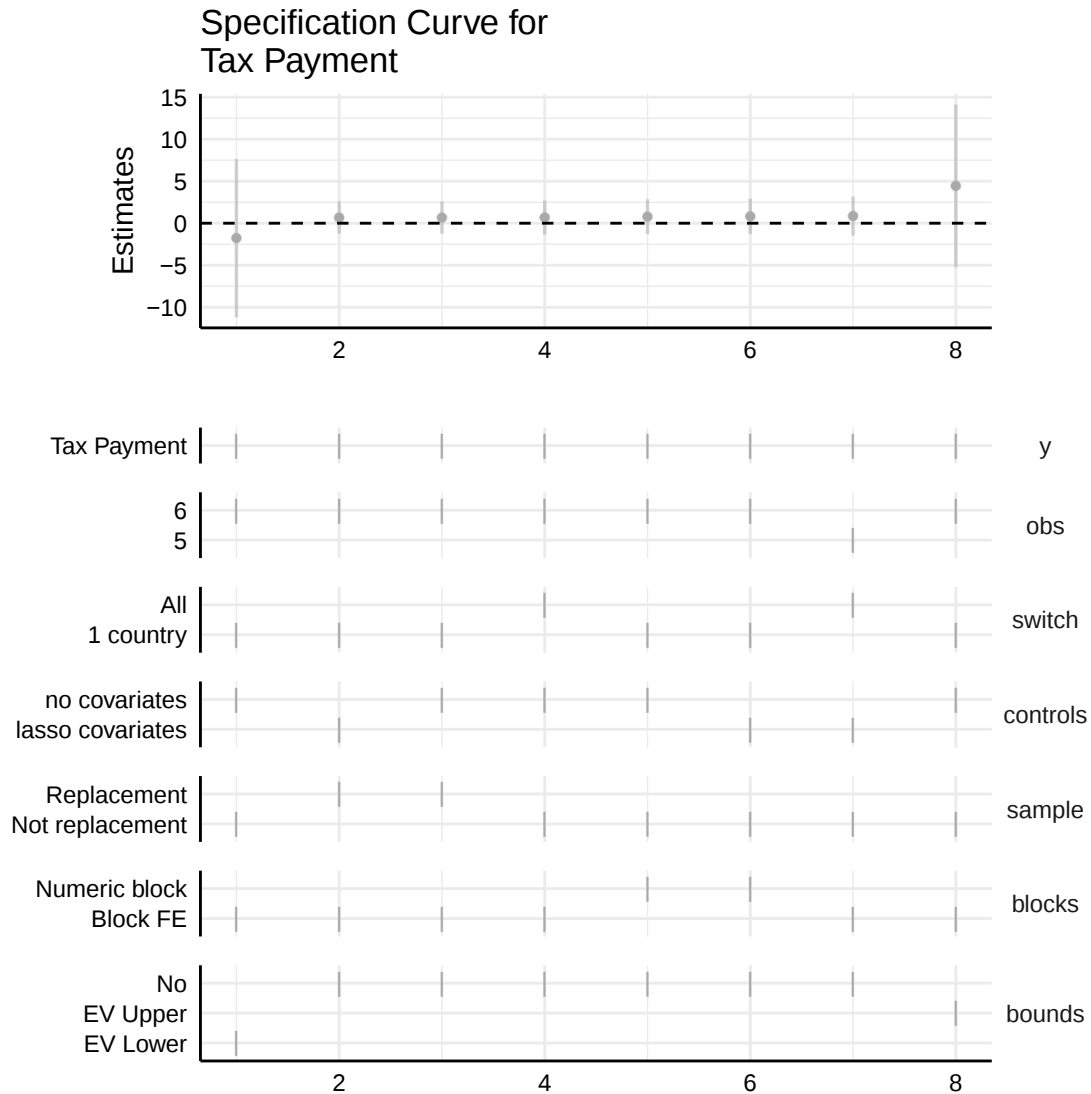
Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated with each point estimate. **Obs** refers to the number of studies included in the meta-analysis. **Switch** indicates whether the specification applies to all studies or one study. **Controls** refers to the specifications with and without baseline covariates. **Sample** refers to a robustness test that involves the study in India. Within experimental clusters, the team surveyed households included in the baseline survey and a replacement sample of households not in the baseline. The main analysis does not include the replacement sample, but as seen here including it in the analysis does not change our results. **Alt_DV** refers to alternative definitions of the dependent variable. **Bounds** refers to a robustness test that involves the study in Nigeria, for which we cannot reject that attrition was systematically related to treatment. Together with the main analysis, we present extreme value bounds.

Figure S3. Robustness of findings across specifications: formalization (meta-analysis)



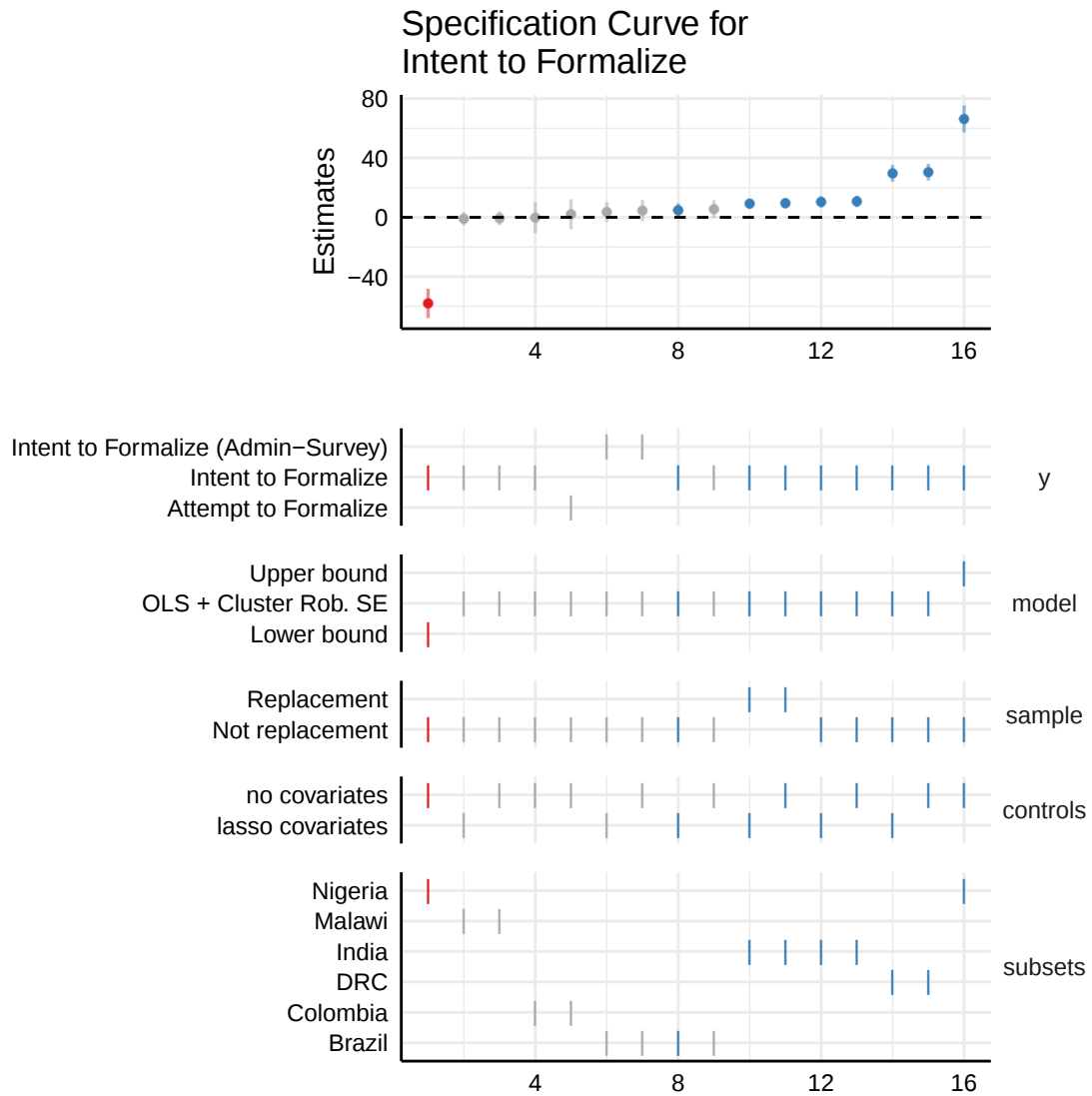
Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated with each point estimate. **Obs** refers to the number of studies included in the meta-analysis. **Switch** indicates whether the specification applies to all studies or one study. **Controls** refers to the specifications with and without baseline covariates. **Sample** refers to a robustness test that involves the study in India. Within experimental clusters, the team surveyed households included in the baseline survey and a replacement sample of households not in the baseline. The main analysis does not include the replacement sample, but as seen here including it in the analysis does not change our results. **Blocks** and **Bounds** refer to robustness tests that involve the study in Nigeria. **Blocks** present the specifications using binary or continuous variables to account for blocks. **Bounds** present extreme value bounds to account for systematic attrition.

Figure S4. Robustness of findings across specifications: tax payment (meta-analysis)



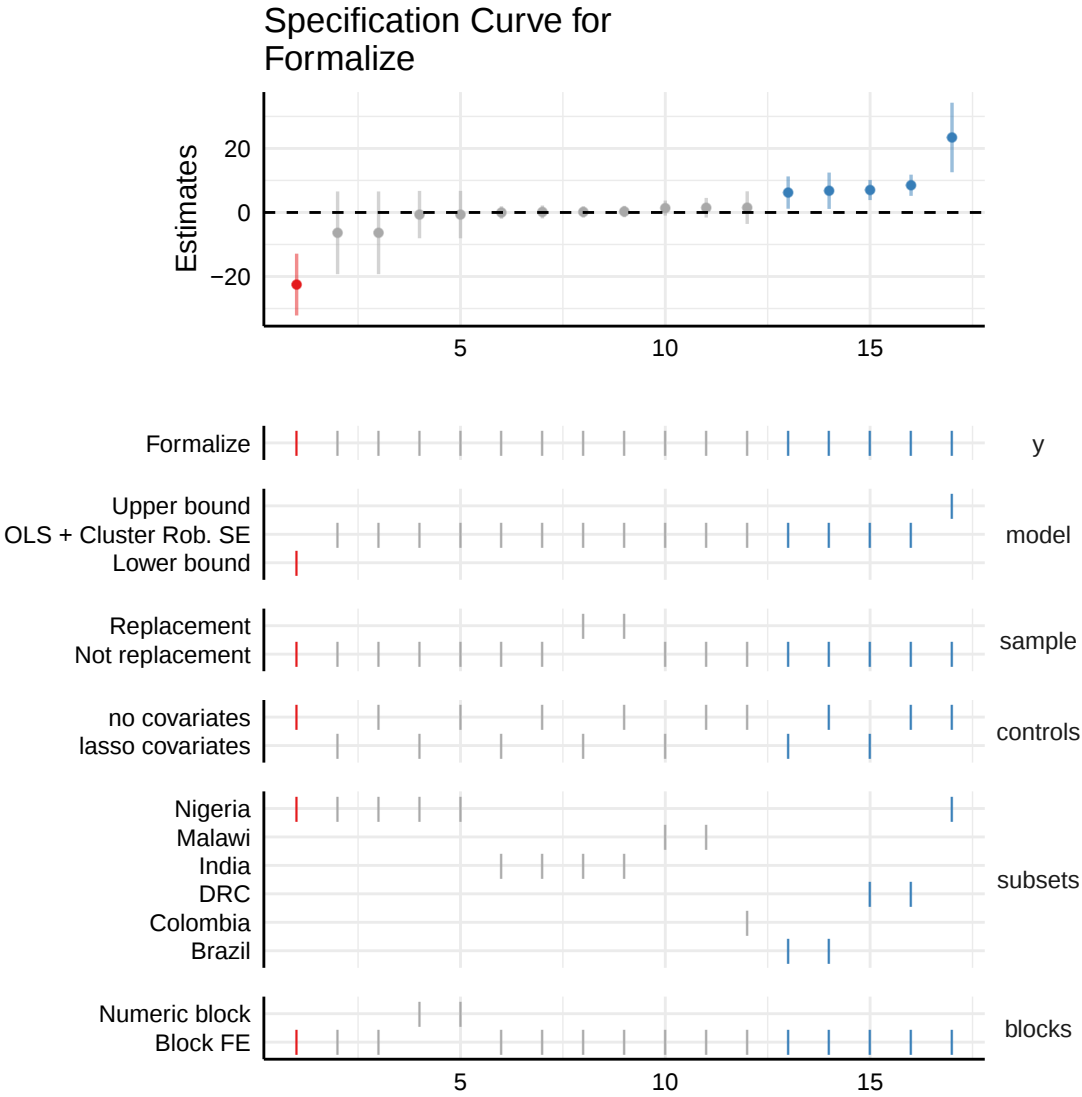
Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated with each point estimate. **Obs** refers to the number of studies included in the meta-analysis. **Switch** indicates whether the specification applies to all studies or one study. **Controls** refers to the specifications with and without baseline covariates. **Sample** refers to a robustness test that involves the study in India. Within experimental clusters, the team surveyed households included in the baseline survey and a replacement sample of households not in the baseline. The main analysis does not include the replacement sample, but as seen here including it in the analysis does not change our results. **Blocks** and **Bounds** refer to robustness tests that involve the study in Nigeria. **Blocks** present the specifications using binary or continuous variables to account for blocks. **Bounds** present extreme value bounds to account for systematic attrition.

Figure S5. Robustness of findings across specifications: intent to formalize (site-by-site)



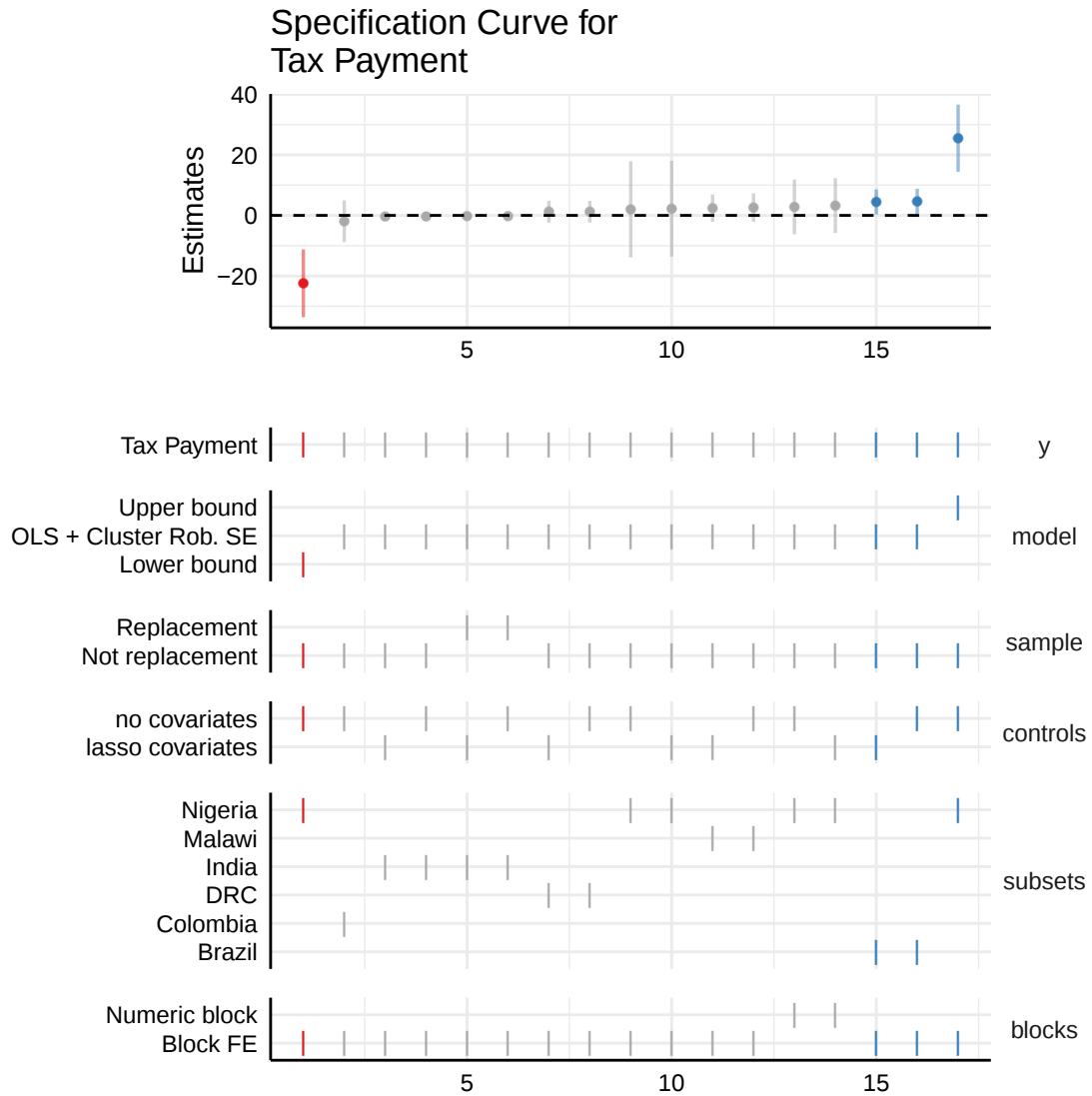
Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated with each point estimate. **Y** refers to alternative definitions of the dependent variable. **Model** presents the main specification and the extreme value bounds for the study in Nigeria, where there is evidence of systematic attrition. **Sample** refers to specifications with and without the replacement sample in India. **Controls** refers to the specifications with and without baseline covariates. **Subsets** indicates the study associated with the specification.

Figure S6. Robustness of findings across specifications: formalization (site-by-site)



Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated with each point estimate. **Y** refers to the definition of the dependent variable. **Model** presents the main specification and the extreme value bounds for the study in Nigeria, where there is evidence of systematic attrition. **Sample** refers to specifications with and without the replacement sample in India. **Controls** refers to the specifications with and without baseline covariates. **Subsets** indicates the study associated with the specification. **Blocks** present the specifications using binary or continuous variables to account for blocks in the Nigeria study.

Figure S7. Robustness of findings across specifications: tax payment (site-by-site)



Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated with each point estimate. **Y** refers to the definition of the dependent variable. **Model** presents the main specification and the extreme value bounds for the study in Nigeria, where there is evidence of systematic attrition. **Sample** refers to specifications with and without the replacement sample in India. **Controls** refers to the specifications with and without baseline covariates. **Subsets** indicates the study associated with the specification. **Blocks** present the specifications using binary or continuous variables to account for blocks in the Nigeria study.