

Fiscal Contracts? A Six-country Randomized Experiment on Transaction Costs, Public Services, and Taxation in Developing Countries

Ana L. De La O,¹ Donald P. Green,² Peter John,³
Rafael Goldszmidt,⁴ Anna-Katharina Lenz,⁵
Martin Valdivia,⁶ Cesar Zucco,⁴ Darin Christensen,⁷
Francisco Garfias,⁸ Pablo Balán,⁹ Augustin Bergeron,⁹
Gabriel Tourek,¹⁰ Jonathan Weigel,¹¹ Jessica Gottlieb,¹²
Adrienne LeBas,¹³ Janica Magat,¹³ Nonso Obikili,¹⁴
Jake Bowers,¹⁵ Nuole Chen,¹⁵ Christopher Grady,¹⁵
Matthew Winters,¹⁵ Nikhar Gaikwad,² Gareth Nellis,⁸
Anjali Thomas,¹⁶ Susan Hyde¹⁷

September 21, 2021

¹Yale University. ²Columbia University. ³King's College London. ⁴Gertulio Vargas Foundation, Rio de Janeiro, Brazil. ⁵RMIT University of Melbourne. ⁶GRADE, Lima, Peru. ⁷University of California, Los Angeles. ⁸University of California, San Diego. ⁹Harvard University. ¹⁰Massachusetts Institute of Technology/JPAL. ¹¹London School of Economics. ¹²Texas A&M University. ¹³American University. ¹⁴University of Stellenbosh. ¹⁵University of Illinois at Urbana-Champaign. ¹⁶Georgia Tech. ¹⁷University of California, Berkeley.

Abstract

We present results from six randomized controlled trials jointly designed to promote formalization and tax payments in low- and middle-income countries. Each randomized intervention used in-person visits, during which citizens received information about the government benefits that come with formalization and assistance in undertaking one of three types of formalization (business registration, property regularization, and access to public services). A meta-analysis shows that the average effect of these interventions on citizens' intent to formalize, on formalization, and on tax payment is indistinguishable from zero. However, we find substantial heterogeneity across sites in individuals' intent to formalize and actual formalization, which suggests that there are both demand- and supply-side barriers to formalization. The results shed light on central questions about informality and underscore the difficulty of regularizing taxation and service provision in low- and middle-income countries.

Significance statement

In the global south, many citizens live and work outside of their countries' legal regulations and protections. This study is the first large-scale multi-country randomized trial to evaluate if in-person outreach efforts to reduce up-front transaction costs and increase the salience of public services can move citizens out of the informal sector. We follow the effects of the interventions on citizens' intention to formalize, their actual formalization, and their payment of the relevant taxes. The results show that the bundled interventions have little effect on average, and when they do work, bureaucracies prevent the formalization process from moving forward. The study has implications for the design of formalization policies often championed by the international community.

1 Introduction

The number of people in low- and middle-income countries living and working outside of their countries' legal regulations and protections is large and rising. The informal sector accounts for between one-third and two-thirds of developing countries' economic activity (1), and, globally, a billion people live in informal housing (2). Although there is a growing consensus that informality limits economic development, erodes governments' fiscal capacity, and diminishes citizens' access to adequate social protection and safe working environments (3; 4), its causes are contested, as are the policies to address it.

Is it possible to promote formalization by reducing entry costs and increasing the salience of its potential benefits? While the persistence of informality following a wave of administrative reforms worldwide has engendered skepticism (5), recent research in economics and political science offers reasons for optimism. On the entry costs side, recent single-case randomized control trials demonstrate that when individuals receive information and personalized assistance to navigate the formalization process, they are more likely to formalize (6; 7; 8). These results suggest that individuals confront behavioral constraints, which in-person assistance can solve. Research from multiple policy domains also shows the importance of personal outreach when removing hurdles to persuade individuals to take costly actions (9; 10; 11).

On the benefits side, scholars have argued that public services could help persuade individuals to opt into formality (12).¹ International development agencies have embraced this argument (4). However, most experimental studies inspired by the *fiscal contract*, in which governments provide public goods in exchange for taxation, have examined tax evasion by firms and citizens already on the tax rolls.² Therefore, we do not know whether public services have the same appeal to those currently outside the formal sector.

This article presents six independent but coordinated randomized controlled trials (RCTs) designed to promote formalization and tax payments. The interventions used in-person visits in which citizens received information and personalized assistance in undertaking one of the following formalization processes: small business registration,

¹See, also, (13; 14; 15; 16; 17; 18).

²See, for example, (19; 20; 21; 22; 23; 24).

acquisition of a property title, or access to public services. The interventions aimed to reduce the up-front transaction costs of formalization and increase the salience of public services available in the formal sector. We follow the effects of the interventions on citizens' intention to formalize, their actual formalization, which also depends on action by government agents, and, ultimately, their payment of the relevant taxes.

We use the multi-site collaborative model in Dunning et al. (2019) (25). 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.³ These coordination efforts allow us to aggregate the results in a meta-analysis and evaluate the external validity of each RCT.⁴ 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.

Aggregating the six experiments' results using a pre-registered meta-analysis, we find little evidence that these interventions on average produced a positive effect on formalization or tax payments. The meta-analysis, however, reveals statistically significant heterogeneity across studies in the interventions' effect on subjects' intention to formalize and formalization. In the causal chain from intention to formalize to the payment of taxes, some interventions failed because they did not sway people to try to formalize. Other interventions raised subjects' intention to formalize, but local bureaucrats prevented formalization from moving forward.

This article makes three contributions to our understanding of informality. First, our multi-site study calls into question the external validity of results from prior single-case field experiments showing that a bundled intervention of information and in-person assistance increases formalization.

Second, we provide experimental evidence supporting the argument that some

³In addition, each RCT includes a study-specific arm, which allows researchers to compare the effectiveness of the common treatment arm.

⁴The six RCTs are part of the Metaketa Initiative, organized by the Evidence in Governance and Politics network, which promotes cumulative learning through collaboration among researchers to conduct multi-site RCTs.

people choose the informal sector while others are forced into it.⁵ In the latter case, our studies show that some of the hurdles that keep people in the informal sector are behavioral (lack of time, cognitive capacity, attention) because when offered personalized assistance, willingness to formalize increases. Other hurdles come from local bureaucrats' responses to the increased demand for formalization. Therefore, people in the informal sector are not alone in maintaining the equilibrium of informality and low tax revenue across contexts. We uncover demand- and supply-side barriers to reducing informality, which suggest that policy makers must address both in order to succeed.

Third, our interventions raise questions about the conditions under which the fiscal contract theory might work. Explaining to individuals the potential formalization benefits derived from public services did not give the bundled interventions a consistent boost across studies. The heterogeneity in our results suggests that there are critical gaps in our understanding of what types of public goods and policies can entice individuals to join the formal sector and pay taxes.

The study has implications for the design of formalization policies often championed by the international donor community. Prior experimental evidence suggested that governments might need to offer personalized assistance to persuade citizens to operate in the economy formally, to register their property, and to pay for public utilities. Given how challenging it is to promote formalization, it was worth exploring whether this type of demand-side intervention was effective in different contexts. Had it worked, future research could have examined how to optimize the intervention and facilitate its scaling-up. It is encouraging that some of our interventions generated interest in formalization even where citizens perceive that public corruption is widespread. Still, interventions may fail to produce the desired result unless local bureaucracies are structured to facilitate formalization rather than block it or extract resources from those seeking to formalize.

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 our results, and Section 5 concludes with a discussion of the implications

⁵See, for example, (4).

of our findings. We provide additional information about our materials and methods in Section 6.

2 Literature review

Scholars have argued that entry costs and formalization benefits are two major determinants of formalization. Most empirical work and policy interventions have focused on the former. Indeed, a worldwide wave of regulatory reform followed the diagnosis that costly regulations and complicated bureaucratic procedures prevent people from joining the formal sector (26), seemingly to no avail (5). One explanation for the limited success of system-wide reforms is that, even when national governments simplified formalization processes, hurdles such as lack of information or the perception that the process is onerous remain (27).

Social science research has argued that removing hurdles, even seemingly minor ones, is a powerful and cost-effective strategy for changing behavior (11). Single-case field experiments have evaluated interventions that remove some of the hurdles that people face to formalize. Results have been mixed. Interventions that address the lack of information seem not to promote formalization (28; 8). Combining information with an offer to reimburse registration costs does not work either (29; 30). However, interventions that bundle information with face-to-face personalized assistance to navigate the formalization process have produced positive effects on the order of 2 to 16 percentage points (6; 7; 8).⁶

A closer look at 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. For example, Benhassine et al. (2018) find 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 (8).⁷ Campos et al. (2015) find that, while information and personalized assistance increased tax registration by 2 per-

⁶Table S7 includes more information about prior field experiments on firm registration.

⁷Benhassine et al. (2018) also find that more personalized assistance, such as help to open a bank account and prepare tax returns forms, increases the effects' size by 13 (std error=1.4) and 16.3 percentage points (std error=1.3), respectively.

centage points (std. error=1.2) in Malawi, an intervention that added group-based information sessions at a bank did not work (7).⁸ Similarly, in Colombia, the bundled intervention 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 (6).

That personalized assistance can promote formalization is broadly consistent with randomized control trials from multiple policy domains showing that personal outreach can persuade people to take costly actions. For example, Morgan et al. (2010) show that personal outreach boosted the effectiveness of organ donation appeals at workplaces (10). Czap et al. (2019) find that in-person visits encourage farmers' participation in the Conservation Stewardship Program, but not handwritten letters (9). On the other hand, personal assistance, too, has occasionally produced disappointing results. Fowlie et al. (2015), for example, find that offering people assistance to adopt an energy savings program did not increase the take-up rate of the program, even though it was free (31).

Overcoming skepticism about benefits is especially pertinent to the challenge of encouraging formalization. When citizens choose the informal sector because they see few formalization benefits (5), removing hurdles might not persuade them to formalize. Following this logic, most interventions that offer people information and assistance include a description of some formalization benefits, such as access to cheaper credit and potentially higher profits. In practice, however, this type of private benefit often does not materialize for small firms, which account for most informal firms.⁹

Political science and economics research has argued that governments could increase the perceived benefits of formalizing by providing public goods and implementing policies that link benefits to operating in the formal sector (12). For example, De Mel et al. (2013) found that an intervention in Sri Lanka that combined information about formalization and a cash transfer to firms willing to formalize equivalent to a half month's profit increased formalization rates by 13 percentage points (std error=3.8). Increasing the cash transfer value to the equivalent of one and two

⁸In Malawi, business and tax registration are separate processes. Campos et al. (2015) found substantially larger effects of interventions on business registrations (54 to 68 percentage points increases).

⁹See, for example, (32; 33; 34; 8; 35).

months' profit produced positive effects of 10 percentage points (std error=3.5) and 27 percentage points (std error=4.7), respectively (29).¹⁰

Other experimental work, which focuses on the behavior of firms and citizens already on the tax rolls, offers additional insights into how public services could be incorporated into interventions to promote formalization and tax payments. Interventions that mailed citizens information about their government's use of taxes to fund public services seem not to increase tax payments in the US (19), Argentina (20) or Switzerland (23; 24). However, information connecting taxes to public services that are widely used in the UK, such as the National Health Service, does increase the timely payment of taxes (22). Moreover, the few experimental studies that evaluate the impact of randomly providing public services on tax payments show that the construction or renovation of sidewalks in Argentina increased the timely payment of property taxes by 3.7 percentage points (std. error=1.6) (36). And, the first-time asphaltting of streets in Mexico increased the payment of property taxes by 4.4 percentage points (std. error= 0.26) (21). These results suggest that in some contexts public services could play a role in promoting formalization, but, as we mentioned earlier, more research on this question is justified because we do not know if individuals operating in the informal sector respond similarly to individuals already on the tax rolls.

As we will describe in the following sections, our multi-site study employed a bundled intervention of information and in-person assistance to promote formalization. We selected sites where governments have taken steps to simplify formalization processes, but some frictions and hurdles can prevent people from formalizing. As in prior work, we included information about formalization processes, and we also incorporated information about specific benefits derived from government services that could enhance the bundled treatment.

3 Design

In this section, we describe the contexts in which our RCTs took place. We then describe the harmonized interventions and discuss key variations across studies.

¹⁰See Table S7, set 5, for more information about this study.

3.1 Context of the experiments

Our RCTs took place in low-income neighborhoods in Rio de Janeiro (Brazil) and Bogotá (Colombia), public marketplaces 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 in these sites. As Table 2 shows, a majority of respondents in our control groups expressed more agreement with the statement that citizens “should only pay their taxes if they agree with the government and its policies” than “citizens should always pay their taxes.” Moreover, most of them believed that the government would not respond to their communities’ needs 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 citizens in our studies have low tax morale and are skeptical of their governments, which arguably makes these sites pertinent but hard cases for efforts to facilitate formalization.

Governments in our sites, like other governments worldwide, have tried to promote formalization by simplifying and reducing the costs of entry into the formal sector. In Brazil, Colombia, and Nigeria, governments have simplified the processes to register small businesses, reduced application fees, and, in some cases, reduced ongoing costs of taxation to encourage 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 in the DRC offered a simplified procedure and subsidized prices for land titles as part of our evaluation. Nevertheless, in the control groups at endline, business registration rates were 16% in Brazil, 10% in Colombia, and 20% in Nigeria; 3% of households in India had a municipal water connection; 13% of residents paid their city fee in Malawi; and, 0% of property owners had a title to their land in the DRC.

Even when regimes are simplified or legal barriers removed, up-front transaction costs could remain an obstacle for citizens to formalize. Preliminary field work in all our sites suggested that citizens lacked information about the requirements to register their firm, acquire a property title, or gain access to public services and utilities. Moreover, citizens widely perceived formalization processes to be onerous, while they rarely knew about the benefits they would receive from firm registration, property titles, and paying for public utilities or services.

3.2 The interventions: commonalities and variations

Our RCTs included three studies that focused on small business registration, one study that focused on property regularization, and two studies that focused on access to public services. The common treatment arm 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. The first component of the intervention was at least one in-person visit in which individuals received information about the process they needed to follow to formalize and about the costs and benefits of formalization. Second, during the visits, individuals received offers of assistance in navigating the formalization process.

Although the processes of formalization varied across sites, our interventions aimed to reduce up-front costs substantially. 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 studies, 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 program), which gives microentrepreneurs subsidized access to social security. In Colombia, the intervention mentioned that formal businesses can sell goods and services to government agencies.¹¹ 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.

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 1.4% to 4.8% of the annualized minimum wage, depending on the house’s 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

¹¹The 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 licence.

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 50 to 100 times the daily minimum wage, the *Contrat de Location* 30 to 50 times that wage, and the *Acte de Vente Notarié* 15 to 25 times the daily minimum wage. The property tax was twice the daily minimum wage salary for most property owners in the sample.

The research teams worked with local organizations or local governments to deliver 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 in 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 harmonized core components of the common treatment arm, a few 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 dispensation.¹² Finally, treatment dispensation took place at the slum-cluster level in India. The rest of the RCTs randomized the intervention at the business or household level.

¹²In the placebo intervention, enumerators provided information on recognizing and dealing with stress.

3.3 Outcomes

Formalization is a process that citizens can initiate, but it is possible (and in some cases likely) that the government could knowingly or unknowingly block the process and frustrate citizens' attempt to formalize. Therefore, first we measure whether subjects assigned to the common treatment arm had an intent 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, a subject who scheduled and received a home visit from a government surveyor is coded as having attempted to formalize. Intent to formalize is a binary outcome.¹³

Formalization takes place when citizens complete the process of registration. A firm is coded as formal if it is registered as MEI in Brazil, registered with the 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 inter-

¹³In 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.

ventions. The type of tax varies across studies. 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.

4 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, in neither case can we reject the null hypothesis that the effects are zero 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).

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).

The country-by-country analysis of the common treatment arm effects is informative because it shows where the process of formalization breaks down in each site. Figure 1 reports these results. For the outcome of whether citizens 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 India, the DRC, and Brazil, the common treatment arm increased citizens' intent to formalize by 11 pp (s.e.=2 pp), 30 pp (s.e.=3 pp), and 5 pp (s.e.=3 pp), respectively.

For the outcome of whether citizens 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 citizens 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 citizens who seek to regularize their property and citizens who manage to complete the process and acquire a property title.

Of the six RCTs, we find that in four (Colombia, Nigeria, Malawi, and India) citizens 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 RCTs 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). We interpret the result in the DRC with some caution because it is possible that not enough time elapsed between the acquisition of titles and the endline measurement of tax payments to observe an effect.¹⁴ Still, there is little evidence that our interventions increased tax collection. Moreover, studies measured subjects' willingness to pay a series of locally relevant taxes, for which we find a tightly estimated zero, as shown in Table S6.

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. 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 citizens' access to pub-

¹⁴In addition, the government did not conduct a property tax collection campaign in 2019, which means that households were not solicited in person to pay their property taxes.

lic services. The other set includes tax morale, perceptions of tax compliance, and willingness to pay taxes. Consistent with the results in Table 4, we find that the common treatment arm did not affect these downstream outcomes. Table S6 reports these findings.

What explains the heterogeneous effects of the interventions on the intent to formalize? One possibility is that the variation across our studies in the public benefits offered in exchange for taxation 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 three dimensions of public benefits that seem to be relevant. One is whether benefits are individual versus collective. For example, subsidized social security in Brazil, land titles in the DRC, and affordable, high-quality water in India seemed to generate interest from our subjects. In contrast, when governments offer collective benefits, such as neighborhood trash collection in Malawi, subjects seemed unpersuaded. A second dimension is whether benefits are tangible versus uncertain. For example, access to water (as in the India study) could seem more tangible than the possibility of applying for a government loan, which may be difficult to value when citizens are uncertain whether they will get it (like in the Colombia study). Finally, citizens seem to be willing to take steps to formalize to obtain government benefits for which there is no superior alternative, such as property rights (as in the DRC study). Other public services, like neighborhood trash collection in Malawi, may be insufficient if citizens have informal means of waste disposal, which are less expensive, even if they are not socially optimal. For example, in Malawi, dumping trash in nearby fields remains a widespread practice. Future research could follow up on these hypotheses to investigate the conditions that enable or undermine a fiscal contract equilibrium.

An alternative possible interpretation that is less compatible with our results is that contextual factors, such as citizens' perceptions of state capacity and government corruption, account for the varying effects on the demand side. Suppose citizens are unwilling to enter into a fiscal contract with 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 provide services.

What explains the slippage between citizens' intent to formalize and actual formalization? One possible explanation is that bureaucratic obstacles limited the effectiveness of the common treatment arm in two cases. 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. 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. Nevertheless, burdensome and sometimes ad hoc additional requirements impeded water connections. One possible reason for the push-back from bureaucratic and political agents is that water mafias, which operate private commercial tanker businesses and sell water in slums at exorbitant rates (37), intervened behind the scenes to sabotage formalization.¹⁵

5 Discussion

In many low- and middle-income countries, a majority of small businesses operate in the informal sector of the economy, millions of people live in informal settlements, and a large number of households lack formal access to public services. Policymakers are eager to break out of this negative equilibrium, and behavioral interventions that reduce entry costs and promote a fiscal-contract-like reasoning among citizens, in which they pay taxes in exchange for public services, are tempting policy instruments,

¹⁵The bureaucratic interference observed in the India and the DRC studies is compatible with recent work showing that governments may allow firms to operate in the informal sector to garner electoral support (38; 39; 40). However, in our study, local politicians and bureaucrats seemed to obstruct the formalization process due to economic incentives rather than electoral calculations.

as they are much easier to promulgate than institutional reforms.¹⁶

Do such behavioral interventions work? Do they set potential taxpayers on the path toward formalization and tax compliance? Meta-analysis of our six studies shows that, on average, a reduction in up-front transaction costs and information about formalization benefits derived from government services did not significantly increase citizens' intent to formalize. Moreover, only a fraction of those who intended to formalize successfully did so, and only a fraction of those who formalized paid more taxes. As a consequence, the negative equilibrium of informality and low tax revenue prevails.

We are quick to acknowledge the limitations of our findings. First, in some cases, when administrative data were not available, studies used firms' or households' self-reports of intent to formalize, formalization, and tax payment, which may be inaccurate. Nevertheless, we find little evidence to suggest that self-reporting bias accounts for our results.¹⁷ Another limitation is that most of our studies measured tax payments a year or less after formalization, which may be a short period of time. Although intuition suggests that initial effects should decay over time as intent to formalize wanes, it is possible that a longer time frame might have revealed strong effects on outcomes such as tax payments.

That said, overall our meta-analysis shows that to persuade citizens to operate in the economy formally, register their property, and pay for public utilities, governments may need to offer in-person assistance, which may not be cost-effective. Although it is encouraging that some of interventions generated interest in formalization even where citizens perceive that public corruption is widespread and the government is indifferent to community needs, personalized outreach remains a costly path for governments that might otherwise hope to extract resources from citizens without investing additional resources to reach and persuade them. And, as we have seen, interventions may fail to produce the desired result unless bureaucracies are structured to facilitate formalization rather than to block it or to extract resources

¹⁶Taxation is thought to encourage citizens to demand effective and responsive governments (41; 42; 43; 44; 45) Had the bundled interventions increased tax payments, future research could have examined any downstream effects on governance-related outcomes.

¹⁷For 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.

from those who seek to formalize. Behavioral approaches to reduce informality seem destined to fail without institutional reform.

6 Materials and Methods

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.

6.1 Covariate Balance

For each study, we evaluate the baseline covariate balance produced by randomization with an omnibus test using the `xbalance` package in R (46). 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 can reject the null of balance (p-value=0.04). To account for this imbalance, we present our results with and without baseline covariates in Tables S4 and S5.¹⁸

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. Overall, we do not find asymmetric attrition between treatment and control (p-value=0.463). Table S2 in the supplementary materials shows this test country by country. Only in the Nigeria case 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

¹⁸For 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 p-value of 0.05 to random chance.

subjects assigned to control were treated. In Brazil, 12% of the subjects assigned to treatment did not receive treatment. In the study in Malawi, according to the canvasser-reported compliance, 1% of the subjects assigned to treatment did not receive treatment.¹⁹ In India, where treatment assignment was done at the cluster level, 4 of the 152 slum-clusters were assigned to treatment but failed to receive it.

6.2 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:

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

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.²⁰ 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 (48). 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.²¹

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 to identify prognostic

¹⁹The 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.

²⁰We follow Gerber and Green’s (2012) recommendations on how to analyze block randomized RCTs when the probability of treatment varies by block (47).

²¹Our 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.

covariates to be included in our specification (49). We also examine the robustness of our results to alternative specifications of our models that take into account study-specific analytical choices that were not included in our pre-analysis plan. 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 to each point estimate.

6.3 Ethical considerations

Careful consideration of ethics informed the design and implementation of each of the six studies. The collection of data from participants was conducted with informed consent, with safeguards for personally identifying information. Risks of harm to participants were 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 predation by 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. 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 appreciably affect the supply or distribution of public goods.

7 Competing interest information for all authors

All authors declare that they have no competing interests.

8 Data sharing plans

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

9 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.

10 Acknowledgements

We are grateful to Jaclyn Leaver for support of this research, and to Shikhar Singh for excellent research assistance. We thank 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.

References

- [1] Ulyssea G (2020) Informality: Causes and Consequences for Development. *Annual Review of Economics* 12(1):525–546.
- [2] UN (2021) Sustainable Development Goal 11: Sustainable Cities and Communities.
- [3] Portes A, Castells M, Benton LA, eds. (1989) *The Informal economy: studies in advanced and less developed countries*. (Johns Hopkins University Press, Baltimore, Md).
- [4] Perry GE, et al. (2007) *Informality: Exit and Exclusion*. (The World Bank).
- [5] Bruhn M, McKenzie D (2014) Entry Regulation and the Formalization of Microenterprises in Developing Countries. *The World Bank Research Observer* 29(2):186–201.
- [6] Galiani S, Melndez M, Ahumada CN (2017) On the effect of the costs of operating formally: New experimental evidence. *Labour Economics* 45:143–157.
- [7] Campos F, Goldstein M, McKenzie D (2015) *Short-Term Impacts of Formalization Assistance and a Bank Information Session on Business Registration and Access to Finance in Malawi*, Policy Research Working Papers. (The World Bank).
- [8] Benhassine N, McKenzie D, Pouliquen V, Santini M (2018) Does inducing informal firms to formalize make sense? Experimental evidence from Benin. *Journal of Public Economics* 157:1–14.
- [9] Czap NV, Czap HJ, Banerjee S, Burbach ME (2019) Encouraging farmers’ participation in the Conservation Stewardship Program: A field experiment. *Ecological Economics* 161:130–143.
- [10] Morgan SE, Harrison TR, Chewing LV, DiCorcia MJ, Davis LA (2010) The Effectiveness of High- and Low-Intensity Worksite Campaigns to Promote Organ Donation: The Workplace Partnership for Life. *Communication Monographs* 77(3):341–356.

- [11] Blair G, Littman R, Paluck EL (2019) Motivating the adoption of new community-minded behaviors: An empirical test in Nigeria. *Science Advances* 5(3):eaau5175.
- [12] Besley T, Persson T (2013) Taxation and Development in *Handbook of Public Economics*. (Elsevier) Vol. 5, pp. 51–110.
- [13] Acemoglu D (2005) Politics and economics in weak and strong states. *Journal of Monetary Economics* 52(7):1199–1226.
- [14] Bates RH, Donald Lien DH (1985) A Note on Taxation, Development, and Representative Government. *Politics & Society* 14(1):53–70.
- [15] Besley T, Persson T (2009) The Origins of State Capacity: Property Rights, Taxation, and Politics. *American Economic Review* 99(4):1218–1244.
- [16] Levi M (1989) *Of rule and revenue*. (University of California Press, Berkeley [u.a.]). OCLC: 1073760749.
- [17] Levi M, Sacks A (2009) Legitimizing beliefs: Sources and indicators. *Regulation & Governance* 3(4):311–333.
- [18] Timmons JF (2005) The Fiscal Contract: States, Taxes, and Public Services. *World Politics* 57(4):530–567.
- [19] Blumenthal M, Christian C, Slemrod J (2001) Do Normative Appeals Affect Tax Compliance? Evidence from a Controlled Experiment in Minnesota. *National Tax Journal* 54(1):125–138.
- [20] Castro L, Scartascini C (2015) Tax compliance and enforcement in the pampas evidence from a field experiment. *Journal of Economic Behavior & Organization* 116:65–82.
- [21] Gonzalez-Navarro M, Quintana-Domeque C (2015) Local Public Goods and Property Tax Compliance: Evidence from Residential Street Pavement. Working paper 15MG1.

- [22] Hallsworth M, List JA, Metcalfe RD, Vlaev I (2017) The behavioralist as tax collector: Using natural field experiments to enhance tax compliance. *Journal of Public Economics* 148:14–31.
- [23] Torgler B (2004) Moral suasion: An alternative tax policy strategy? Evidence from a controlled field experiment in Switzerland. *Economics of Governance* 5(3):235–253.
- [24] Torgler B (2013) A Field Experiment in Moral Suasion and Tax Compliance Focusing on Underdeclaration and Overdeduction. *FinanzArchiv* 69(4):393.
- [25] Dunning T, et al. (2019) Voter information campaigns and political accountability: Cumulative findings from a preregistered meta-analysis of coordinated trials. *Science Advances* 5(7):eaaw2612.
- [26] de Soto H (1989) *The other path: the invisible revolution in the Third World*. (Harper & Row, New York), 1st ed edition.
- [27] IFC (2013) *Reforming Business Registration : A Toolkit for the Practitioners*. (The World Bank).
- [28] De Giorgi G, Rahman A (2013) *SME Registration Evidence from a Randomized Controlled Trial in Bangladesh*, Policy Research Working Papers. (The World Bank).
- [29] de Mel S, McKenzie D, Woodruff C (2013) The Demand for, and Consequences of, Formalization among Informal Firms in Sri Lanka. *American Economic Journal: Applied Economics* 5(2):122–150.
- [30] de Andrade GH, Bruhn M, McKenzie D (2014) A Helping Hand or the Long Arm of the Law? Experimental Evidence on What Governments Can Do to Formalize Firms. *The World Bank Economic Review* p. lhu008.
- [31] Fowlie M, Greenstone M, Wolfram C (2015) Are the Non-Monetary Costs of Energy Efficiency Investments Large? Understanding Low Take-up of a Free Energy Efficiency Program. *American Economic Review* 105(5):201–204.

- [32] Rand J, Torm N (2012) The Benefits of Formalization: Evidence from Vietnamese Manufacturing SMEs. *World Development* 40(5):983–998.
- [33] Demenet A, Razafindrakoto M, Roubaud F (2016) Do Informal Businesses Gain From Registration and How? Panel Data Evidence from Vietnam. *World Development* 84:326–341.
- [34] McCaig B, Nanowski J (2019) Business Formalisation in Vietnam. *The Journal of Development Studies* 55(5):805–821.
- [35] McKenzie D, Seynabou Sakho Y (2010) Does it pay firms to register for taxes? The impact of formality on firm profitability. *Journal of Development Economics* 91(1):15–24.
- [36] Carrillo PE, Castro E, Scartascini C (2021) Public good provision and property tax compliance: Evidence from a natural experiment. *Journal of Public Economics* 198:104422.
- [37] Bjorkman L (2015) *Pipe politics, contested waters: embedded infrastructures of millennial Mumbai*. (Duke University Press, Durham).
- [38] Holland AC (2016) Forbearance. *American Political Science Review* 110(2):232–246.
- [39] Holland A (2017) *Forbearance as redistribution: the politics of informal welfare in Latin America*, Cambridge studies in comparative politics. (Cambridge University Press, Cambridge, UK ; New York, NY).
- [40] Feierherd G (2020) Courting Informal Workers: Exclusion, Forbearance, and the Left. *American Journal of Political Science* p. ajps.12576.
- [41] Brautigam D, Fjeldstad OH, Moore M, eds. (2008) *Taxation and state-building in developing countries: capacity and consent*. (Cambridge University Press, Cambridge ; New York). OCLC: ocn174130607.
- [42] Prichard W (2010) Taxation and State Building: Towards a Governance Focused Tax Reform Agenda. *IDS Working Papers* 2010(341):01–55.

- [43] Paler L (2013) Keeping the Public Purse: An Experiment in Windfalls, Taxes, and the Incentives to Restrain Government. *American Political Science Review* 107(4):706–725.
- [44] Ross ML (2004) Does Taxation Lead to Representation? *British Journal of Political Science* 34(2):229–249.
- [45] Weigel JL (2020) The Participation Dividend of Taxation: How Citizens in Congo Engage More with the State When it Tries to Tax Them*. *The Quarterly Journal of Economics* 135(4):1849–1903.
- [46] Hansen BB, Bowers J (2008) Covariate Balance in Simple, Stratified and Clustered Comparative Studies. *Statistical Science* 23(2).
- [47] Gerber AS, Green DP (2012) *Field experiments: design, analysis, and interpretation*. (W. W. Norton, New York), 1st ed edition.
- [48] Borenstein M (2009) *Introduction to meta-analysis*. (John Wiley & Sons, Chichester, U.K.). OCLC: 352835868.
- [49] Cooper J, Paluck E, Annan J (year?) A Pair-Matched Randomized Evaluation of Faith-Based Couples Counseling in Uganda, (American Economic Association), Technical report. Type: dataset.

11 Tables and Figures

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 morale	-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 morale** 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.

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

Table 4: Meta-analysis of the effects of the interventions on intent to formalize, formalization, and tax payment (in percentage points)

Outcome	Estimate	SE	P-value	P-value BH correction	Heterogeneity test (p-value)
Intent to Formalize	9.3686	5.7101	0.176	0.176	76.306 (0)
Formalization	2.9163	1.7978	0.166	0.176	22.4719 (0.0004)
Tax Payment	0.6757	0.7867	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. 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 random effects models. P-values in columns 3 and 4 are from two-tail 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.

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-tail 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-tail 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

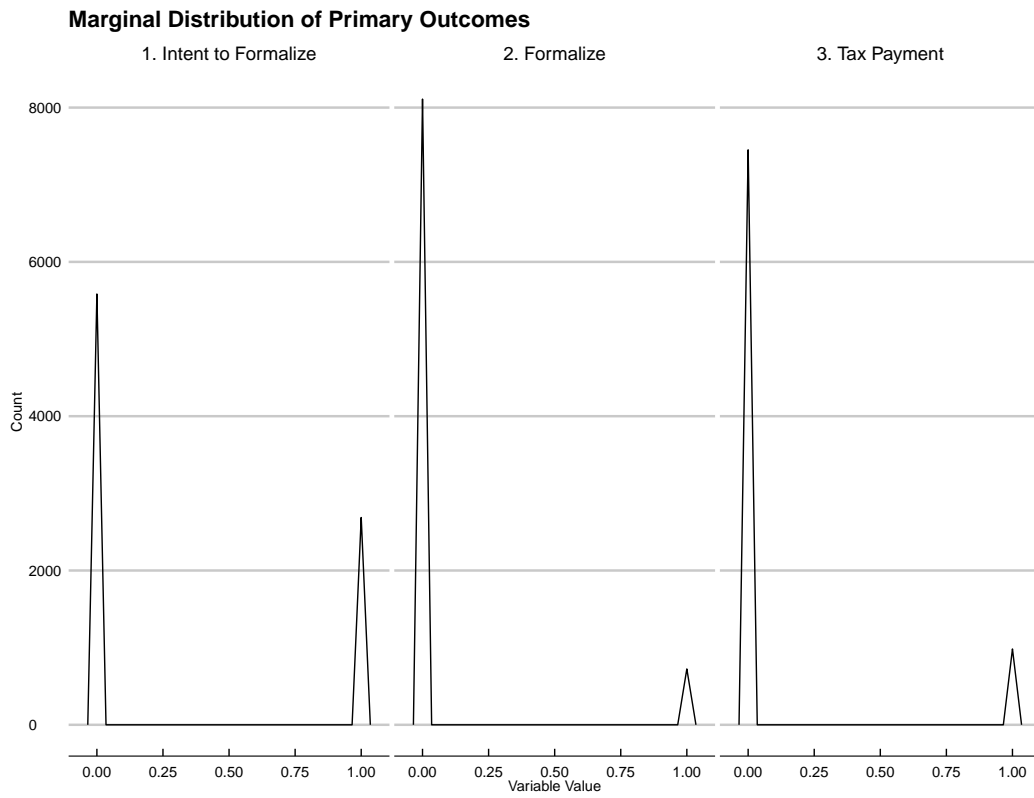
Notes: **Closest public service** refers to access to a public pension in Brazil, access to a public pension and healthcare in Colombia, access to a primary school (within a 15-minute walk) in Nigeria, access to tubed water in India, self-reported use of government-provided dumpsters as the method of 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-tail 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)

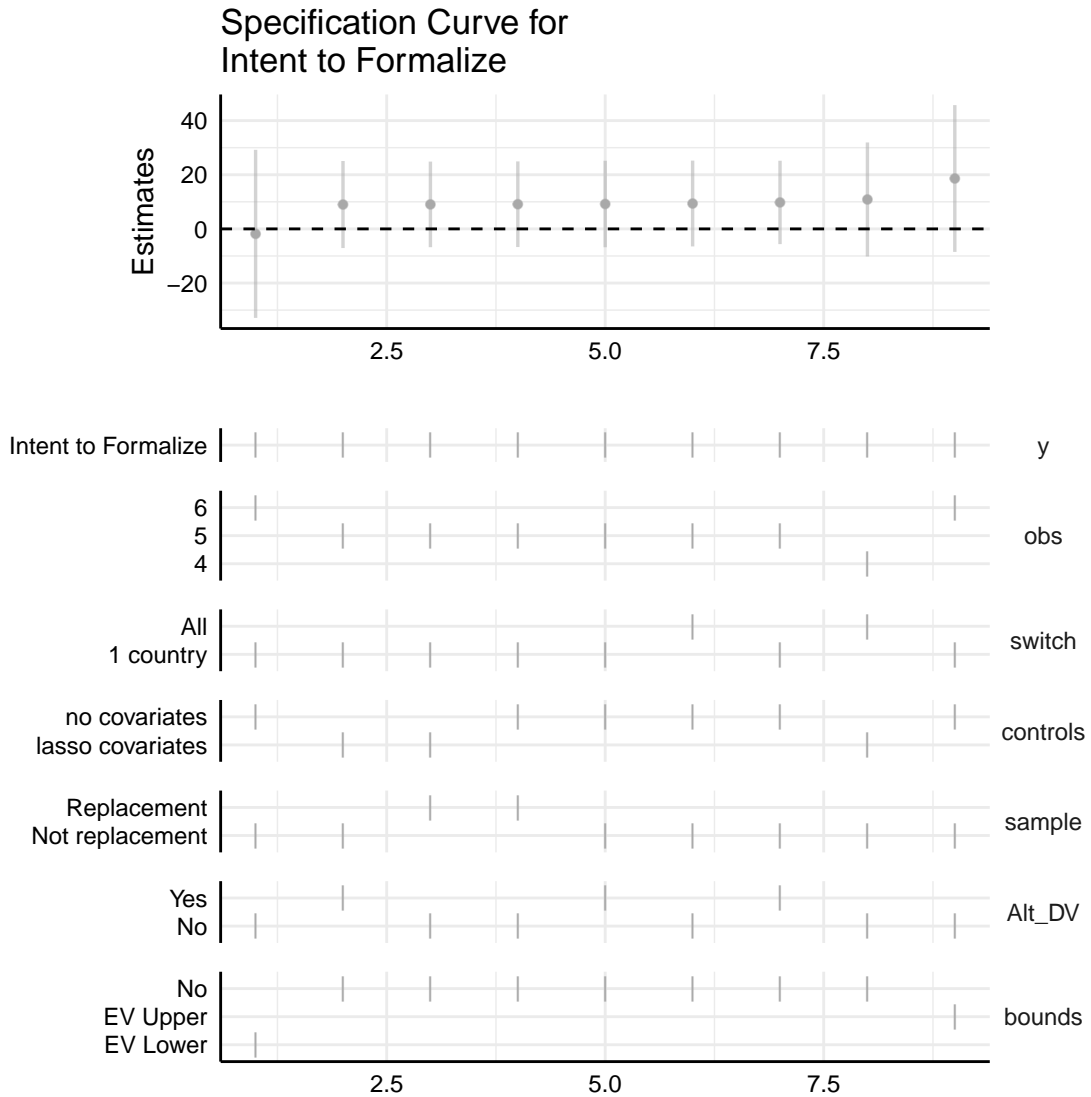
Notes: Benhassine et al. (2018) also report effects by number of months since intervention. The magnitude of the effect seems constant after 3-4 months, approximately. In de Mel et al (2013), firms had additional time to register (one month). In Galiani et al. (2017), the effects disappear two years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

Figure S1. Distribution of Primary Outcomes



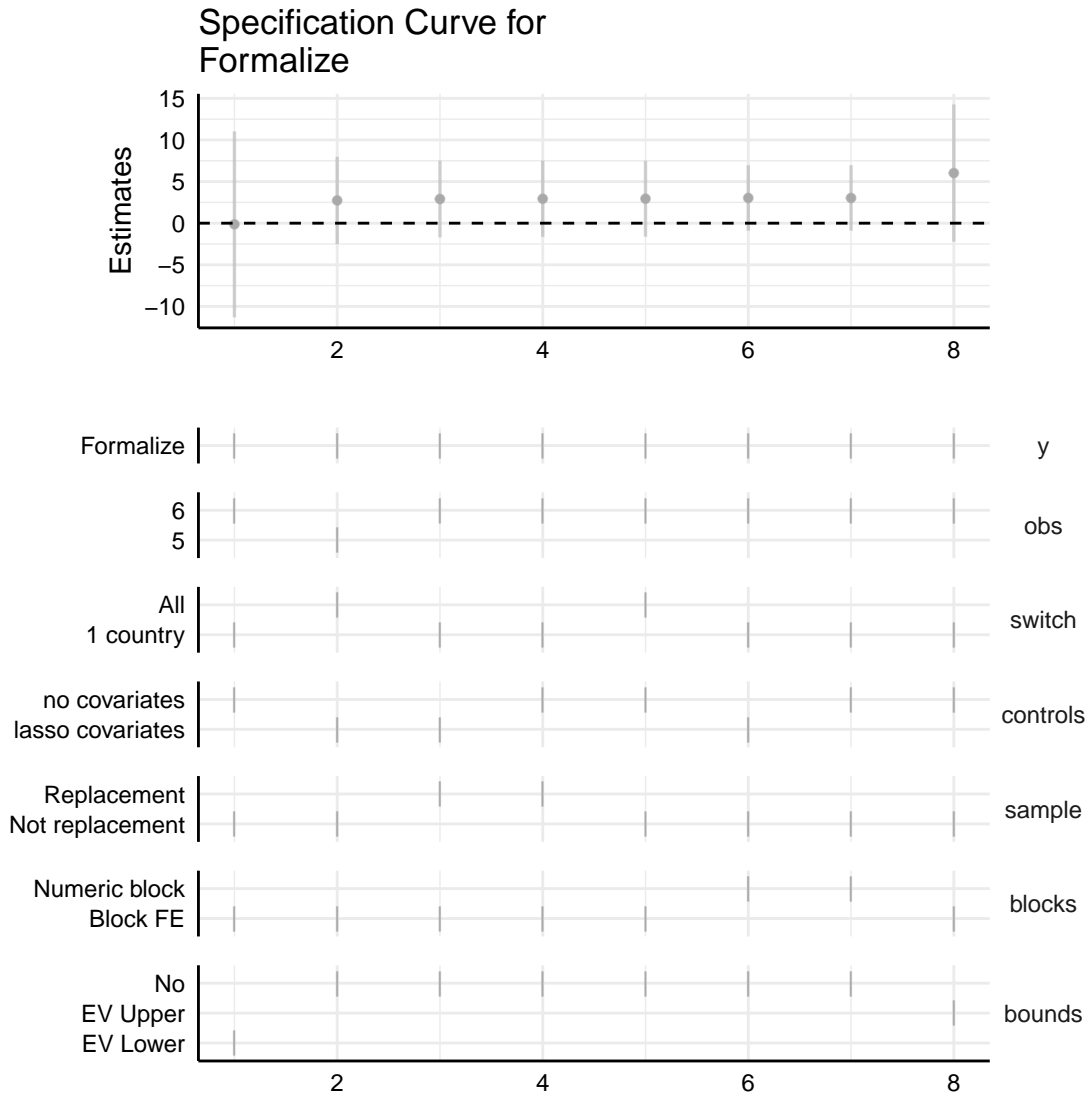
Notes: This figure presents the marginal distribution of the three primary outcomes: intent to formalize, formalization and tax payment. These three outcomes are binary.

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



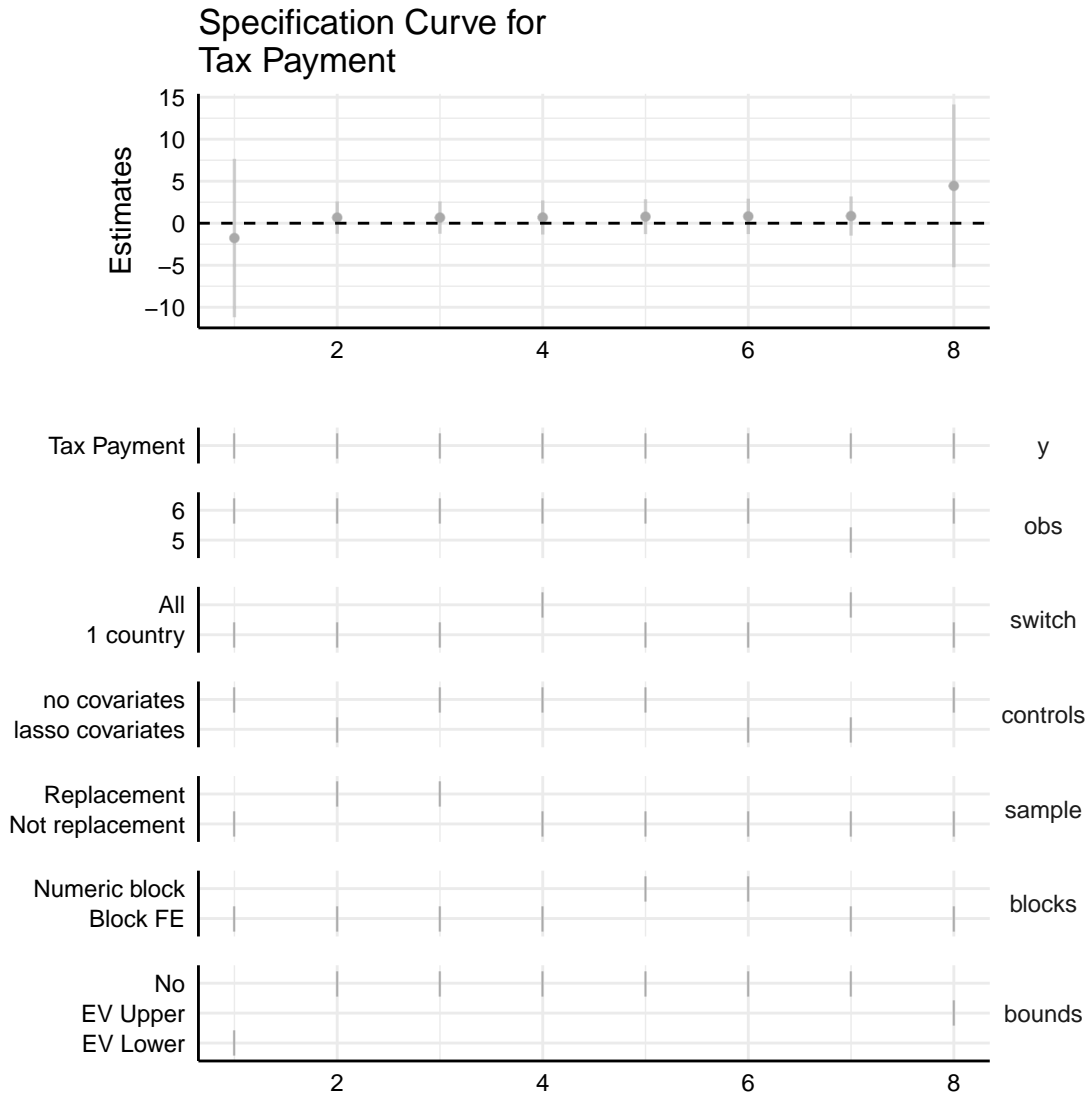
Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated to each point estimate. **Obs** refers to the number of studies included in the meta-analysis. **Switch** indicates whether the specification applies 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)



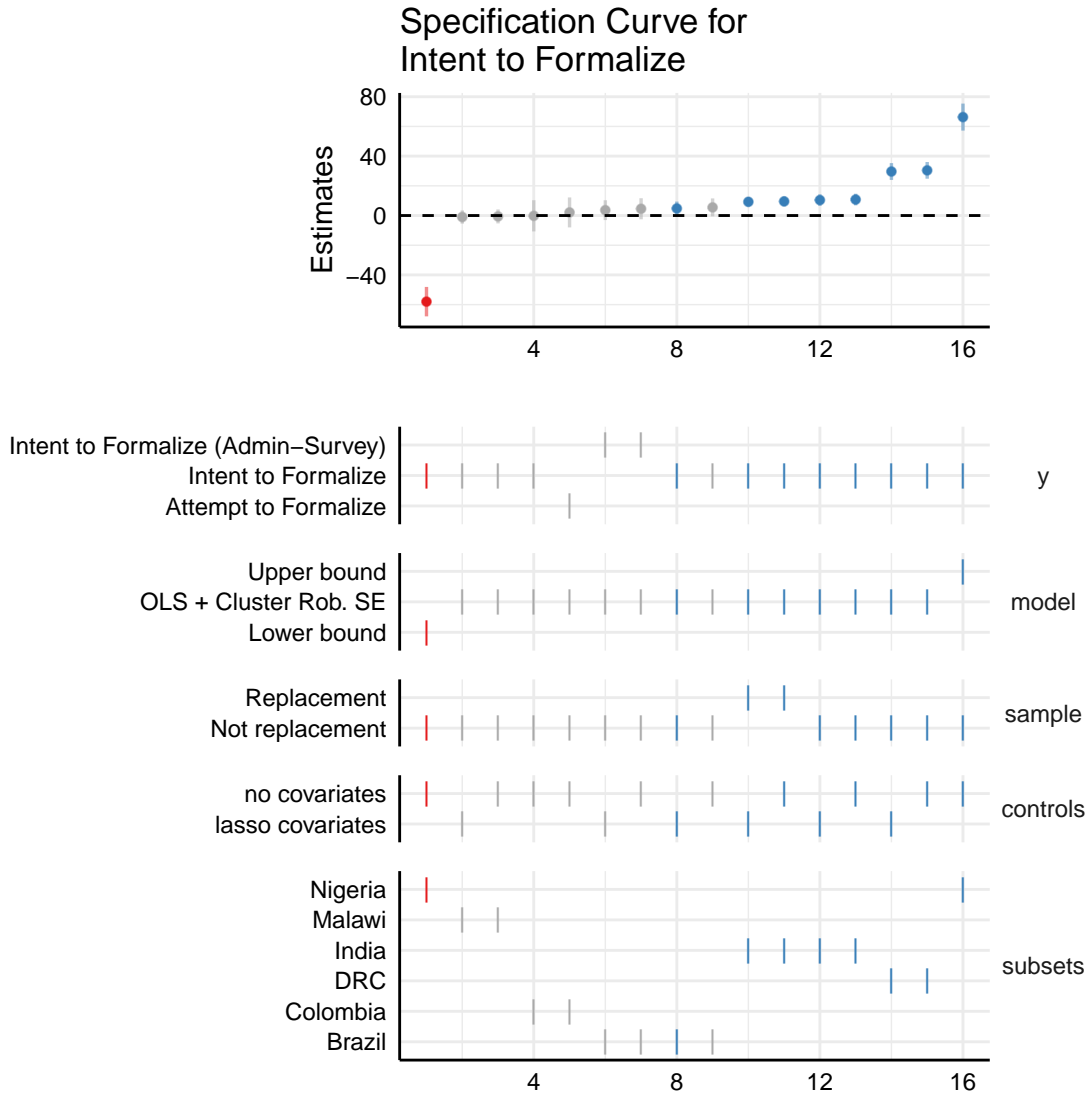
Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated to each point estimate. **Obs** refers to the number of studies included in the meta-analysis. **Switch** indicates whether the specification applies 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)



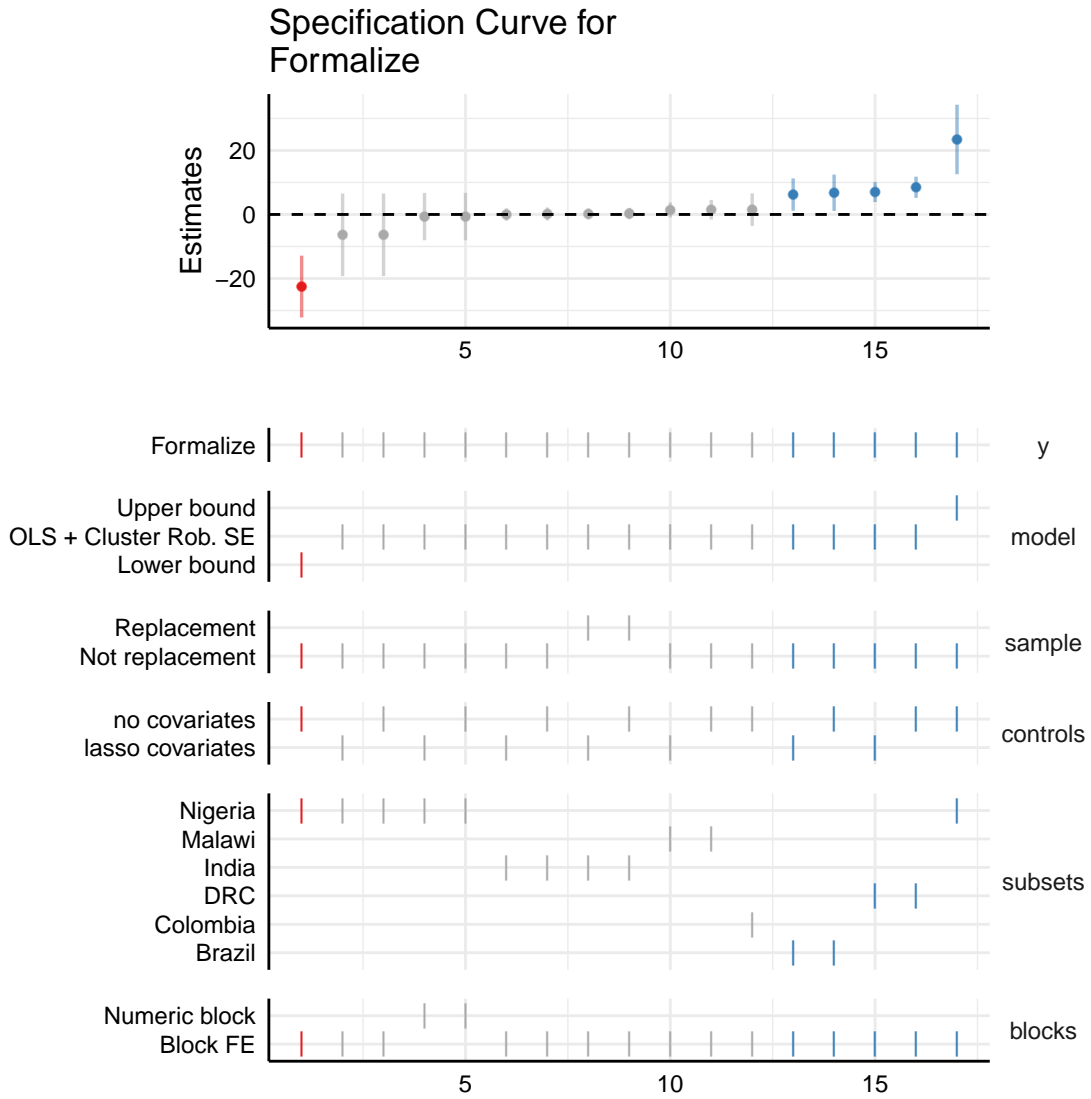
Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated to each point estimate. **Obs** refers to the number of studies included in the meta-analysis. **Switch** indicates whether the specification applies 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)



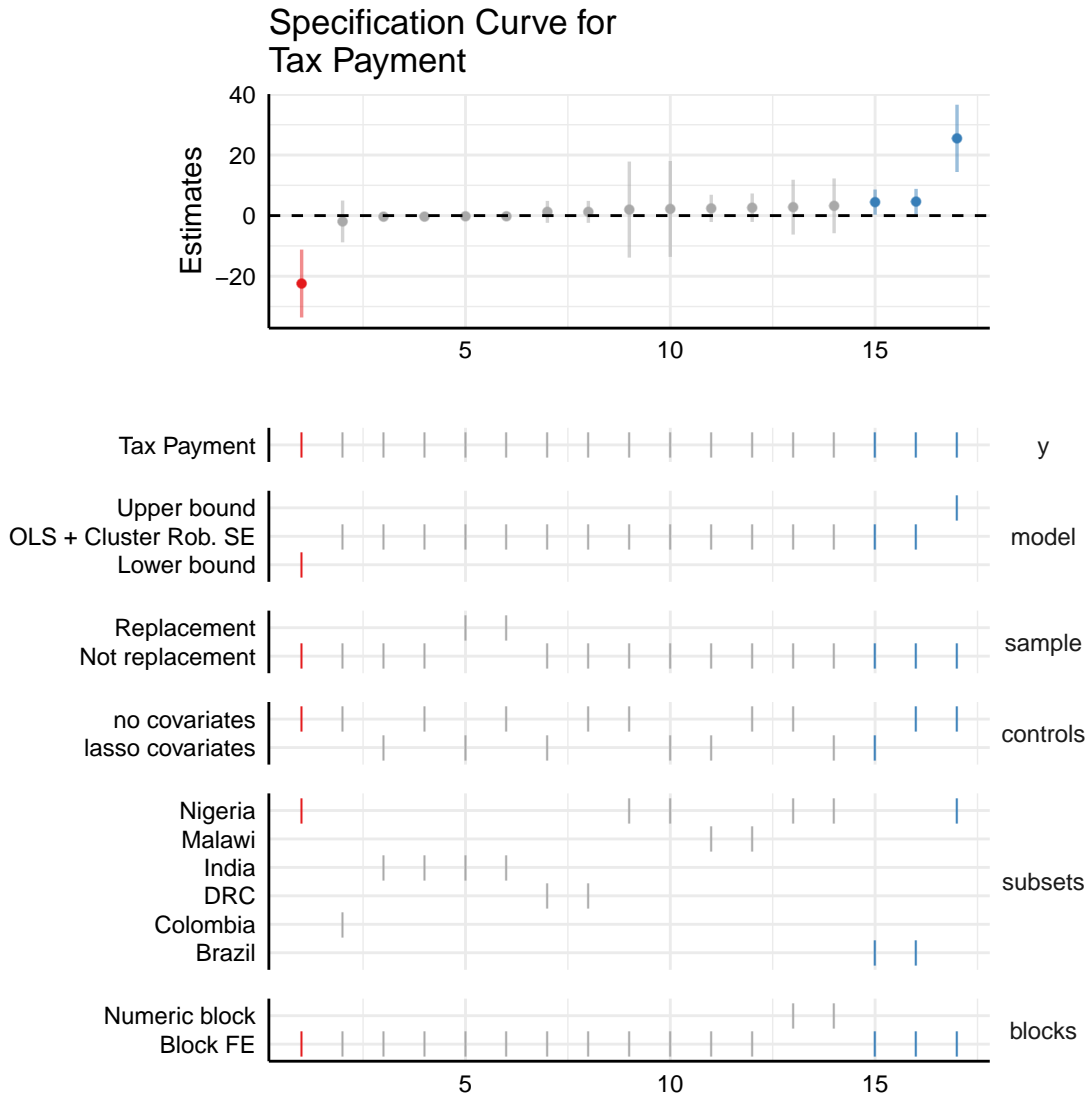
Notes: The top panel depicts the estimated ITT effect with its confidence intervals, and the bottom panel describes the model specification associated to each point estimate. **Y** refers to alternative definitions of the dependent variable. **Model** presents the main specification and the extreme value 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 to 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 to 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 to 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 to 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 to the specification. **Blocks** present the specifications using binary or continuous variables to account for blocks in the Nigeria study.

Metaketa II: Formalization, Tax Compliance, and Public Service Provision

Meta-analysis Pre-analysis Plan (Amended)

Ana L. De La O¹, Don Green¹, Peter John¹, Cyrus Samii¹,
Rafael Goldszmidt², Anna-Katharina Lenz², Martin Valdivia²,
Cesar Zucco², Darin Christensen³, Francisco Garfias³, Pablo Balan⁴,
Agustin Bergeron⁴, Gabriel Tourek⁴, Jonathan Weigel⁴,
Jessica Gottlieb⁵, Adrienne LeBas⁵, Janica Magat⁵, Nonso Obikili⁵,
Jake Bowers⁶, Nuole Chen⁶, Christopher Grady⁶, Mathew Winters⁶,
Anjali Bohlken⁷, Nikhar Gaikwad⁷, Gareth Nellis⁷, Susan Hyde¹

May 20, 2019

¹Author annotations are: #1 Metaketa steering committee, #2 Brazil study, #3 Colombia study, #4 DR Congo study, #5 Nigeria study, #6 Malawi study, #7 India study. We are grateful to Tara Slough, Fredrik Savje, and Peter Aronow for useful feedback, and to Shikhar Singh for excellent research assistance. This amendment was registered before the steering committee had any access to baseline or endline data.

Contents

1	Introduction	3
2	Motivation	3
3	The Metaketa Approach	4
4	Projects	5
5	Interventions	5
6	Hypotheses	7
6.1	Primary Hypotheses in the meta-analysis paper	7
6.2	Secondary Outcomes in the meta-analysis paper	7
6.3	Hypotheses to be included in a book-length project	7
7	Outcome measures in the meta-analysis paper	8
7.1	Attempt to formalize	8
7.2	Formalization	8
7.3	Access to public services	8
7.4	Tax compliance	9
7.5	Tax morale	9
8	Outcome measures in the book-length project	9
8.1	Knowledge and Perceptions about Taxation	9
8.2	Attitudinal Outcomes	9
8.3	Civic and Political Participation	10
9	Analysis details	10
9.1	Creating indexes	10
9.2	Primary Meta-analysis Strategy	10
9.3	Secondary Meta-analysis Strategy	11
10	Potential problems and strategies to address them	11
10.1	Randomization Check	11
10.2	Item-level Missingness on Covariates	11
10.3	Item-level Missingness on Outcomes	12
10.4	Item-level Missingness on assignment to treatment and treatment delivered	12
10.5	Different specification choices	12
11	Moderators	12
12	Ethics	13
13	Timing	13

1 Introduction

In this document we describe the research and analysis strategy for an EGAP Metaketa comprised of six field experiments on the formalization of citizens relationship with the government, public service provision, and tax compliance in low- and middle- income countries, with special attention to contexts where state capacity is low. First, we outline the motivation for a Metaketa on this topic. Then, we describe the Metaketa approach to accumulate knowledge, after which we describe the six projects and their common treatment arm and outcome measures. Next, we enumerate the hypotheses and details of the meta-analysis to be included in a stand alone paper. We also report additional hypotheses that will be the basis of a book-length project. We conclude with a brief discussion of our approach to ethics and describing the timing of the research.

2 Motivation

Taxation is crucial for development because it endows governments with resources needed to provide public services, and it encourages citizens to demand effective and responsive governments (Huntington 1991; Ross 2004; Paler 2013). Yet, many developing countries are trapped in a vicious cycle in which governments are unresponsive and unaccountable, the provision of quality public services is lacking, and tax compliance is low.

A vast scholarship has studied why some individuals comply with their taxes, and others do not. Yet, much of this work has focused on developed countries. Hence, the lessons learned may be specific to countries where citizens should expect that the government will use tax revenues to deliver public services, and that they are visible, or ‘legible (Scott, 1998), to the state through civil and commercial registration systems.

In developing countries, in contrast, government use of tax revenues to provide services may be questionable. And, the informal sector remains very large (McKenzie and Woodruff 2013), often with a majority of small businesses operating in the informal sector of the economy, millions of people living in informal settlements, and a disproportionate number of households lacking formal access to public services. In this context, could formalization trigger a chain of effects that has governments respond by providing services, and then culminates in an increase in tax compliance?

Like tax compliance, a citizen’s decision to formalize its relation with the state seems to be the result of a comparison of the expected benefits of formalization to its expected costs (Perry, et al. 2007; McKenzie and Sakho 2010; Neuwirth 2011). In the case of formalization, however, costs not only include future tax liability but, in many cases, include a costly and complex process. People in the informal sector may lack the information, time, and resources (financial and human capital) to navigate the bureaucracy and oftentimes arcane regulations. Moreover, in many cases, citizens are not certain that benefits of formalization will materialize. Then, the high costs of the process of formalization and its uncertain benefits may deter citizens from transitioning to the formal sector.

Experimental work on the topic has found that interventions that only provide information about the process of registration, including its benefits, have no effect on firms’ decision to formalize (Andrade et al. 2013; De Giorgi and Rahman 2013; Bruhn and McKenzie 2014). More expensive interventions, such as cash payments or business train-

ing, lead to higher rates of formalization (De Mel et al. 2013; Benhassine et al. 2016), but these are not cost-effective interventions, and they may not produce durable effects (Galiani et al. 2016). Finally, a few studies have shown that interventions that offered information about the process of formalization and assistance to people to undergo such process lead to higher rates of formalization (Campos et al. 2015; Benhassine et al. 2016; Galiani et al. 2016). However, the generalizability of the results of these studies may be limited. In Benhassine et al. (2016)'s and Galiani et al. (2016)'s studies, samples were drawn from Benin's largest city (Cotonou), and Colombia's capital city (Bogotá). In Campos et al. (2015)'s study, the sample included firms with larger revenues (25 percentile and above), with a fixed location, with more than one person working in the business, and with at least one worker contracted outside of family members and business owners. Hence, these studies show that in large cities, and among more profitable firms, there may be a latent demand for formalization, which is blocked by transaction costs. Yet, it is unclear whether the effects of the bundled treatment of information and assistance could be generalized to small- and mid- size firms outside of urban centers. Moreover, these studies have focused on the process of formalization of a business, which may not produce generalizable lessons for other formalization processes, such as the acquisition of a land title, or the formal access to basic public services.

The six projects in this Metaketa, and our meta-analysis, aim to contribute to this literature by studying the effects of a common treatment arm, to be implemented in a consistent manner by all projects, which combines information about the process of formalization, its benefits and costs, as well as assistance and/or subsidies to remove transaction-cost type barriers that impede those who otherwise wish to formalize to do so. Although the contexts of the projects are different, spanning from Colombia to India, in all cases preliminary field work by PIs suggests that there is a latent high demand for the formalization processes included in this Metaketa, which is probably driven by the tangible benefits associated with them, including the possibility to access valuable public services. Moreover, in all cases citizens pay costs of remaining in the informal sector, including unofficial payments, dependence on non-state utilities providers that extract high rates in exchange for low quality services, uncertainty, and fear of government repression. Details on the projects are included below. We will explore if the common treatment arm, which will encourage citizens to formalize its relation with the state, helps establish a fiscal contract between citizens and their governments, whereby local governments deliver public goods in exchange for tax compliance (Levi 1989; Tyler 1990; Levi and Sacks 2009; Paler 2013; Ali et al. 2014)

3 The Metaketa Approach

Metaketas are integrated research programs where multiple teams of researchers work on projects in parallel to generate generalizable answers to major questions of scholarly and policy importance. The goal of a Metaketa is the accumulation of knowledge. The core pillars of the Metaketa approach are:

1. **Major themes:** Metaketas focus on major questions of scholarly and policy relevance with a focus on consolidation of knowledge rather than innovation

2. **Strong designs:** all studies employ randomized interventions to identify causal effects
3. **Collaboration and competition:** teams work on parallel projects, they collaborate on design but may produce conflicting results
4. **Comparable interventions and measures:** by design, differences in findings should be attributable to contextual factors and not to differences in design
5. **Analytic transparency:** common commitment to analytic transparency including design registration, open data and materials, and third-party replication prior to publication
6. **Formal synthesis** based on ex-ante planning and integrated publication to avoid file-drawer bias

This Metaketa is administered by the Evidence in Governance and Politics (EGAP) network at the University of California, Berkeley. The initiative was launched in May 2016 and will run until March 2020.

4 Projects

A summary of the projects with details on their experimental designs is included in Table 1.

5 Interventions

The types of formalization included in this Metaketa are: registration of a small business (Brazil, Colombia, and Nigeria), acquiring a property title (DR Congo), and acquiring formal access to a publicly provided service, like tubed water (India), or waste collection service (Malawi). Preliminary field work by PIs suggests that there is a latent demand for these types of formalization. In Brazil, formalization of small businesses grants micro-entrepreneurs access to the highly valued social security system, and there seems to be a perception that it could protect them from harassment by street level bureaucrats. In Bogotá, Colombia, informal businesses lack information about business licences, and over ten percent of them are misinformed about their own status (Galiani, Meléndez, and Navajas Ahumada 2017). In Nigeria, members of trade associations reported, during preliminary research, that there was a strong demand for public services seen as linked to payment of Lagos State’s income tax, such as school registration. Accordingly, Lagos Internal Revenue Service officials indicate that applications for taxpayer registration increase in the periods preceding school term opening. Also, many Lagosians mentioned formalization and tax payment as grounds for demanding improved public services, such as electricity connections and road upgrading. In Malawi, informal interviews and focus groups suggest that there exists a strong demand among citizens for formal relationships with the city of Zomba, and that Zomba City Council members are aware of it. In particular, among

citizens who expressed an opinion about the types of services the city of Zomba should provide, 88% mentioned waste collection. In Mumbai, India, focus groups and interviews with slum residents suggest that there are several factors contributing to a high latent demand for the formalization of water services. First, municipally provided water is considerably cheaper than privately sourced water connections. Second, the quality of government provided water is higher than the quality of illegally provided water, owing to well-functioning filtration and chlorination systems. Third, the supply of government provided water is considered to be stable and reliable, whereas illegal water lines are frequently cut and discontinued. Finally, private water connections in Mumbai are largely provided by the so-called “water mafia”. Engaging with these illegal groups is intimidating for slum dwellers, who count among the poorest and most vulnerable residents of the city. Additionally, anthropologists and political scientists who have conducted in-depth ethnographic studies of the politics surrounding water access in Mumbai have reported widespread interest in government provided water connections among Mumbai slum dwellers (Anand 2011; Anand 2014; Björkman 2015). In DR Congo, participants of focus groups expressed interest in obtaining a formal title, if a surveyor came to their house to help with the process, which suggests that the main barrier to formalization is not lack of interest but transaction costs.¹

The common treatment arm in this Metaketa aims to remove transaction-cost type barriers that impede those who otherwise wish to formalize to do so. It consists of a bundled treatment. The first component of the intervention is information about the process citizens need to follow to formalize their relationship with the government. Second, subjects assigned to treatment will be offered information about the benefits and costs of formalization (i.e. tax liability). Third, and finally, the intervention will include a subsidy and/or assistance meant to reduce the costs of the process of formalization.

More specifically, individuals assigned to the common treatment arm will be offered a consulting session about the MEI program (Microentrepreneur program) in Brazil, which aims to formalize small firms. In Colombia, the treatment group will receive a visit from a local organization, in which they will be informed about the steps to process a *matrícula mercantil*. In both cases, treated individuals will be offered assistance to navigate the process of formalization at their business location. Similarly, in Nigeria treatment involves a consulting session about the process of obtaining a tax payer identification and an electronic tax clearance certificate (e-TCC). Treated individuals will be offered information about benefits and costs of formalization, including an assessment of their income tax liability for the first and for subsequent years, as well as assistance to fill out the necessary paperwork. In DRC, the process of acquiring a property title may seem complex and expensive to people. Thus, the common treatment arm will provide subjects with information, a discounted price for a legal title, and on site assistance in filling paperwork. In Malawi, treated individuals will be offered information about the opportunity to receive waste collection services from the city, if they pay the city fee, as well as information about the availability of bank transfers for city rates and Zomba City Councils tax forgiveness

¹Evidence from land titling programs in Mexico and Argentina also suggest that informal dwellers are keenly interested in securing formal titles, both in rural and urban settings. In Mexico, de Janvry, Gonzalez-Navarro, and Sadoulet (2014) note that a voluntary rural land-certification program implemented between 1992 and 2006 successfully certified 92% of the communal land it targeted. For a program in Buenos Aires, Argentina, Galiani and Schargrodsy (2010) report that 87% of owners of informal plots seized the opportunity of gaining a formal title when offered one.

plan. The intervention will also include two free waste collection pickups to signal that the city have the capacity to provide the service. In India, acquiring a connection to a water main involves a complex bureaucratic process. Thus, the common treatment arm includes, in addition to the information and on-the-ground assistance in completing the paperwork, help to liaise with the municipal engineers, and help to contact an appropriate plumber.

In all studies the common treatment arm will be compared to a pure control group. In addition, studies include alternative treatment arms, which are meant to be the basis of individual articles.

6 Hypotheses

6.1 Primary Hypotheses in the meta-analysis paper

In our stand-alone paper, we have four primary hypotheses that link the encouragement intervention to a chain of outcomes, including citizen's intent to formalize their relation with the state, formalization, access to public services, tax morale, and tax compliance.

- H1 Common treatment arm increases citizen's intent to formalize their relationship with the government
- H2 Common treatment arm increases citizen's formalization of their relationship with the government
- H3 Common treatment arm increases citizen's access to public services tightly related to the formalization process
- H4 Common treatment arm increases citizen's tax compliance (i.e. payment of taxes or dues for services provided) directly related to the process of formalization

6.2 Secondary Outcomes in the meta-analysis paper

Secondary hypotheses relate to outcomes that are not directly involved on the process of formalization, but that could be impacted as a result of a citizen formalizing its relation with the state:

- H5 Common treatment arm increases citizen's access to other public services
- H6 Common treatment arm improves citizen's tax morale more generally, and increases willingness to pay taxes not directly involved with the formalization process

6.3 Hypotheses to be included in a book-length project

In addition to the previous hypotheses, we plan to analyze in a book-length project a series of outcomes that are not directly involved on the processes of formalization in the interventions, but that could be impacted as a result of a citizen formalizing its relation with the state, becoming a tax payer, and/or experiencing public service provision (or lack thereof):

- H7 Common treatment arm improves citizen’s attitudes towards the government
- H8 Common treatment arm increases civic and political participation
- H9 Common treatment arm affects perceptions about prevalence of non-compliance with taxation, as well as probability of being caught not paying taxes, and the severity of the punishment associated with it
- H10 Common treatment arm affects knowledge and perceptions about taxes
- H11 Common treatment arm reduces reliance on non-public providers of utility services

7 Outcome measures in the meta-analysis paper

7.1 Attempt to formalize

All common treatment arm interventions will encourage citizens to formalize their relationship with the state (in different domains). Yet formalization could be a complex process, which some citizens may not complete. For example, a citizen may initiate the paperwork to formalize, but may find it impossible to navigate the bureaucracy until completion. Moreover, formalization is the product of actions undertaken by citizens and by the state. Citizens may complete their part of the process, but it is possible that the state could fail on its end. Therefore, the first outcome of interest consists on measuring if subjects exposed to the common treatment arm attempt to formalize. In some studies, like in Brazil and Nigeria, attempt to formalize takes place when subjects fill in and submit the paperwork to register their business. In some other studies, like in Malawi, citizens are considered as having attempted to formalize when they sign up to begin paying city rates in exchange for receiving waste collection service. Attempt to formalize is a binary outcome.

7.2 Formalization

Formalization takes place when citizens complete the process of registering their business (Brazil, Colombia, and Nigeria), acquiring a property title (DR Congo), acquiring a formal water connection (India), and make their initial payments of city rates and received waste collection service by the city (Malawi). Studies will make use of administrative data on formalization when such data is available, or self-reported validated data. Formalization is a binary outcome.

7.3 Access to public services

Researchers will measure access to public services. In most of the studies, formalization is tightly related to the provision of specific services, like waste collection in Malawi, tubed water in India, social security in Brazil, and public education in Nigeria. In other cases, formalization could be related to access to public services in a more indirect way. For example in DR Congo, formalization of property rights is a private benefit, which nonetheless derives from a government service (i.e. honoring formal rights in judicial settings). Researchers will measure citizens access to the most relevant public services in their surveys. Access to the most relevant public service is a binary outcome.

In some cases, although citizens may have gained access to a public service, they may not see that service materialize in the short term. For example, in Brazil citizens may have access to social security, but they will receive a pension from the state once they reach the retirement age. In DR Congo, citizens may acquire a property title. Yet, whether a property title protects a citizen's property is only revealed in particular circumstances, for example in the case of a dispute. To account for this, researchers will also measure in their surveys perceptions of access to public services.

In addition to the above, we also expect that formalization could lead to familiarity with state agencies that spills over into the ability to access other services. Researchers will measure access to a set of public services. From these questions, we will construct an index of access to public services.

7.4 Tax compliance

Researchers will measure if subjects pay their taxes at endline. The type of taxes varies across studies. In Brazil, researchers will measure payment of a flat rate fee that combines a social security contribution and all industry sector taxes. In Colombia, researchers will focus on payment of the commerce tax. In DR Congo, researchers will measure payments of property taxes. In Malawi, researchers will measure payment of the city-rate (after the encouragement intervention and waste collection). In Nigeria, researchers will measure payment of personal income tax. And, in India, researchers will measure payment of water consumption fees. Researchers will collect data on tax compliance from administrative sources when possible, or else from survey data.

Like access to public services, we expect that the common treatment arm could have spillover effects to other types of taxes. Hence, researchers will measure payment of other taxes at endline, and will create an index of tax compliance.

7.5 Tax morale

Researchers will measure in their surveys whether respondents believe that citizens should always pay taxes, or should only pay taxes if they agree with the government (probing intensity).

8 Outcome measures in the book-length project

8.1 Knowledge and Perceptions about Taxation

Researchers will also measure perceptions of prevalence of non-compliance with taxation, perceptions of probability of being caught when not paying taxes, and perceptions about the severity of the punishment when caught not paying taxes. From these items, standardized indexes of related outcomes will be created.

8.2 Attitudinal Outcomes

Researchers will measure various attitudes towards the government including perceptions of state capacity, trust in government, perceptions of public corruption, support for ruling

party or general approval of the government, perceptions of attribution of public services, among others. When appropriate, standardized indexes of related outcomes will be created.

8.3 Civic and Political Participation

Researchers will measure participants' self-reported civic and political participation, as well as citizens' engagement with the state to access services. Standardized indexes of related outcomes will be created.

9 Analysis details

Table 2 lists our outcomes of interest. We have grouped outcomes into categories. Within each category, we list the indicators we will use to measure each outcome. To adjust for multiple comparisons, we will follow the procedure used in Burde, Middleton and Samii (2016). That is, within each category of outcomes, we will use the Benjamini-Hochberg correction to constrain the false discovery rate at 0.05.

9.1 Creating indexes

As mentioned before, when appropriate, we will combine survey items to form indexes. Table 2 lists indexes and their corresponding components. To create an index we use the following procedure:

- Code all components so that higher values indicate better outcomes.
- At the respondent level, for each component, we calculate a z score by standardizing using the country control mean and standard deviation. For example, let a family of outcomes l have a component k for which we have a measure $Y_{i,j,l,k}$ where i references the individual and j the country. Then:

$$z_{i,j,l,k} = \frac{Y_{i,j,l,k} - \overline{Y_{j,l,k}|Z=0}}{SD(Y_{j,l,k}|Z=0)} \quad (1)$$

- Next, we average all the z-scores for an individual i to get $z_{i,j,l}$ (or a single z score).
- Finally, we standardize the single z-score by the country control mean and standard deviation:

$$z_{i,j,l}^0 = \frac{z_{i,j,l} - \overline{z_{i,j,l}|Z=0}}{SD(z_{i,j,l}|Z=0)} \quad (2)$$

9.2 Primary Meta-analysis Strategy

We will estimate the common treatment arm impact on outcome Y by calculating the intent-to-treat (ITT) meta-estimate of assignment to treatment in the following way:

First, for each study, we will estimate the effects of the common treatment arm on Y via ordinary least squares (OLS):

$$Y_{i,b} = \alpha + \beta_1 Treatment_{i,b} + \theta_b + \epsilon_{i,b} \quad (3)$$

where i is the individual (or firm), b is the block (for the studies which employ block randomization), $Treatment$ is an indicator for random assignment to the treatment arms. We will control for any strata θ used in the individual studies to perform block randomization. If probability of treatment varies by block, we will also apply inverse probability weights (as suggested by Gerber and Green 2012). Stata-type robust standard errors will be clustered at the level of randomization.

Next, to increase the precision of our statistical inferences, we will add baseline covariates to equation (1). Following Annan, Boyer, Cooper, Heise, and Levy Paluck (2019), we will use an adaptive lasso to identify prognostic covariates to be included in our specification.

Finally, we will use a random effects model to aggregate the six country-level estimates.

9.3 Secondary Meta-analysis Strategy

We will report the Complier Average Causal Effect. For each study, we will use a two stage least squares regression, in which we use assignment to treatment as an instrument for formalization. As before, we will include block fixed effects for studies with block randomization, and we will apply IPW when probability of treatment varies by block. Stata-type robust standard errors will be clustered at the level of randomization. Then, we will use a random effects model to aggregate the six country-level estimates.

Next, to increase the precision of our statistical inferences, we will add baseline covariates to the two stage least squares regressions. Following Annan et al. (2019), we will use an adaptive lasso to identify prognostic covariates to be included in our specification.

To assess whether our results are robust to departures from the strict exogeneity assumption, we will derive analytic bounds for our estimations (Conley et al. 2012; Nevo and Rosen 2012).

10 Potential problems and strategies to address them

10.1 Randomization Check

For each study, we will assess the covariate balance produced by randomization with a randomization check following Hansen and Bowers (2008). If the omnibus test returns a $p < .05$, we will investigate with the relevant PIs the randomization code, data collection, and field work.

10.2 Item-level Missingness on Covariates

We anticipate two types of item-level missing data on covariates: (i) data coded as -999 if the subject refused to answer or no response was recorded; and -999 if she replied “Don’t Know/ Can’t Say”. Some questions are not applicable to certain respondents. These are coded as -998 . We do not consider these cases as missing data. None of the rules laid out below apply to the -998 .

As a reminder, we will estimate the effects of the common treatment arm without and with covariates. In the latter case, to avoid losing observations due to item nonresponse, when a covariate value is missing, we will assign the country mean and include an indicator for missingness on that item.

For baseline indexes, we will regress each index component on the rest, and use the predicted values to fill in missing values.

10.3 Item-level Missingness on Outcomes

We expect that some outcome values will be missing. For single-item outcomes, we will treat missingness as a case of attrition. That is, we will test if there is a relationship between missing outcomes and treatment assignment. As in the primary analysis strategy, we will take into account the design of the studies by including block fixed effects, and applying IPW, when appropriate.

If we find that there is no systematic relation between missing outcomes, and treatment assignment, we will simply drop the observations with the missing outcomes. On the other hand, if we find that there is asymmetric missingness, we will report the extreme value bounds following Gerber and Green (2012, p.226).

For outcome indexes, we will assess if there is a treatment-attrition relationship for each index component. If not, then we regress each index component on the rest, and use the predicted values to fill in missing values. If we find that there is a asymmetric missingness in the index components, then we will report extreme value bounds following Gerber and Green (2012, p.226). We will compute the bounds by assigning minimum and maximum values for all components with missing values.

10.4 Item-level Missingness on assignment to treatment and treatment delivered

If Z is missing, we drop the respondent. If D is missing, we will code the observation conservatively as a contact, since this won't affect the ITT and will lead to an underestimate of the CACE. If any such instance exists, we will re-contact the field team and seek an explanation.

10.5 Different specification choices

In addition to the two specifications included in this document (i.e. equation (1), and equation (1) plus baseline covariates), other model variations might be estimated to deal with unforeseen circumstances, or to deal with design issues that we have not fully captured in this document. We will report deviations from this pre-analysis plan in robustness checks, and we will also conduct a specification curve analysis to show how point estimates of the estimands of interest change with different specification choices.

11 Moderators

We expect that some baseline covariates will act as moderators. That is, some baseline factors are not likely to be affected by the treatments, but they might be responsible

for heterogeneous treatment effects. Specific measures will be harmonized to the extent possible across studies. Heterogeneous treatment effects will be explored in the country-specific papers. Moderators include: benefits of formalization, baseline attitudes towards the government (for example, approval of the government (or ruling party), trust in the government, perceptions of state capacity, perceptions of public corruption), baseline access to public goods, gender, education, income, and resident versus migrant status. In the book project, we will draw from these variables to describe the project sites, and explore the contexts in which informality thrive.

12 Ethics

All projects in the Metaketa will abide by a common set of principles above and beyond minimal requirements (i.e. securing formal IRB approvals, avoiding conflicts of interest, and ensuring all interventions do not violate local laws):

- The EGAP principles on research transparency <http://egap.org/resources/egap-statement-of-principles/>
- Protect staff: Do not put research staff in harm's way.
- Informed consent: Subjects will know that information they receive is provided as part of a research project. Core project data will be publicly available in primary languages at <http://egap.org/research/metaketa/>
- Partnership with local civil society actors to ensure appropriateness of information
- Non-partisan interventions: Only non-partisan information will be provided

13 Timing

Metaketa teams all agree to work according to a common timeframe, to make good faith efforts to complete all interventions and data collection by the agreed upon end date, and to restrict any individual project publication or presentation of results, which draws from the common treatment arm, until submission for publication of the meta-analysis. However, Metaketa members have agreed to consider individual teams' proposals to publish individual papers drawing on alternative treatment arms. For such proposals to move forward, Metaketa members need to reach a positive consensus before the team moves forward with diffusion of results, and submission to academic journals.

References

- Anand, Nikhil. 2011. "Pressure: The Polytechnics of Water Supply in Mumbai." *Cultural Anthropology* 26(4): 542-563.
- Anand, Nikhil. 2014. "Municipal Disconnect: On Abject Water and its Urban Infrastructures" *Ethnography* 13:497.
- Annan, Jeannie, Christopher Boyer, Jasper Cooper, Lori Heise, and Betsy Levy Paluck. 2019. Pre-analysis Plan for: A Pair-Matched Randomized Evaluation of Faith-Based Couples Counselling in Uganda.
- Ayalew Ali, David, Matthew Collin, Klaus Deininger, Stefan Dercon, Justin Sandefur, and Andrew Zeitlin. 2014. "The Price of Empowerment: Experimental Evidence on Land Titling in Tanzania." Center for Global Development Working Paper 369 (May).
- Benhassine, Najy, David McKenzie, Victor Pouliquen, and Massimiliano Santini. 2016. "Can Enhancing the Benefits of Formalization Induce Informal Firms to Become Formal? Experimental Evidence from Benin" (November 29). World Bank Policy Research Working Paper No. 7900.
- Björkman, Lisa. 2015. Pipe Politics, Contested Waters: Embedded Infrastructures of Millennial Mumbai. Duke University Press.
- Borenstein, Michael, Larry V. Hedges, Julian P.T. Higgins, Hannah R. Rothstein. 2009. Introduction to Meta-Analysis. Wiley.
- Bruhn, Miriam, and David McKenzie. 2014. "Entry Regulation and the Formalization of Microenterprises in Developing Countries." *The World Bank Research Observer* 29: 186-201.
- Burde, Dana, Joel A. Middleton and Cyrus Samii. 2016. "Pre-Analysis Plan for Assessment of Learning Outcomes and Social Effects of Community-Based Education in Afghanistan" EGAP registration archive. ID 20160615AB.
- Campos, Francisco, Markus P Goldstein, and David J McKenzie. 2015. "Short-Term Impacts of Formalization Assistance and a Bank Information Session on Business Registration and Access to Finance in Malawi." World Bank Policy Research Working Paper No. 7183 January 1, 2015.
- Conley, Timothy G. , Christian B. Hansen, and Peter E. Rossi. 2012. "Plausibly Exogenous." *The Review of Economics and Statistics*, 94:1, 260-272.
- De Giorgi, Giacomo; Rahman, Aminur. 2013. "SME Registration Evidence from a Randomized Controlled Trial in Bangladesh." Policy Research Working Paper;No. 6382. World Bank, Washington, DC.
- De Mel, Suresh, David McKenzie, and Christopher Woodruff. 2013. "The Demand for and Consequences of Formalization among Informal Firms in Sri Lanka." *American Economic Journal: Applied Economics* 5: 122-50.
- Galiani, Sebastian, Marcela Melndez, and Camila Navajas Ahumada. 2017. "On the Effect of the Costs of Operating Formally: New Experimental Evidence." *Labour Economics* 45:143157.
- Gerber, A. S., & Green, D. P. 2012. Field experiments: Design, analysis, and interpretation. WW Norton.
- Hansen, B.B. and Bowers, J. 2008. "Covariate Balance in Simple, Stratified and Clustered Comparative Studies," *Statistical Science*, Vol. 23, No. 2, 219236.

- Huntington, Samuel P. 1991. *The Third Wave: Democratization in the Late Twentieth Century*. University of Oklahoma Press.
- Levi, Margaret. 1988. *Of Rule and Revenue*. Berkeley and Los Angeles, CA: University of California Press.
- Levi, Margaret and Audrey Sacks. 2009. "Legitimizing beliefs: Sources and indicators" *Regulation and Governance*, Volume 3, Issue 4 (December) Pages 311-333.
- McKenzie, David and Yaye Seynabou Sakho. 2010. "Does it pay firms to register for taxes? The impact of formality on firm profitability", *Journal of Development Economics*, 2010, vol. 91, issue 1, 15-24.
- Neuwirth, R. 2011. *Stealth of Nations: The Global Rise of the Informal Economy*. New York: Pantheon.
- Nevo, Aviv and Adam M. Rosen. 2012. "Identification with Imperfect Instruments" *The Review of Economics and Statistics*, August, 94(3): 659-671.
- Paler, Laura. 2013. "Keeping the Public Purse: An Experiment in Windfalls, Taxes, and the Incentives to Restrain Government" *American Political Science Review*. Volume 107, Issue 4 November 2013, pp. 706-725.
- Perry, Guillermo, William Maloney, Omar Arias, Pablo Fajnzylber, Andrew Mason and Jaime Saavedra. 2007. "Informality: Exit and Exclusion." World Bank Latin America and Caribbean Studies: World Bank, Washington D.C.
- Ross, Michael. 2004. "Does Taxation Lead to Representation?" *B.J.Pol.S.* 34, 229-249.
- Tyler, Tom. 1990. *Why People Obey the Law*. New Haven, CT: Yale University Press.

Table 1: Description of projects in Metaketa II

	Brazil	Colombia	DR Congo	Malawi	Nigeria	India
Type of formalization	Microenterprise registration	Business registration	Property title	Formal access to public service (waste collection)	Informal vendors registration	Formal access to public service (tubed water)
Location	Rio das Pedras	Soacha	Kananga	Zomba	Lagos	Mumbai
Implementing agency	Sabrae	Local organization	Provincial government	Zomba City government	Centre for Public Policy Alternatives	YUVA and Pani Haq Samiti
Type of implementing agency	Non-profit local organization	Local organization	Local government	Local government	Non-partisan think tank	Local NGOs
Common treatment arm:						
Information about process	Consulting session about MEI program	Consulting session about formalization	Consulting session, and on-site appraisal	Consulting session about the process	Consulting session about e-TCC	Consulting session about the process
Information about costs and benefits	yes	yes	yes	yes	yes	yes
Subsidies/assistance	Offer to formalize in site	Assistance to complete paperwork	Assistance to complete paperwork/discounted rates for obtaining legal tile	Assistance to pay city fees via mobile phones	Offer of assistance to fill in paperwork	Assistance navigating the bureaucratic process
Pure control condition	yes	yes	yes	yes	yes	yes
Unit of randomization	Business	Business	Household	Household	Individual	Shum plots
Method of randomization						
Clustered randomization	no	no	Randomization done remotely by research team	within randomly selected neighborhoods	domly selected shum clusters	domly selected shum clusters
Block randomization	yes	yes	no	no	no	yes
			no	yes	yes	yes

Table 2: Outcomes of interest in meta-analysis paper

Outcome categories	Indicator	Data source
Formalization	Intent to formalize	Administrative/ verifiable data
	Formalization	Administrative data verifiable data
Access to public services	Access to public good most related to the process of formalization	Survey data
	Perceptions of access to public good most related to the process of formalization	Survey data
	Index of access to public goods: -access piped water -access electricity -access trash -access sewage -access transport -access roads -access lights -access health -access education -access pension	Survey data
Tax compliance	Payment of tax or service directly related to the formalization process	Administrative data/ survey data
	Index of payment of taxes that respondent thinks she is required to pay (index components vary by country)	Survey data
Tax Morale	Belief that citizens should always pay taxes/ or should only pay taxes if they agree with government (probing intensity)	Survey data

Table 3: Outcomes of interest in book-length project (in addition to outcomes in Table 2)

Outcome categories	Indicator	Data source
Perceptions of state capacity	Perception of government's capacity /willingness to respond to a hypothetical weather-related emergency	Survey data
Trust in government	Trust in government (offices/agencies relevant to the process of formalization)	Survey data
	Index Trust in government: - trust in national government - trust in provincial government - trust in city government - trust in tax ministry	Survey data
Trust in civil society	Index Trust in civil society: - trust in community leaders - trust in NGOs	Survey data
Perceptions of public corruption	Perception of public corruption Reports of bribe demands	Survey data Survey data
Support for incumbent government	Evaluation of ruling party /current government performance	Survey data
Civic and political participation	Index based on a battery of questions about participation	Survey data
Access to public services	Reliance on alternative providers of services	Survey data
Tax Compliance	Index Perceptions of tax obligations	Survey data
	Index knowledge of gov. responsible for taxes	Survey data
	Index Tax payment solicitations	Survey data
Perceptions non-compliance probability of being caught and punishment	Perceptions of prevalence of non-compliance Perceptions of relevant authority knowing who complies with taxes and who doesn't Perceptions of probability of punishment if caught not paying taxes	Survey data Survey data Survey data