

Asymmetry in Civic Information: An Experiment on Tax Participation among Informal Firms in Togo*

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Abstract

In low-income countries, a substantial wedge exists between firms' knowledge of their tax obligations and what is required by law, especially for microenterprises in the informal sector. We conducted a randomized controlled trial on informal firms in Lomé, Togo, to test whether alleviating asymmetry in civic information improves tax participation. The intervention trained firms on the tax code and the purpose of taxation to capture the reciprocal nature of taxation central to modern states. We find that participation increased among higher revenue firms and dropped more among lower revenue firms, resulting in a net decrease in tax participation. Further evidence suggests that lower-revenue firms expanded their economic activities and that the change in the composition of taxpayers likely led to an increase in total tax revenues, offsetting the decrease in participation. These findings indicate that aggressive tax collection practices toward microenterprises can be counterproductive when reinforcing civic information asymmetry.

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1 Introduction

In low-income countries, governments face considerable information constraints when dealing with micro and informal enterprises (MIE) (La Porta & Shleifer 2016), which often constitute the largest share of economic entities. At the same time, these MIEs typically lack basic knowledge about how the government functions and their obligations to the state (Araujo-Bonjean & Chambas 2004). In particular, MIEs in Togo can regularly pay taxes without an elementary understanding of their tax responsibilities.

Asymmetric civic information can frustrate coordination on rules that are in the “spirit of law”. Firms may not be aware that they are being unfairly taxed and, due to standard informational asymmetries regarding firm activity, tax collectors may, unwittingly or strategically, enforce tax obligations in ways that are contrary to what is formally required. As a consequence, taxation practices on the ground may potentially be at odds with the tax laws and the fiscal contract (Khan, Khwaja & Olken 2016, Gordon & Li 2009). This disconnect can hinder economic activity and thwart crucial gains in state legitimacy that comes from tax participation (Weigel 2020).

Using a randomly selected sample of 383 MIEs in Lomé, Togo, we randomly assigned an intervention that trained firm owners about their tax obligations and the benefits of paying taxes to 191 firms, with the remaining MIEs serving as the control group. The intervention should improve firms’ knowledge about the fiscal contract. Unlike in developed countries, where tax avoidance (intensive margin) is the central issue, tax evasion (extensive margin) is also a critical issue in many

developing countries with large informal sectors, and participation depends upon how visibly the firms choose to operate. Accordingly, we assessed the effect of the intervention on tax participation and economic activities shortly after the close of the tax season.

As a result of the intervention, we first show that firms in the treatment group are more likely to possess correct knowledge about their tax obligations, both concerning which types of taxes they are responsible for and what constitutes their obligation.

Second, we find treatment effects ranging from 9.2 to 13.7 percentage points (or 22% -33% relative to the control mean). The effects are statistically significant at the 10% to 1% levels across specifications, adding various baseline control variables. Further analysis shows evidence of positive sorting. Firms with relatively low revenue at baseline, determined based on revenue as a percentage of the value of total assets, are less likely to pay taxes in the treatment group, even conditioned on a tax collector's visit. This positive sorting along firms' revenues matches the spirit of the law since what firms should be paying is closer to what they pay.

Third, we find that the intervention increased the economic activities of the MIEs. The intervention led to an increase of 0.98 open orders measured on the survey days (a 58% increase relative to the control mean). The point estimates on the estimated firm revenue are large (19%) but not statistically significant.

Fourth, in terms of average tax revenue paid (taken over all firms) and presumably collected by the tax collector, we relied on self-reported data and found a positive effect of \$4.49, representing a 128% increase relative to the control mean (although there are a high number of missing values on this variable). The increase in tax

revenue collected is thus suggestive evidence that the distribution of taxpayers shifted toward higher revenue firms. Thus, even though tax participation has decreased, tax revenue likely has increased in the aggregate.

What explains these changes in tax payment behavior? Although a fine establishment of the mechanism is beyond this study design, we conduct an exploratory investigation and discuss several hypotheses, starting with our central hypothesis of empowerment. To enrich this exploratory analysis, we also conducted in-depth, post-survey qualitative interviews and a short quantitative survey to document the process of change that treated firms underwent.

To assess the empowerment story, we first measure firms' take-up of the intervention by verifying their knowledge about the fiscal contract several months after the training. We obtain a local average treatment effect on the treated by instrumenting the knowledge variable with treatment assignment. The 2SLS estimate's magnitude ranges from 0.55-0.78 and is precisely estimated, suggesting a large change in tax payment from participation to non-participation for those whose knowledge of the fiscal contract changed in response to the intervention. Second, when a tax official visits firms, the treatment group is much less likely to pay taxes than the control group, suggesting increased bargaining power due to better knowledge of the social contract and tax code. The qualitative post-survey interviews that we conducted of treated firms confirm that a common explanation for not paying taxes is that "when firms don't make money, they don't pay taxes." This sentiment stands in stark contrast to our pre-intervention survey that finds two-thirds of firms considered taxes as somewhat or very difficult to predict in advance and, correspondingly, we also find

the treatment effect is stronger for those firms that felt taxes were unpredictable at baseline.

With these findings, we contribute to two strands of literature. First, a growing number of papers have investigated the role of information and transparency using randomized controlled trials (Kosack & Fung 2014). However, relatively few of these papers examine taxation, a key aspect of building state capacity, and none, to our knowledge, has given attention to the tax behavior of MIEs, the largest category of firms, also often assumed not to be paying taxes altogether because informality is conflated with evasion (Besley & Persson 2013, Joshi & Ayee 2008). Our findings indicate that civic information on the fiscal contract plays an essential role in the tax outcomes of MIEs.

We argue that the focus on MIEs allows us to better study knowledge as power. The literature typically studies providing information to citizens about their entitlements (e.g., Banerjee et al. 2015, Reinikka and Svensson 2004). However, knowledge may not empower citizens if there are power imbalances to overcome when citizens make demands. Instead, we provide training about firm owners' obligations to the state. Since the fulfillment of obligations depends upon the agency and interest of the firm owners, knowledge can more easily transform into empowerment. In a related paper to ours, working at a more aggregate level, Timmons and Garfias (2015) show a strong relationship between information revelation and tax mobilization by examining how making municipal budget audits public affects tax compliance among citizens. Since information about audits is still essentially information about entitlements (i.e., citizens are entitled to a government free from corruption), we build on

this literature by showing an effect of information about obligations to the state.

Second, we contribute to the growing literature on tax mobilization in developing countries. Within this literature, there are several interesting lab experiments, and a few field experiments on the determinants of tax compliance (see (Mascagni 2016) for a review). Most tax experiments focus on the deterrence of tax avoidance. In this literature, information treatments consist of the likelihood of audit or evidence of evasion. These studies find null or positive treatment effects (Shimeles, Gurara & Woldeyes 2017, Mascagni, Nell & Monkam 2017, Carrillo, Pomeranz & Singhal 2017, Pomeranz 2015, Brockmeyer, Smith, Hernandez & Kettle 2019)¹. The field studies, both experimental and non-experimental are scant, especially in Africa. We contribute to filling that gap, with an added emphasis on the extensive margin of tax compliance relevant to the adoption of the fiscal contract. Bergeron et al. (2021) also examine the extensive margin of property tax participation of registered property owners who are already implicitly adopting the fiscal contract. In contrast, we do not rely on administrative data to identify firms and, consequently, are not bound by the informational constraints of the state. When there is low state capacity, Balan et al. (forthcoming) demonstrate that informational constraints on state actors are severe, implying that those who are identifiable by the state are likely to be different from those who are not. Indeed, we show the extensive margin is sensitive to civic information about the fiscal contract².

¹A related strand of literature assesses the potential of the new information technologies to enhance compliance (Awasthi and Bayraktar 2014, Ali et al. 2015, Okunogbe and Pouliquen 2017)

²A related margin for firms in the informal sector is informal taxation by non-state actors (Jibao, Prichard & van den Boogaard 2017, Sánchez de la Sierra 2020).

Our examination of the extensive margin reveals an empowerment effect of information which is vital for the functioning of the fiscal contract (Ali et al. 2014 , Gauthier and Reinikka 2006 , Khan et al. 2016). A standard assumption in the literature is that civic information improves with greater participation. In our context where state capacity is low, this assumption may not be appropriate. Without such a feedback loop, the empowerment effect serves to discipline the state’s discretionary control. Even though we find a reduction in tax participation, the intervention improves institutional congruence, promoting the legitimacy of the fiscal contract, and is accompanied by positive sorting and increased tax mobilization. In our view, the results show that firms willingly adopt the fiscal contract, despite the limited level of formalization. These findings demonstrate we need to reevaluate the extent to which the informal sector is an obstacle to development.

The rest of the paper is organized as follows: in section 2, we present the contextual background and the intervention, section 3 discusses the research design, section 4 presents the main findings of the study, and section 5 concludes.

2 Context and description of the intervention

2.1 MIEs and tax compliance in Togo

Togo ranks high among African countries in terms of domestic revenue mobilization, hovering around 20% of the GDP and reaching 22% in 2016³. In the context of Togo,

³A recent rebasing of the economy drove the ratio down. However, a uniform re-basing of the economies in the region will likely leave Togo still among the top-ranked for domestic revenue mobilization

some of the most important challenges remain the tax participation among firms in the informal sector and land and property taxation, both on the extensive and the intensive margins.

The capital city, Lomé, where we conducted this study, is home to over 1.5 million people in the greater metro area, more than a fifth of Togo’s population. The greater Lomé area is also the heart of Togo’s economic activities and home to 63% of firms or economic entities according to the 2018’s first-ever census of firms in Togo, accounting for 71.4% of all employment. Additionally, a staggering 86% of the firms fall under the government’s formal classification of informal firms - which is the population of interest in this study. In Lomé, 79% of the firms in the census are statutorily in the informal sector. In Togo, this sector is required to pay taxes, both national and local taxes.

Firms such as barbers, carpenters, and tailors usually consist of an owner-operator and several apprentices. Despite these firms’ small size, they are visible and long-lived, often requiring a fixed work-space and an inventory of materials, and often employ signage. In addition, many of these firms participate in the formal apparatus of the state, either by paying official fees, obtaining official licenses, or paying taxes on their economic activity.

At the time of this study⁴, the tax law classified all firms with revenues below

⁴As per the current law, following recent reforms after this study were completed, all firms with revenues below 60,000,000 CFA (about \$100,000) are now classified as informal. These firms are further split in two groups for tax purpose. Firms with a revenue above \$50,000 but below \$100,000 are under a regime called "regime déclaratif" and pay flat tax rate as in the previous law (2% and 8%). All firms below 30,000,000 CFA (\$ 50,000) revenue are now under a "Regime forfaitaire" and pay a lump sum that varies by brackets of revenues. For example, all firms operating in the service sector whose revenue fall between \$0 and \$5000 must all pay the same amount of \$40. The amount rises to \$370 if the revenue exceeds \$5000 but below \$10,000. Given the low revenue range

30,000,000CFA (about \$50,000)⁵ under the "informal" regime. These firms are subject to a synthetic tax called "Tax professionnelle Unique (TPU)", a single composite tax that exempts them from most other regular taxes, including the value added tax (VTA). These firms must declare their revenues and pay a tax rate of 2.5% (for firms producing goods and or engaged in trade trades) or 8.5% (for services) are imposed. The government reserves the right to contest the revenue declared and conduct an audit. The minimum amount to be paid cannot be lower than 6,000CFA (\$12) and 12,000CFA (\$24) respectively for goods/trade and services . Since these firms typically do not employ formal accountants (they are not required by law to do so), there is typically a severe informational burden to establish the revenue and apply the tax code.

Governments in West Africa have designed tax policies to accommodate the wide range of firm types that exist. In Ghana, for example, there is a tax stamp system specially designed for firms in the informal sector. In Togo, the statutory laws offer firms to voluntarily declare the amount of revenue and pay the taxes accordingly. The authorities may contest the declaration and conduct an audit or accept it and assess the amount of taxes owed. Through interviews with firms and tax officials in our initial fieldwork, we learned that voluntary disclosure was extremely rare. More common is the practice of a tax collector physically showing up at the firm once a year, and assessing a tax amount based on the firm's assets and size, often determined

of the informal microentreprises, this reform effectively applies a unique lump sum tax to the vast majority of them. It removes much of the discretion of tax collectors to determine the revenue, but it is also now structurally regressive as lower revenue firms pay an equivalent larger share of their revenues.

⁵For simplicity, we use an exchange rate of 500CFA for \$1.

by a glance around the shop. The firm receives a ticket to pay a certain amount, and taxes should be paid at the tax office. Since the formal law requires firms to pay according to their revenue (or profits if firms can submit formal accounting of them), tax officials, due to information constraints, must approximate a firm's revenue in order to adhere to the tax code. Instead of directly asking, tax officials estimate a firm's revenue in ways that lack transparency. They do this, in practice, by calculating expected revenues, relying both on information specific to the firm (visible assets, general firm appearance) and prior information on how similar firms have paid taxes or disclosed revenue. While this method may excel in its pragmatic merit (the value of a firm's assets strongly predicts our measure of revenue), tax collectors could and often do intentionally or unintentionally overcharge firms.

Similarly, firms could engage in several mechanisms of evasion. First, firms might hide assets or work orders or try to appear less busy than they are to record less tax by tax officials. Second, tax collectors can take a bribe from the firm to avoid tax payments. Since these firms have no tax identification number, tax collectors have quite a bit of discretion concerning who owes taxes and what amount to charge. Third, firms may physically hide or close down to avoid tax collector visits. Finally, firms may negotiate with tax collectors to lower their payments, relying on social pressure. Importantly, for any of these mechanisms, firms may or may not know the actual amount of tax they owe and simply try to minimize the tax amount they give to the tax collector. Additionally, having knowledge of what tax amount that they owe is beneficial for their ability to bargain.

2.2 Intervention and theory of change

Our intervention consisted of training about tax obligations and benefits of taxation to firm owners and society at large. The content included was based on the tax code and our interviews with senior tax officials concerning information that they would like these firms to know. Along with their tax obligations, the treatment also consisted of what taxes revenues are used for and the cost of these services. A sample packet is shown in figures 1 and 2. The intervention represents the reciprocal nature of taxation and the fiscal contract.

The research was based at the Center for Research and Opinion Polls, a research think tank based in Lome. A team of enumerators composed of university students received comprehensive training on the tax code. All enumerators sent to the field had to pass an exam on the tax code, which consisted of working through different firm-specific tax scenarios. Both national and local taxes were covered.

The treatment group was surveyed at baseline and endline. In between surveys, these firms received an information packet and consultation. We then followed up 30 days later for individual consultations on the tax code and its application to the firm. In addition, firms were asked to answer a few questions about their firm and depending on their response, the enumerators provided the relevant information if there were any inconsistencies with the state's and the firm's understanding of tax compliance. The control group was surveyed at baseline and endline with no interaction ex interim. For reference, the timing of the experiment is summarized in Figure 3.

Asymmetry in civic information is an important policy concern for low-income

countries. Several scholars and policymakers have pushed for greater transparency in governance in order to improve service delivery as well as to empower citizens.⁶ Firms operating in the informal sector are likely to suffer most from asymmetric civic information since there is less accountability for acting in ignorance of this information. Moreover, the majority of the firms that we study are located at the extensive margin of taxation, where an improvement in information could dramatically influence overall tax compliance (as opposed to larger firms that, because of their size, are easy to target, although they may evade taxes), both for the worse or better. Indeed, when firms' decisions are sensitive to civic information, a vicious or virtuous cycle could emerge depending upon whether better information discourages or empowers firms to comply with the tax rules.

Since MIEs commonly base their understanding of the tax system on their perceptions of and experiences with collection practices, asymmetric civic information can present challenges to tax mobilization when it distorts the social contract. For example, if the tax law establishes a relationship between a firm's profits and the tax that they owe, but tax collectors assess tax obligations based on the firm's fixed assets, firms may be more likely to hide when they experience negative shocks or choose not to increase economic activity when they experience positive shocks.

From a theoretical standpoint, one would expect that the provision of knowledge of taxation and the fiscal contract would reduce expectation mismatches, lead to better experiences with tax officials and more positive attitudes toward tax mobilization. These factors should lead to greater tax participation and lower the cost

⁶The World Development Report, "Making Services Work for Poor People" calls for greater transparency (World Bank 2004).

of collection. However, in an environment where the informational gap arises from institutional weaknesses, and not merely rational ignorance, the effect of bridging the informational gap may reduce tax participation in at least two important ways: i) by empowering entrepreneurs to refuse to pay if the requested amount is much farther than what they legally owe and ii) by making tax collectors more selective in the firms they target, reducing the likelihood of a tax audit for smaller revenue firms.

Then, improvement in civic information should result in better sorting of firms by profits, proxied by revenues, into tax payment. Firms with very low revenue (and likely close to zero profits) that would likely have only had experience paying taxes due to being approached by a tax collector are empowered to refuse to pay tax amounts established by tax collectors' informal approximations of revenue based on observable assets. Firms with high revenue that are likely highly visible and probably were already paying taxes may also be empowered to voluntarily comply or self-report because this would reduce the uncertainty in their tax obligations. If the low revenue group is more numerous and/or has worse knowledge, we could see a negative treatment effect on average, even though positive sorting may improve tax mobilization overall.

Finally, we note that the intervention merely improves civic knowledge and does not and cannot solve the other informational asymmetries between firms and the state. In particular, firms still have considerable control due to their private information on their revenue and profit.

3 Research design

3.1 Sampling and randomization of treatment assignment

At the time of this study, Togo, like most African countries, did not have a census of enterprises to serve as a sampling frame. Therefore, we first performed a survey of a random and representative sample of 643 micro and informal firms located in greater Lomé. The survey questionnaire contained questions on firm characteristics such as a complete description of assets, monthly expenditures and revenue, participation in, and experiences with the local and national tax system. Additional components of the survey concern the firm owner and are questions on civic engagement and political participation.

Since exact addresses of firms were not known in advance, we adopted a sampling technique employed by Afrobarometer across Africa, including several times in Togo. Using census tracts as enumeration areas, we randomly selected enumerations areas and starting points on the map (see Figure 4). Upon arriving at the starting point, the enumerators start from the nearest junction and select 5 businesses in each direction by counting businesses on both sides of the street and selecting every fifth business they encounter. This method has consistently produced a representative sample for Afrobarometer.

We used this database as our sampling frame for this experiment. We refine the sample by dropping ambulant vendors whose tracking in an experiment is nearly impossible. We also dropped highly underrepresented types of firms to generate more homogeneity among participant firms. Of the 499 remaining firms, we randomly

selected 424 firms and then randomly assigned half to treatment and half to control. Back in the field about ten months later, enumerators were able to track a total of 383 out of 424 firms to start the experiment – a 90% rate. Missing firms include closed firms, wrong records of addresses at the baseline as most of these firms do not have an official address, firms with temporary locations that may have moved, etc. None of the firms refused to participate once located.⁷ The treatment group included 191 firms, and the control group consisted of 192 firms.

The original sample of 424 can identify a minimum effect size of 13.5 percentage points (from a baseline outcome of 41% tax participation), with a statistical power of 80% at a 5% significance level. With the final sample of 383 observations, the minimum effect size increases slightly to 14.3 percentage points. The baseline data allows us to gain additional statistical power by adding control variables, including the baseline values of the outcome variable. Assuming that these control variables can explain up to 30% of the outcome variable’s variation, the study will be adequately powered to identify a minimum effect size of 12 percentage points.

3.2 Data

The analysis relies on survey data collected at baseline and endline in both treatment and control firms. To maximize tracking at the endline, we further strengthened the tracking mechanism at the baseline, with additional phone records of several people related to the firm and detailed descriptions of the location by the enumerators. The

⁷Two of the 212 firms assigned to the control group made initial contact with the enumerators and tried to find time to complete the survey but were only able to complete a minor portion of the endline survey.

data, both at the baseline and endline, included detailed questions about the firms and owners. We collected information about their knowledge of tax rules, tax participation, and economic activities. We also collected information about implementation in the treatment firms only. All the training of firm owners and data collection were conducted by qualified enumerators who passed a comprehensive exam on the tax code.

At the endline, we successfully reached all 383 firms, 192 in the control group and 191 in the treatment group.

Since our primary focus is tax participation at the extensive margin, the main outcome of interest is a dummy variable indicating whether the firm has paid taxes during the last tax season. This variable tracks the extensive margin of tax compliance since all firms in our survey must pay taxes. An obvious additional outcome is the tax amount paid. We collected self-reported amounts paid, albeit with significant missing values in part because we required evidence of payment in the form of a receipt. We will nevertheless investigate the data we have on hand, assigning zero to non-payment.

The next set of outcome variables concern economic activity directly. The most reliable and verifiable measure of economic activities is a measure of the open orders on the day of the survey. Additionally, since we suspect that our treatment will have differential impacts on high and low revenue firms, this measure offers a revenue-neutral measure of economic activity. There is sufficient homogeneity in the types of firms; most are in the service sector, which makes this measure a meaningful one.

We also look at firms' estimated revenue during the month prior to the survey.

Direct questions about revenue, especially for firms who may not be currently paying taxes, can be sensitive questions, and firms may elect not to answer such questions. To estimate firms' revenues, we first ask questions about the number of orders in the past month, the value of the minimum and maximum order, and the value of the two most recent orders and use this information to estimate a firm's revenue. These questions also give us an idea about revenue level and variability. Second, we replicate what the tax authorities do informally by calculating the expected revenue from a regression of the first measure of revenue on the firm's assets.

3.3 Summary statistics and identification

According to the initial sampling frame survey, only 8% of firms are officially registered, while 41% pay taxes. While there are a variety of firms in the sample, they are primarily tailors or embroidery (40%) and barbers or hairdressers (37%). Firms are more likely to participate in the tax system if: firm owner is not poor (self-assessed), has a higher number of dependents, is male, the firm has a higher value of assets, and firm has electricity. Surprisingly, years of education are not related. Nearly all firm owners, however, say that firms should pay taxes to the state (Blimpo and Castañeda Dower 2017).

We asked firms which factors determine the amount of tax you pay? The factors we asked about were revenue, profits, assets, the number of apprentices, the firm's appearance, ethnicity, political connections, or others. Nearly half (49%) answered that they didn't know whether a particular factor mattered at least once and about a quarter of firms (24%) answered that they didn't know for all factors. Figure 5

reports on firm owners’ knowledge. While about a third of firms identify revenue as a condition, overwhelmingly, the assets and firm appearance are mentioned.⁸

Under asymmetric civic information, greater participation can lead to greater levels of ambiguity. In table 1, using the baseline survey, we explore whether firms that pay taxes are associated with greater amounts of misinformation. Each column represents whether the firm perceives the factor affects the evaluation of their tax obligations. Under symmetric civic information, we would expect greater participation to be associated with better information. In this case, paying taxes at baseline should be positively associated with the dependent variables in columns (1) and (2) and negatively associated with all others. Instead, we see a much different pattern. Not only are the associations positive for columns (3) - (6), the magnitudes of the associations are largest for column (3), which is the factor most likely to contribute to an evaluation in the informal practice described to us by tax officials.

3.4 Empirical strategy

The main identifying assumption is that the expected net effect of both unobserved and observable variables (other than treatment assignment) is statistically similar in the treatment and control groups. This is achieved through the randomization process that we controlled. As is commonly done, we verify that the observable characteristics of the firms and firm owners are balanced. In table 2, we present results from running a regression of treatment status (an indicator variable taking

⁸Thirty-three firms select the option “Other” and specified factors that we had not anticipated. Among those firms, 60% identified the rental cost of the firm’s facility as the main factor, and the rest were split among several other factors, including the prominence of the neighborhood where the firm is located or how busy the firm’s surroundings appear to be.

a value of one if received the intervention and participated in the endline survey and zero if assigned to the control group and participated in the endline survey) on several firms and firm owners' characteristics measured at baseline. We then run a simultaneous test of the null that these variables (or subgroups of them) can be excluded to assess any systematic correlation with treatment status. According to the F-statistics in columns (1)-(4), which correspond to simultaneous tests for firm characteristics (1), firm owner characteristics (2), both (3) and both conditional on firm type fixed effects (4), we fail to reject the null that these variables are simultaneously orthogonal to the treatment assignment variable - indicating that the randomization was successful and that the two groups are ex-ante balanced based on observable characteristics, and balanced in expectation on unobserved variables. A simple comparison of means between the two groups at the endline can therefore be interpreted as causal.

We estimate the following regression to understand whether the treatment affects tax participation:

$$y_i = \alpha + \pi_{ITT} * Treatment_i + \epsilon_i \quad (1)$$

where y is the outcome of interest (e.g., an indicator that a firm currently pays taxes.). $Treatment_i$ is determined by randomized assignment, and hence π is an estimate of the intent-to-treat effect. For the benchmark estimate, we use the difference in means. We also report the ANCOVA estimator with baseline tax participation. To improve precision, we also include a set of control variables, which are a firm's size and value of assets, whether a firm has electricity and signage, the firm's num-

ber of apprentices, an indicator of firm owner having completed primary education, firm owner’s age, marital status and number of dependents and the firm owner’s self-reported poverty status and firm-type fixed effects.

To verify that this effect operates through changes in knowledge of the fiscal contract, we construct a measure of take-up of the intervention. To do this, we create a set of indicator variables that assign a value of one to a firm if the firm takes up knowledge of the social contract and zero otherwise. The first indicator is based on knowledge of the tax code and is coded as one when the firm gives the correct determinants according to the tax law at the endline survey (Obligation). Since a firm that demonstrates that it knows the fiscal contract is likely to differ systematically from firms that do not, we instrument for take-up with treatment assignment to get a local average treatment effect. The second indicator is whether the firm specifies an opinion about whether taxes are being put to good use. Firms can answer “Yes”, “No” or “I don’t know.” The variable then indicates the firms that do not answer “I don’t know” since firms with an opinion are more likely to view taxation as a fiscal contract (Accountability). The final indicator is the product of the two previous indicators and represents whether the firm understands the reciprocal nature of taxation. The two underlying indicators are positively correlated, which is reassuring and suggestive that the treatment effectively communicated both sides of the fiscal contract. Thus, we estimate the following regression to understand whether knowledge affects tax participation:

$$y_i = \alpha_2 + \pi_{LATE} * Knowledge_i + \eta_2 PaysTaxPre_i + u_i \quad (2)$$

$$info_i = \alpha_1 + \psi Treatment_i + \eta_1 PaysTaxPre_i + v_i \quad (3)$$

where y is an indicator that a firm currently pays taxes, we report results with the ANCOVA estimator with baseline tax participation.

Finally, we explore channels of influence through heterogeneity analysis. We take a factor X , measured at baseline, and interact it with the treatment variable to give the following specification:

$$y_i = \alpha + \pi * Treatment_i + X_i\beta + \gamma * Treatment_i * X_i + \epsilon_i \quad (4)$$

where the marginal effect of the treatment is still the quantitative object of interest. We are interested in how this effect varies at different values of the factor X .

4 Results

4.1 Main results

4.1.1 Has the intervention improved participants' tax knowledge?

We begin by examining whether the intervention in the treatment group translated into better knowledge about taxation. Table 3 reports large gains in knowledge of the tax code by estimating equation (2) where the dependent variables are various measures of the knowledge of the tax code. The percentage of firms who can correctly designate two municipal and national taxes increased respectively by 23 and 24 percentage points from low means in the control groups of respectively seven and three percent (columns I and II). The first two of these measures of firms' knowledge are particularly relevant because the authorities were concerned, among other things, that firms were confused about which taxes to pay and to whom. Specifically, we asked which two (out of five options) of the following taxes are collected by the Mayor's office (Revenue Authority). As we see above, the treatment had an impact on selecting the correct answer for both types of taxes, but we also see partial knowledge increase: for the Mayor's office, 97% of the treatment group had at least one correct response, compared to 53% with at least one correct response in the control group; and for the Revenue Authority taxes, we observe a similar pattern in the treatment group, 82% had at least one correct response, compared to only 22% with at least one correct response in the control group. These differences are striking evidence that the treatment effectively transmitted knowledge that tax authorities themselves wanted firms to know. In Column III, we report knowledge

of an important tax for the visibility of the firm. While 72% of the control group firms knew about signage taxes, the treatment increased that number further by 24 percentage points.

In Column IV of Table 3, we show the results for the main emphasis of the intervention, the share of firms who can identify the statutory factors that should determine their tax burden. As discussed above, we measure this by whether the firm possesses correct knowledge about the determinants of their tax obligations. The treatment increased the firm owners' knowledge by 17 percentage points or a 74% increase relative to the control mean. In Column V, we examine another relevant margin: the confidence that firms have in their knowledge of the tax system, whether or not this information is correct. We can measure this by whether they report "I don't know" when asked about which factors determine their tax obligations. The share of firms affirming at least one factor, whether it mattered formally or not, was higher in the treatment group (76% compared to 67% in the control group). Less reluctance to answer in the treatment group could signal more confidence in understanding how taxation works. In Column VI, we find that the treatment makes firms more likely to have an opinion, good or bad, about how the government uses tax revenue. This is our measure of accountability since firms connect taxation with their assessment of government performance. Finally, in Column VII, we show the treatment increased knowledge of both aspects of the fiscal contract. Taken together, we find solid grounds for the internal validity of the study and demonstrate the sustained take-up of our intervention with treatment firms having dramatically better knowledge and confidence even several months afterward.

4.1.2 Has the intervention impacted tax participation?

Firms who received the treatment are nine percentage points (a 23% decrease from the baseline) less likely to pay taxes than firms who did not (statistically significantly different from zero at the 10% level), reported in column (1) of table 4. Around 40% of firms in the control group pay taxes at endline, which is in line with the 41% of firms that pay at baseline. There is considerable scope for factors to influence the extensive margin of tax compliance in this context, and information is one of these factors.⁹ Thus, while this strong response is not surprising, the direction of the effect may be.

In the remaining specifications, we add several baseline values of control variables to increase the precision of the estimates. In column (2), we add the baseline status of tax participation; we include owner characteristics (column 3), then firm characteristics (column 4), and finally firm-type fixed effects (column 5). As a result, the relationship becomes more precisely estimated. In all specifications, the point estimate is stable at 13.7 percentage points (or 33%, statistically significantly different from zero at the 1% level).

In table 5, we report the 2SLS estimates (equations (2) and (3)), tying further the impact to the intervention. The first-stage relationship is strong and has the correct sign for each of our indicators of take-up. Again, we see that better knowledge of the fiscal contract leads to less tax participation. The magnitude of the coefficient is large, suggesting that the tax participation decision is highly sensitive for those

⁹This sensitivity is especially important considering how expectations can give rise to Pareto-ranked multiple equilibria in this setting, i.e., views about legitimacy affect tax compliance, but tax base affects views about legitimacy.

firms whose information sets changed due to our treatment.

Another important evidence of the treatment comes from firms' knowledge of the taxes that they pay conditional on paying taxes. Since all firms are responsible for paying TPU, we present in the table 6 the percentage of firms that report paying the TPU by treatment status. What is striking is the majority (60%) of the control group that reports paying taxes don't know if they are paying TPU. In contrast, most (70%) of the firms that report paying taxes in the treatment group report paying the TPU. This pattern is broadly similar across the various other taxes that we asked firms about. Thus, tax participation and knowledge are now better aligned in the treatment group than in the control group that suffers from asymmetric civic information.

This evidence is supported by an investigation of treatment effect heterogeneity with respect to how unpredictable a firm estimates taxes are to be pre-treatment. In Figure 7, we see that those firms that found taxes unpredictable at baseline are less likely to participate in paying taxes at endline (statistically significant at the 5% level). More generally, this result speaks to the importance of knowledge since those firms that would have benefited most from the treatment are likely the ones who perceived taxes to be unpredictable at baseline.

4.1.3 Which firms pay taxes and which firms do not?

We argue that in this economic environment with costly revenue verification, some firms have expected tax revenues that are less than the cost of verification. In such situations, tax enforcement is not credible and possibly would operate counter to the

spirit of the law. Thus, a negative relationship between tax participation and tax knowledge is consistent with the spirit of the law being implemented. Firms with low revenues feel empowered to report that they should not pay, and firms with high revenues are empowered to meet their tax obligation. Even though tax participation declines, one could argue that tax compliance increases since the difference between what is owed and what is paid likely decreases. Essentially, better knowledge of the fiscal contract leads to better sorting firms who pay taxes, and overall tax revenue could increase.

We present two pieces of evidence that this sorting occurs. First, we compare the tax behavior of large revenue firms to small revenue firms. As Figure 6 shows, in the control group, the proportion of firms that pay taxes is statistically similar for firms with revenue in the upper quartile of the revenue distribution than for those in the bottom three quartiles. In contrast, the upper quartile-revenue firms are more likely to pay taxes than firms in the bottom three quartiles in the treatment group.

Second, a firm with a low ratio of baseline revenue as a share of the value of their assets is likely to have been taxed unfairly and experienced the wedge between formal obligations and informal practices. Figure 8 demonstrates that, at a high ratio, the treatment group firms behave statistically similar to control group firms, whereas, at a low ratio, treatment group firms are less likely to pay taxes than control group firms, suggesting that firms are now positively sorting into tax participation.

4.1.4 Has the intervention impacted firms' activities and tax revenues?

Better knowledge of the fiscal contract should empower firms to operate more openly and form more reliable expectations about their business environment. The intervention should then reduce hiding and increase economic activities.

We find that the intervention increased the firm's economic activities significantly. The number of open orders on the survey day increased by 0.84 - 0.98 relative to 1.7 active open orders on average in the control group, representing up to a 58% increase. The point estimates on the total revenue for the month prior to the survey date are also large and positive but not statistically significant (Table 7, columns 1 and 2). While the revenue report is subject to recall issues and inaccuracies, the measure of open, active orders is much more precise and provides strong evidence that the intervention enhanced economic activities among treated firms. The reduced need to take avoidance measures is one likely channel through which this effect operates. Interestingly, paying taxes at baseline is also associated with greater economic activity, indicating either that these more visible firms were targeted in the past or that there is a positive feedback loop between tax participation and operating more openly.

We collected data on the amount of taxes paid. However, as mentioned earlier, this measure was subject to many missing values, likely because of several reasons, including the fact that we required enumerator confirmation through receipts. In addition, since most firms did not pay taxes, the effective number of non-zero responses is much lower than the actual sample in the study (41% paid taxes at the baseline). Of the 60 firms who showed payment receipts, 27 were in the control group, and 33

were in the treatment group. Therefore, we imputed all non-payment and missing with zeros. With this caveat, Table 7, in columns 5-6, shows that the intervention increased tax payment by 3.52 - 4.45 dollars, more than doubling revenue relative to the control group with a base of 3.5 dollars (accounting for the imputation of zeros for non-payment and missing values).

4.2 Exploratory analysis of mechanisms

As indicated earlier, a granular and causal analysis of the mechanisms at play is beyond the scope of this experimental design. Nevertheless, we conduct exploratory analysis to provide arguments and evidence supporting or refuting several rationalisations of the finding, starting with our main hypothesis of empowerment, which stipulates that participants' improved knowledge of the tax code has increased their bargaining power relative to tax collectors.

4.2.1 Empowerment as channel of the impact

To verify that the treatment did indeed matter, we selected a group of treated firms that had changed their tax behavior after the treatment and performed in-depth, post-survey targeted interviews (14 in total). The interviews confirmed that firms learned important civic information from our intervention. These firms also identified agency costs and informational asymmetries as problems with the current method of taxation. For example, when asked why firms do not currently pay taxes, a common response was that “when firms don’t make money, they don’t pay taxes.” Overall, these interviews supported the view that improved civic information empowered firms

to implement the spirit of the law when the evolved informal practices would have led to a sub-optimal outcome.

We complemented these in-depth interviews with a short quantitative survey of 121 firms (61 control, 60 treated), specifically designed to probe the internal contours of the change initiated by the treatment. Here, our focus is on understanding the process of arrival to a particular state or outcome in contrast to a causal effect that captures to which states or outcomes treated firms have arrived. We grouped our firms into four tax payment paths: those that paid taxes both pre and post, those that did not pay taxes pre or post, and those that paid pre (post) but not both. We then randomly sampled firms in each category, ensuring that control and treatment group firms were selected with equal probability. By design, we compare firms with observationally equivalent behavior to see if undergoing the treatment is associated with different beliefs, perceptions, or interactions with the tax system than the control group¹⁰.

Controlling for tax payment history, treated firms are 25 percentage points more likely (the difference is statistically significant at the 1% level, $p\text{-value} = 0.001$) to have self-declared their taxes (35% of firms report to have self-declared). This is especially true of firms that had regularly paid taxes where most firms report having self-declared. Controlling for tax payment history, treated firms were no more likely to report that they had changed their attitude toward paying taxes in the past two years. However, conditional on a change in attitude (only 30% of firms report having changed their attitude) and controlling for tax payment history, treatment firms are

¹⁰Full results available by request

30 percentage points more likely to say that knowledge of tax rules was the key information that affected this change (the difference is statistically significant at the 5% level, $p\text{-value} = 0.015$).

We asked a hypothetical question concerning how a firm like theirs would be in the position of having not paid taxes: they would have gone into hiding during the tax collection season; they would have declared that they had not earned any revenue, they would have thought that there would be little chance the authorities would come after them, they would have done so inadvertently because they would not have known the rules). Controlling for tax payment history, treated firms are 19 percentage points more likely to report that the hypothetical firm would have declared that they had not earned any revenue (reported 21% of the time) (statistically significant at the 1% level, $p\text{-value} = 0.01$). We also asked about their beliefs about how likely a tax agent would have visited this firm. Controlling for tax payment history, treatment firms are 19 percentage points more likely to say a visit by the tax collector is highly probable (the difference is statistically significant at the 5% level, $p\text{-value} = 0.018$).

The final piece of evidence is presented in Table 8. We see very different behavior in the treatment and control groups when tax officials visit firms. Firms that tax officials visit likely systematically vary from those that are not visited. In the absence of the treatment, the probability of a visit by a tax collector should be independent of treatment assignment, but the treatment itself could affect the likelihood of treatment. That said, the patterns that we observe are instructive. When tax officials visit firms, firms in the treatment group are much less likely to pay taxes

than those in the control group. We also see that the separation that drives the result in figure 6 also occurs for those firms that tax officials do not visit. We see no statistical difference between those firms that pay and don't pay taxes in the control group, whereas treatment group firms that pay taxes have higher revenue, suggesting that treatment group firms are empowered to self-declare when their revenue is high enough.

4.2.2 Contamination as a driver of the effect?

One concern with these results is contamination. Since we cannot control the spread of training given to treatment firms after the intervention, some control firms could have received the information. Given our setting, we think it is unlikely that the treatment and control firms interacted very much. Moreover, in order for contamination to explain away the result, the control group would have to have responded more positively than the treatment group to the treatment, which could happen if a treated firm provided information to a control group firm and was better at communicating it than the treatment intervention on its own. Yet, instead, as we mentioned above, we observe that firms in the control group roughly pay taxes at a similar rate as the overall baseline rate. Firms who were treated may have shared this information within their social network, but there are no obvious connections between their social networks and firms in the control group, besides geographic proximity, which would have had considerable decay due to our sampling strategy since social networks mostly fall on family/village and trade lines.

4.2.3 Increased corrupt practices?

Corrupt practices could explain the negative relationship between the treatment group and tax participation. Tax officials may be purposefully failing to implement the tax code for their betterment, and the treatment could expose this behavior. We analyzed how the treatment affected endline assessments of corruption, and there are no statistically significant differences. Since corruption is persistent, we could also investigate how the treatment effect varies by baseline assessments of corruption at the local level. We see that firms with a higher assessment of local corruption at baseline are less likely to drop out (relative to a stable mean response for the control group across different levels of perceived corruption). The relationship is exactly the opposite as the one that would explain the negative response, i.e., those who faced higher levels of corruption are more likely to drop out. However, only one of the conditional treatment effects is statistically significant, suggesting that corruption is not a major channel of influence.

4.2.4 Increased knowledge of the expected cost of non-compliance?

The treatment could have also reduced the cost of cheating by providing information to firms that they can self-declare their revenue, and if they do not self-declare, a tax official may visit them. In this case, smaller firms may choose to hide instead of self-declaring, and larger firms should self-declare less than what they owe. In such a case, we would expect to see participation and tax amounts collected decrease for the treatment group, which we do not observe. Additionally, if the result is driven by cheating, we would not expect to see the striking differences between the treatment

and control groups in tax participation by those visited by tax officials and those who are not.

5 Conclusion

In this experimental study, we find that firm owners react strongly to an intervention that revealed the fiscal contract. We find that firms in the treatment group are less likely to pay taxes, the effect is skewed toward lower revenue firms, and the intervention increased economic activities and may have increased overall tax revenues.

A better understanding of the fiscal contract empowers firms to act to increase tax compliance in an environment with information constraints. Firms with low revenue are empowered not to pay less than tax officials' imperfect assessments demand, and firms with high revenue are empowered to self-declare. In this way, tax revenue may increase through better sorting of tax compliance, even though overall tax participation declines. This finding is consistent with the view that the large informal sector is a significant source of additional tax revenues (Benjamin and Mbaye 2010) and more efficient targeting could better leverage firms' voluntary sorting into tax participation. Additionally, this sorting may carry a significant positive welfare gain due to the large productivity gap between the informal and formal sector (La Porta and Shleifer 2014).

The enhanced economic activities suggest that empowerment improved the relative bargaining position of firms, enabling them to operate more openly (at least

during tax time) and, consequently, generating greater economic activity. In addition to better targeting and sorting through firm revenue, it is also likely the case that firms with a greater propensity to hide are now operating more openly and actively participating in the tax system, a key factor in building state capacity (Weigel 2020).

While our findings point to the importance of civic information for tax mobilization, we have not established how important voluntary compliance is and what drives it (Allingham and Agnar 1972, James et al. 1992, Slemrod et al. 2001, Chetty et al. 2014). If larger revenue firms are more likely to self-declare due to the treatment, the importance of voluntary compliance could be substantial, which is a powerful source of holding the state accountable.

The results may also be consistent with several other explanations. First, the tax rule revelation could have further exposed corruption, making firms less willing to pay taxes. Second, firms could have learned that the audit probability is lower than they thought, increasing the value of not paying taxes. Third, given that the intervention included disclosing the use of tax money by the authorities, firms could have learned that their taxes were not going toward their preferred services, again making firms less willing to pay taxes. While we do not find supportive evidence for any of these explanations, we cannot rule out that they are at play. Nevertheless, this study advances our knowledge on several fronts, including evidence of the possibility of a virtuous cycle of improved governance, transparency, and tax mobilization. Future research could investigate in more detail these avenues and how citizens' or firms' perceptions of the state affect economic behavior and to what degree these interactions place constraints on the state. (Bergeron, Tourek & Weigel 2021) is one

step forward in this research agenda.

Finally, while the question of external validity should be cautiously assessed, our sample consists mainly of barbers and hairdressers, embroiderers and tailors, and mechanics and vulcanizers – firms that are quite common in the informal sector across Africa and elsewhere in the world. A more likely threat to external validity concerns the specific tax institutions and practices that have evolved in Togo. Nevertheless, the findings in Blimpo and Castañeda Dower (2017) support the pertinence of asymmetry of civic information for other institutional contexts. We find a striking similarity in tax behavior among similar types of firms in Benin, where we document that asymmetry in civic information exists. However, we observe greater symmetry in civic information and greater tax compliance for similar firms located only a few kilometers across the Togo-Ghana border in Aflao, Ghana. Therefore, we believe that our experimental results can inform policy beyond Togo.

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Etude des petites entreprises du secteur privé

I. Message aux entreprises sur la taxation

1. Taxes municipales

La Mairie de Lomé est habilitée à prélever auprès de tout acteur économique qui opère dans ses limites des taxes dites municipales. En tant que *[insérer le type de métier]*, deux types de taxes municipales vous concernent :

- La taxe sur la publicité :
 - i. Plaques directionnelles :

2500 F par face et par an dans la limite de 1m². La plaque peut ne pas contenir une flèche indicatrice. Ce qui est nécessaire c'est l'information qui est inscrite sur la plaque pour situer l'entreprise. Si la surface de la plaque directionnelle dépasse 1 m², ce qui voudrait dire qu'elle contient des informations qui vont au-delà de la simple situation géographique. En ce moment il revient aux agents techniques de la Mairie d'apprécier pour voir si cette plaque doit passer dans la catégorie des panneaux réclames ou pas.



Par exemple l'une des plaques ci-dessus contient non seulement des informations sur la propriétaire du salon de coiffure, mais aussi des informations sur les différents services qui sont offerts dans ce salon. Une telle plaque peut être jugée comme faisant partie de la catégorie des panneaux. Très souvent les gens utilisent trop de plaques qu'il n'en faut et par la suite ils se rendent compte qu'ils ne peuvent pas supporter les coûts. Dans ce cas, ils peuvent retourner vers les services compétents de la Mairie ; qui à leur tour dépêchent leurs agents sur le terrain pour enlever les plaques qu'ils jugent inutiles. Cette modification fera l'objet d'une actualisation de la base de données au niveau de la Mairie.

Figure 1: Content of the intervention on tax obligations

II. Message aux entreprises sur l'usage des recettes fiscales

1. Gestion des recettes municipales par la Mairie de Lomé

Les recettes municipales de la Mairie de Lomé contribuent à la provision de toute une panoplie de services de la Mairie aux habitants de la ville de Lomé. Parmi ces services, l'on peut citer :

- i) l'éclairage public,
- ii) le ramassage et la gestion des ordures ménagères,
- iii) le balayage des voies publiques,
- iv) le maintien de la propreté de la ville,
- v) l'assistance aux personnes nécessiteuses telles que les orphelins et les malades,
- vi) l'entretien, la surveillance et l'exploitation des cimetières,
- vii) l'inhumation des morts délaissés à la morgue du CHU Sylvanus Olympio de Tokoin.
- viii) Etc.

En 2014, les dépenses faites par la Mairie pour faire face aux services susmentionnés sont :

- Éclairage public, feux tricolores 1.007.405.865 F



Figure 2: Content of the intervention on the public use of tax revenues

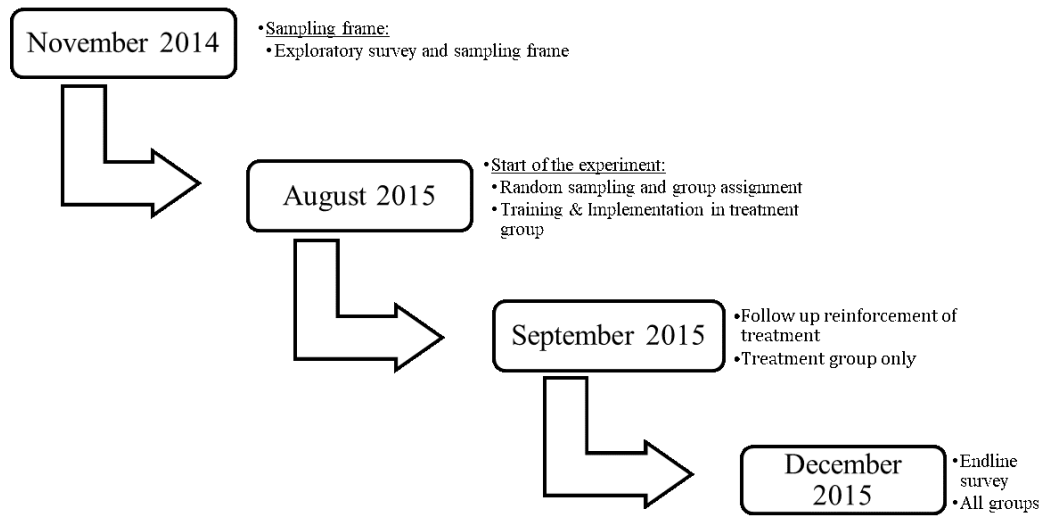


Figure 3: The timeline of the study

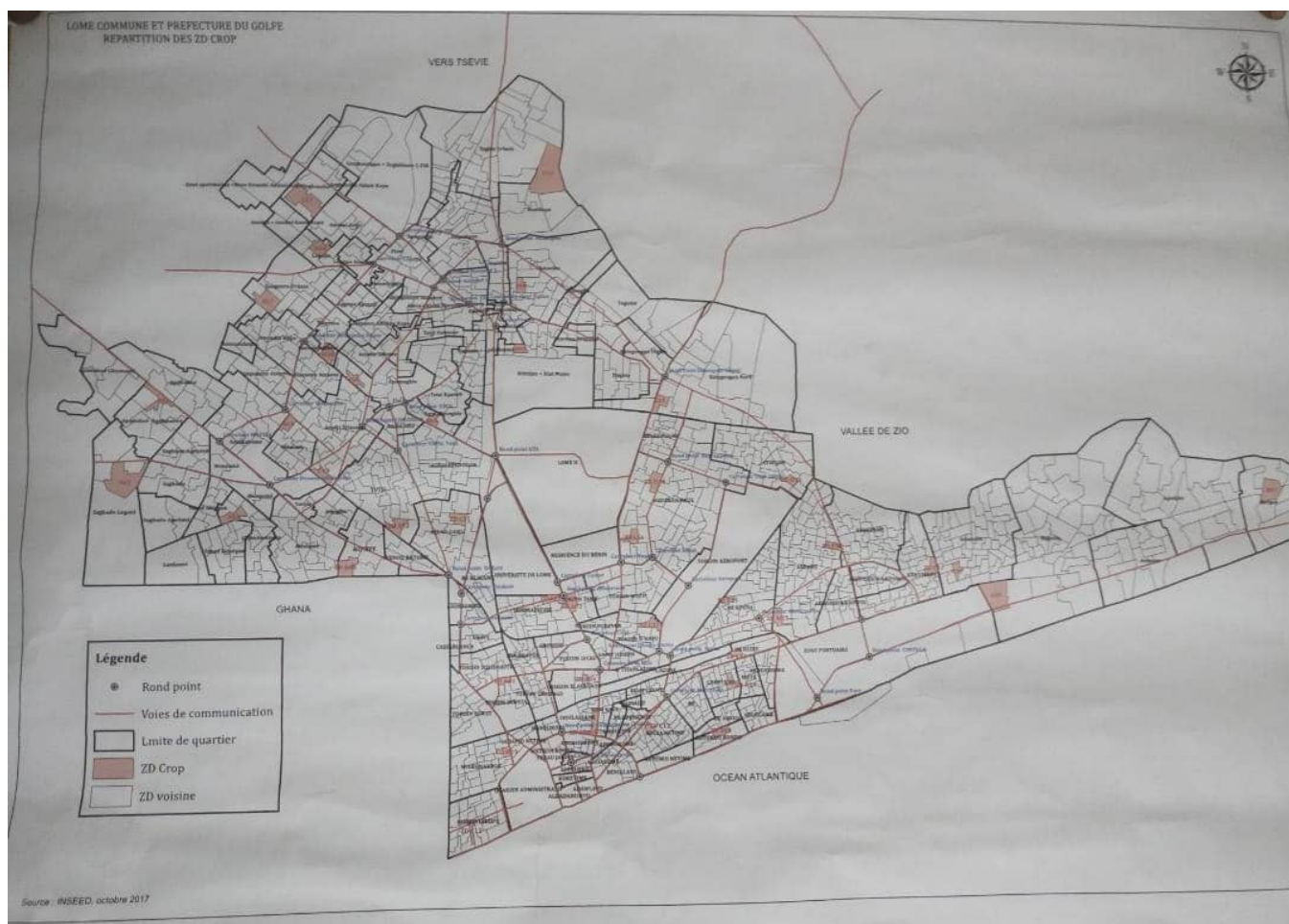


Figure 4: Map of selected enumeration areas

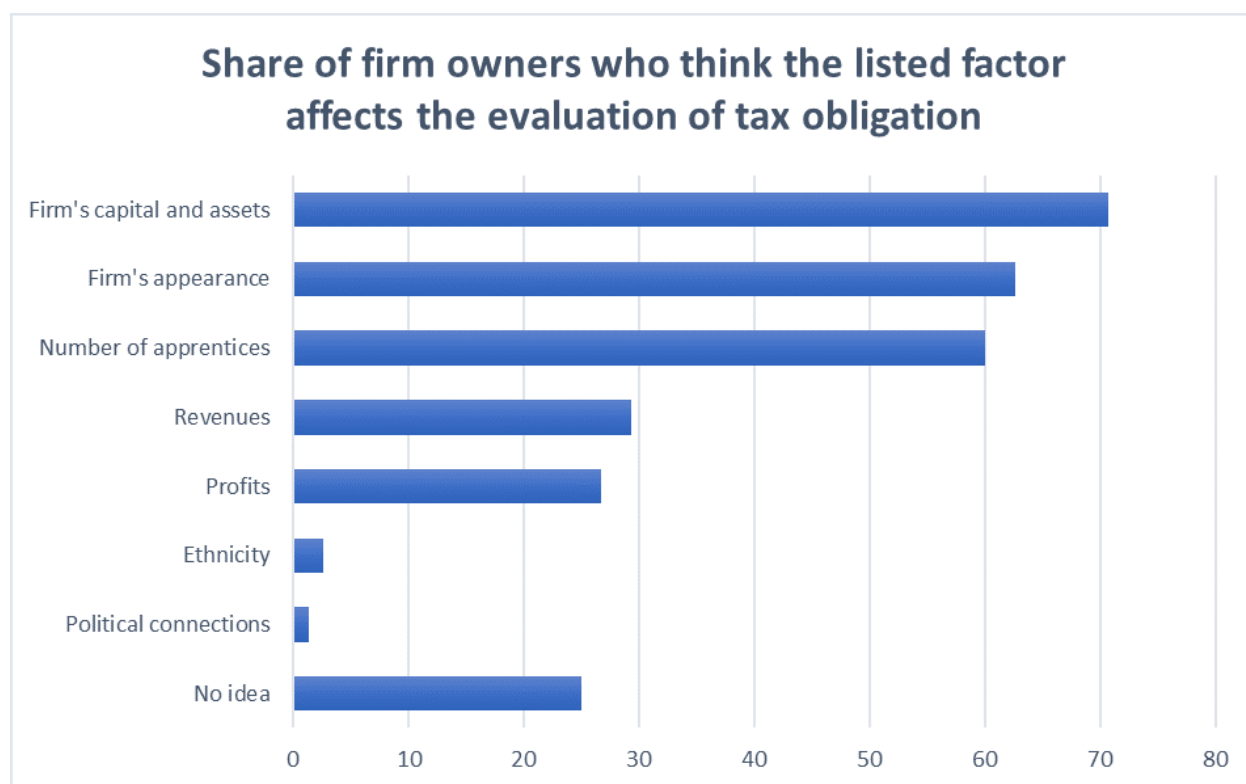


Figure 5: Firm owners' perception of the determinant of tax liability

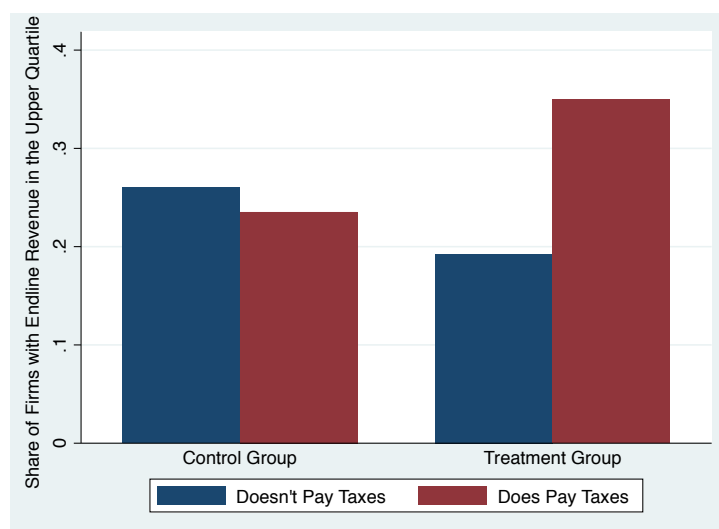


Figure 6: Firm revenue and tax compliance

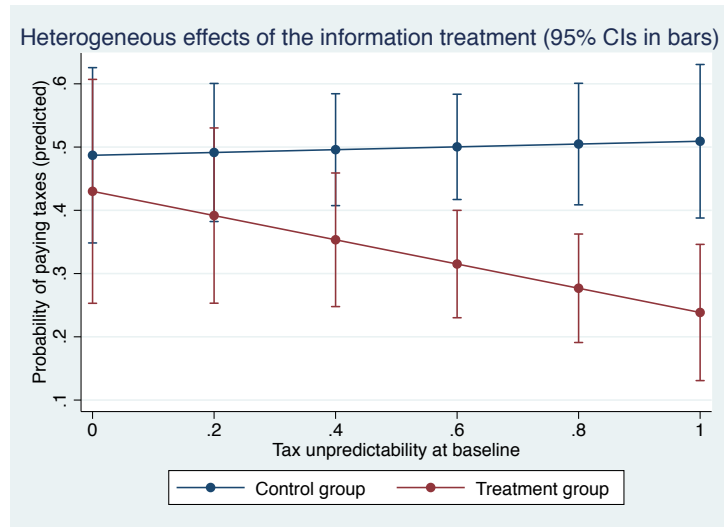


Figure 7: Heterogeneous treatment effects: Tax unpredictability

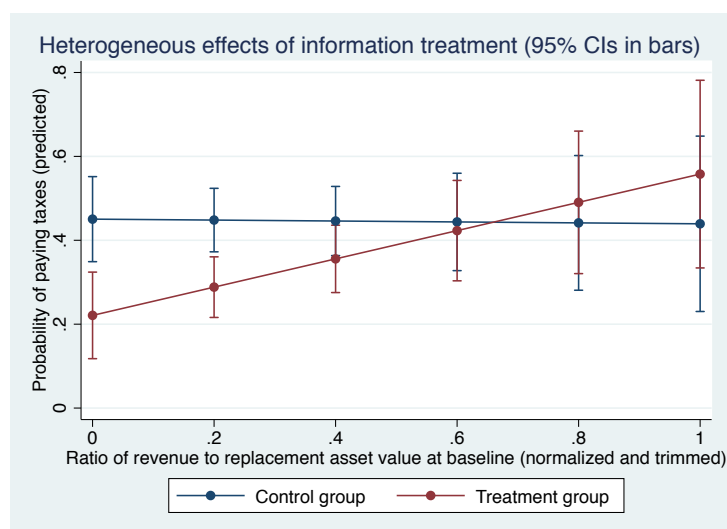


Figure 8: Heterogeneous treatment effects: Actual vs. expected revenue

Table 1: Paying taxes and tax information at baseline

Factor determines tax obligations?	Profits	Revenue	Assets	# of Apprentices	Firm Appearance	Ethnicity
	(1)	(2)	(3)	(4)	(5)	(6)
Pays taxes at baseline	0.051 (0.042)	0.077 (0.048)	0.209 (0.054)	0.138 (0.054)	0.122 (0.059)	0.032 (0.019)
Owner controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm type FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.02	0.12	0.23	0.24	0.10	0.04
N	383	383	383	383	383	383
Mean dep. var.	0.17	0.18	0.52	0.45	0.48	0.03

Notes: Standard errors in parentheses. The dependent variables are indicators of whether the firm affirms that a factor matters for the evaluation of tax obligations. Standard errors are robust to arbitrary forms of heteroskedasticity.

Table 2: Covariate Balance

Dep. Var. = Treatment status	(1)	(2)	(3)	(4)
Firm owner completed primary school	-0.092 (0.053)		-0.121 (0.054)	-0.102 (0.053)
Firm owner age	0.003 (0.003)		0.002 (0.003)	0.002 (0.003)
Firm owner is female	0.018 (0.054)		0.058 (0.060)	-0.004 (0.059)
Firm owner is married	0.011 (0.058)		0.015 (0.058)	-0.000 (0.057)
Firm owner's number of dependents	0.006 (0.009)		0.004 (0.009)	0.006 (0.009)
Firm owner is poor (self-reported)	-0.102 (0.059)		-0.073 (0.062)	-0.061 (0.060)
Pays taxes at baseline		0.085 (0.056)	0.081 (0.057)	0.069 (0.056)
Firm's asset value (in logs)		0.012 (0.027)	0.012 (0.028)	-0.002 (0.027)
Firm has electricity hook-up		0.080 (0.064)	0.094 (0.066)	0.083 (0.065)
Firm size		0.037 (0.038)	0.047 (0.038)	0.032 (0.035)
Firm has signage		-0.070 (0.055)	-0.088 (0.058)	-0.062 (0.058)
Number of apprentices		-0.007 (0.015)	-0.010 (0.015)	-0.012 (0.014)
Firm has a license		-0.016 (0.097)	-0.055 (0.100)	-0.064 (0.099)
Firm type fixed effects	No	No	No	Yes
F-test statistic	1.51	1.09	1.31	0.93
p-value	0.17	0.37	0.21	0.52
R^2	0.02	0.02	0.04	0.03
N	383	383	383	383

Notes: Standard errors in parentheses. The dependent variable is an indicator of whether the firm was assigned treatment. Standard errors are robust to arbitrary forms of heteroskedasticity. The F-test statistics is for the null that the covariates are jointly excludable.

Table 3: Impact of the intervention on firms' knowledge

	I	II	III	IV	V	VI	VII
Treatment=1	0.23 (0.04)	0.24 (0.03)	0.24 (0.04)	0.17 (0.05)	0.10 (0.05)	0.23 (0.05)	0.20 (0.04)
Control Mean	0.06	0.02	0.72	0.23	0.67	0.52	0.15
R ²	0.09	0.12	0.11	0.03	0.01	0.06	0.05
N	383	383	383	383	383	383	383

Notes: Standard errors in parentheses. The dependent variable is an indicator of whether I = respondent correctly identified two municipal taxes from five options, II = respondent correctly identified two National taxes from five options , III = respondent is aware of signage tax (separate from revenue), IV= respondent knows the factors that matter for their tax burden- revenues or profit, V= respondent affirms at least one factor matters for their tax burden, VI = respondent has an opinion about whether the government puts taxes to good use, VII= product of IV and VI, a measure of knowledge of the social contract.

Table 4: Impact of the intervention on firms' tax participation

	Dep. var. = Pays taxes at endline				
	(1)	(2)	(3)	(4)	(5)
Treatment=1	-0.092 (0.049)	-0.127 (0.045)	-0.137 (0.045)	-0.137 (0.045)	-0.137 (0.046)
Pays taxes at baseline		0.367 (0.047)	0.387 (0.049)	0.337 (0.052)	0.302 (0.055)
Constant	0.406 (0.036)	0.274 (0.036)	0.135 (0.109)	-0.265 (0.271)	-0.391 (0.296)
Owner controls	No	No	Yes	Yes	Yes
Firm controls	No	No	No	Yes	Yes
Firm type FE	No	No	No	No	Yes
R^2	0.01	0.15	0.17	0.23	0.26
N	383	383	383	383	383

Notes: Standard errors in parentheses. The dependent variable is an indicator of whether the firm currently pays any taxes (for the fiscal year). Owner controls are marital status, primary education, gender, age, number of dependents and self-reported as poor. Firm controls are value of assets, electricity hook-up, firm size, number of apprentices and whether firm has signage. Standard errors are robust to arbitrary forms of heteroskedasticity.

Table 5: Treatment take-up and tax participation

	Dep. var. = Pays taxes at endline					
	Take-up: Obligation		Take-up: Accountability		Take-up: Both	
	First-stage		First-stage		First-stage	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment=1	0.164 (0.047)		0.232 (0.048)		0.198 (0.043)	
Firm Knowledge		-0.778 (0.361)		-0.550 (0.230)		-0.644 (0.272)
Pays taxes at baseline	0.052 (0.049)	0.408 (0.064)	0.016 (0.049)	0.376 (0.055)	0.020 (0.044)	0.381 (0.056)
F-test statistic	11.94		23.00		21.06	
<i>N</i>	383	383	383	383	383	383

Notes: Standard errors in parentheses. The dependent variable is an indicator of whether the firm currently pays any taxes (for the fiscal year). Standard errors are robust to arbitrary forms of heteroskedasticity.

Table 6: Knowledge of types of taxes that firms are actually paying

	Do you pay TPU?			Total taxpayers
	Yes	No	Don't Know	
Treatment group	70%	15%	15%	60
Control group	15%	24%	60%	78

Table 7: Impact of the intervention on firm's activities and tax payment

Dependent variable =	Revenues last month (USD)		Number of current orders		Taxes paid (USD)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment=1	22 (35)	16 (32)	0.97 (0.51)	0.84 (0.47)	4.45 (2.10)	3.52 (1.92)
Pays taxes at baseline		78 (40)		1.39 (0.57)		9.64 (2.35)
Control Mean	85	85	1.7	1.7	3.5	3.5
R ²	0.001	0.01	0.01	0.03	0.01	0.06
N	330	330	382	382	383	383

Notes: Standard errors in parentheses. The dependent variables are respectively the firms' estimated revenues the last year in USD, the number active orders at the time of the survey, and the reported amount of taxes paid for the last round of tax collection in USD.

Table 8: Information and power

	Visit by tax official		No visit by tax official	
	Treatment	Control	Treatment	Control
Pays taxes	38.6%	80%	24.4%	30%
Doesn't pay taxes	61.4%	20%	75.6%	70%
	$N = 44$	$N = 30$	$N = 119$	$N = 120$

Revenue at endline by group:

	Visit by tax official		No visit by tax official	
	Treatment	Control	Treatment	Control
Pays taxes	\$1345	\$759	\$1582	\$1217
Doesn't pay taxes	\$965	\$616	\$642	\$1099

Notes: Revenue trimmed by upper and lower 5%.