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KNOWLEDGE WITHOUT TAXATION? EXPERIMENTAL EVIDENCE FROM A TAX EDUCATION PROGRAM

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Working Paper 36002
http://www.nber.org/papers/w36002

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
November 2026

We gratefully acknowledge financial support from USAID and Harvard University. We thank Louis Becker, Augustin Bergeron, Emily Breza, Raj Chetty, David Cutler, Eva Davoine, Rema Hanna, Lev Klarnet, Gabriel Kreindler, Daniel Overbeck, Emily Silcock, Davide Viviano, Mary Yilma, and Oliver York for helpful discussions and comments. We are especially thankful to James Dzansi, Henry Telli, and David Lagakos for their help in setting up the field experiment and data collection, and to Abass Abdul Fatawu for helping implement the field experiment. We thank Sedzro Mensah and the excellent team at DataPlas for their support in the data collection process. We are indebted to the staff at the local government of Ablekuma Central for their collaboration and expertise. We thank K. Shankari from the NREL for helping us use the software OpenPATH. The project has ethics approvals from the Committee on the Use of Human Subjects at Harvard University (protocol IRB24-1119). The trial has been registered at the AEA Registry: AEARCTR-0015079. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 36002
November 2026
JEL No. D83, H26, H31, O17

ABSTRACT

A property tax experiment in Ghana randomly provides tax education to property owners and frontline officials. The intervention raises tax knowledge, which spreads through social networks to untreated neighbors, but does not increase tax payments. Educated taxpayers and officials become more engaged in their interactions without developing more collaborative relationships. Taxpayers revise downward their beliefs about enforcement capacity and become more aware of legal ways to reduce their tax burden. Knowledge is a double-edged sword: it can stimulate citizens to engage with the tax system while exposing the state's limited capacity to enforce it.

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1 Introduction

Settings with low tax compliance are oftentimes also places where people have limited knowledge about taxation. In an urban Ghanaian municipality, where less than 15% of citizens comply with property taxes, 92% of property owners could not name one characteristic determining their tax liability. In Pakistan, 93.7% report not knowing the property tax formula (Khan et al., 2016). Knowledge is similarly low among Rwanda's registered taxpayers (Mascagni et al., 2024) and across other African countries (Isbell, 2017) where local tax compliance is often below 15% (Knebelmann et al., 2026).[1]

In such settings, improving knowledge about taxpayers' responsibilities (e.g. to register, file, pay) and rights (e.g. to demand information, appeal, complain) may increase tax payments. Knowledge can help taxpayers navigate the payment process and also encourage quasi-voluntary compliance, an especially valuable route when enforcement capacity is limited. Taxpayer education programs around the world are often explicitly designed under the assumption that better-informed taxpayers are more likely to comply (OECD, 2021).[2]

However, it is not clear that increasing knowledge will actually improve compliance. Recent theory emphasizes that teaching the tax code also teaches taxpayers how to minimize liabilities (Craig and Slemrod, 2024). Taxpayers may uncover flaws in the collection process and revise their beliefs about enforcement capacity downwards, encouraging avoidance or evasion that weak tax administrations struggle to mitigate. Moreover, the tax payment process is often deeply interpersonal and requires interactions with frontline officials (Overbeck and Lungu, 2025). Better-informed taxpayers may be willing to pay but unable to complete the process if officials lack the knowledge to help. Finally, officials have substantial discretion over whom they approach to deliver bills and follow up with (Dzansi et al., 2025), and their official compensation increases in the amount of property tax payments. Officials may strategically choose to avoid knowledgeable

[1]The authors cite studies reporting property tax compliance rates of 10% in Uganda, 9% in Senegal, 9% in the DRC, and 2% in Liberia.

[2]India's Central Board of Indirect Taxes and Customs reports that it "aimed to promote voluntary compliance and strengthen stakeholder engagement through taxpayer education." Indonesia's Directorate General of Taxes states that its Business Development Services "education program aims to improve taxpayer compliance" (our translation). Kenya's KRA TV is dedicated to "empowering the nation through tax education to enhance voluntary compliance." The Uganda Revenue Authority's 2025 newsletter frames taxpayer education as "empowering taxpayers with the information they need to ensure voluntary compliance." The Ghana Revenue Authority introduced the Sustained National Tax Education program to "[i]nstitutionalize tax education" and "build a strong culture of voluntary compliance."

taxpayers if collecting from them requires greater effort without a commensurate increase in the likelihood of payment.

Whether knowledge improves compliance is therefore an empirical question. Rigorous evidence on taxpayer education remains scarce despite the global prevalence of such programs. Is knowledge elastic to a 'typical' program? Does knowledge encourage quasi-voluntary compliance or avoidance, or both, and what is the net effect on tax payment? How do tax officials shape the link between taxpayer knowledge and compliance, and does improving their own knowledge alter these dynamics? In this paper, we make progress on addressing these questions through a multi-arm experiment that randomly exposes taxpayers and tax officials to tax education and which is designed to trace the multiple potential channels linking knowledge to tax outcomes.

The experiment is embedded in a typical property tax campaign in an urban district in the Greater Accra Region of Ghana. During the campaign, the local government assigns tax officials to geographical areas, called units, where they are responsible for delivering the tax bill to property owners, assisting them with queries, collecting payments, and conducting follow-up enforcement if needed. In collaboration with a quasi-government institution that trains public officials in Accra and an independent private-sector tax expert, we designed educational content to improve property taxpayers' declarative knowledge (the "know that") and practical knowledge (the "know how"). The education content placed equal emphasis on taxpayers' rights and their responsibilities, thereby aiming to foster active and engaged citizens and interactions. The main component of the education campaign was a tax education workshop and a concise preview of its content at the end of the baseline survey.

We randomized treatment at the unit level and within a unit, at the household level. Units are randomly assigned to one of three treatment groups (TG1, TG2, TG3) and a pure control group (CG). Within each TG unit, a subset of households are randomly assigned to receive the educational content and the workshop invite: we refer to these as 'TG-treated' households, and the remaining households in TG units as 'TG-untreated' households. The three TG-units differ only on the tax official side. In TG1, officials receive no additional information or training. In TG2, officials additionally receive spatial information on a map identifying areas with a high density of educated households (due to their TG-treated status), allowing us to test whether officials strategically respond to the presence of educated taxpayers in their assigned unit. In TG3, officials receive both the spatial information and the same workshop as treated households, allowing

us to test whether increasing knowledge on both sides alters the nature of interactions and tax outcomes. Geographical units with property owners were randomly paired with tax officials.

We find significant effects of the taxpayer education among TG-treated households on both declarative knowledge (e.g., the process of taxation, taxpayers' rights and responsibilities) and practical knowledge (e.g., how to request a property revaluation). The effects are meaningfully large—especially given that participation in the workshop was 23%—but not heterogeneous across a range of household characteristics. The positive effects for taxpayers are not confined to TG3 units (where officials also received the tax education), even though we find that officials in the TG3 units saw positive knowledge effects (relative to other officials) after their education workshop.

Second, there are large treatment effects on knowledge among the TG-untreated households compared to CG households, concentrated in practical knowledge. We also find that both TG-treated and TG-untreated households are more likely to have had conversations about property taxes than CG households, including on practical topics. By leveraging our experimental design, which induces random spatial variation in the exposure of households to nearby TG-treated households (Borusyak and Hull, 2023), we show that greater exposure to TG-treated households increases a household's likelihood of having such conversations. This suggests sizable knowledge spillovers from the TG-treated households to the TG-untreated households: due to these social interactions, the education intervention has a social multiplier for tax knowledge of 1.6.

The intervention successfully increased knowledge of both taxpayers and tax officials. However, across multiple independent data sources, we find null effects on the main tax outcomes: tax official visits to households, tax bill delivery, and property tax payment. These null results hold for both TG-treated and TG-untreated households, and across the three TG units, and remain fairly precisely estimated across multiple dimensions of household heterogeneity.

To understand mechanisms, we investigate if the intervention had effects on intermediate outcomes that could reconcile the positive treatment effect on knowledge with the null effects on tax outcomes. To begin with, education may have changed taxpayers' views of the taxation process and government. We find some evidence of this. First, neither TG-treated nor TG-untreated households show changes in tax morale. Second, both groups know more than CG households about ways to minimize payments, consistent with their practical knowledge gains, but are no more likely to use them. This may reflect more accurate views: they more often recognize that the circumstances justifying these actions do not apply to them or that the government may not

take them seriously. Third, both groups also more accurately perceive lower local government enforcement capacity than CG households who overestimate enforcement. These changes among both TG-treated and TG-untreated households highlight how educational content spreads through social interactions. Although the shifts in beliefs about tax minimization and enforcement may have been too small to reduce compliance, they may explain why greater knowledge did not increase payments.

Taxpayer education may shape compliance by changing the nature of taxpayer-tax official interactions: Knowing their responsibilities could help taxpayers engage more productively and increase payments but knowing their rights could make those interactions more confrontational, encouraging disengagement and reducing compliance. We find that at endline, households in TG units rate themselves more positively and report greater engagement in interactions. These effects are almost as large for TG-untreated as for TG-treated households and extend beyond TG3 units. Yet households' ratings of officials do not improve, even in TG3. As a result, we see no movement towards more co-productive, collaborative relationships; instead, more households in TG units report having a clear sense of their own objectives while assessing the tax official as unresponsive. Officials answered nearly identical questions, with qualitatively similar but smaller effects among TG3 officials. Knowledge therefore improved taxpayers' self-assessment and engagement, but their greater ability and willingness to engage were not matched on the official's side.

Finally, a third potential mechanism is that the intervention impacts officials' strategies for who to interact with – for example, officials may want to avoid educated households by making fewer visits to them. However, in TG2, we find no treatment effects on interactions in areas that were highlighted as having a higher share of educated households, and this result does not change in TG3 (where the official received the education as well). Moreover, the treated status of households does not alter the extent to which officials target other household characteristics for visits, including among TG2 and TG3 officials.

Taken together, our results show that taxpayers engage with the educational content, using it to update their beliefs and actions, particularly regarding enforcement. Through social interactions, the educational campaign generates a set of engaged taxpayers beyond those directly treated with an implied social multiplier well above 1. Knowledge improves their self-assessments in interactions but does not build a more trusting, collaborative relationship with the state. Education thus stimulates agency without fostering trust in or outright acceptance of the taxation process.

Increasing officials' knowledge does not alter these dynamics.

Our results reflect the pros and cons in the long standing debate about citizen agency. On the one hand, we find that a light intervention did increase knowledge and citizen engagement, which may support accountability and quasi-voluntary compliance in the longer run (Levi, 1988; North and Weingast, 1989), including by prompting the state to undertake comprehensive training of their officials (Acemoglu and Robinson, 2019). On the other hand, revenue gains in the short run were absent, and changes in enforcement beliefs and tax planning knowledge illustrate how informed taxpayers can resist collection. Such resistance may be warranted if the state is overly extractive (Levi, 1988), but it may also facilitate evasion in settings such as Ghana where enforcement is limited, straining scarce public resources.

Related literature We contribute to the literature on taxpayer knowledge and education programs. Several studies suggest that individuals are not fully informed about the tax and transfer policies that are relevant for them (Duflo et al., 2006; Liebman and Luttmer, 2015); Feldman et al. (2016) show that taxpayer misperceptions lead to unintended behavioral responses to tax reforms. Fewer studies investigate the impacts of tax education – in the US, see Chetty and Saez (2013), Kuziemko et al. (2015) and Stantcheva (2021). Our work is closely related to Mascagni, Santoro and Mukama (2024), who deploy multiple quasi-experimental methods to evaluate the Rwandan Revenue Authority's education program.[3] Chetty and Saez (2013) and Mascagni et al. (2024) find muted impacts on reported income, consistent with our null result on tax outcomes. Relative to prior work, our study experimentally evaluates taxpayer education in a setting where limited enforcement leaves substantial room for knowledge to influence payment decisions. Moreover, our design captures equilibrium effects of information provision on both sides of the taxpayer-official interaction.[4]

Education generates a large social multiplier for knowledge through interactions between treated and nearby untreated taxpayers. This is among the first direct experimental evidence of tax knowledge spreading through social networks, consistent with US EITC evidence from taxpayer mover designs (Chetty, Friedman and Saez, 2013).[5] These spillovers underscore the importance of

[3]Although invitation to the program is not directly randomized, Mascagni et al. (2024) use propensity score matching and an instrument based on their survey protocol to deal with endogenous program participation.

[4]Chetty and Saez (2013) find that the effect of information to taxpayers depends on how it interacts with advice provided by tax preparers, consistent with the intuition that education's impacts are shaped by interactions in equilibrium.

[5]Related studies trace how non-knowledge tax shocks, mainly involving enforcement, spread through social and professional networks in the US (Boning, Guyton, Hodge and Slemrod, 2020), Uganda (Almunia, Henning, Knebelmann, Nakyambadde and Tian, 2025), South Africa (Lediga, Riedel and Strohmaier, 2026), and Austrian TV licensing (Drago,

equilibrium effects: impacts on directly treated taxpayers alone do not fully capture education's consequences.

We also contribute by studying the role of interactions between taxpayers and officials in tax systems and the factors which shape these interactions. In developing countries, most work has been either taxpayer-centric (e.g., Brockmeyer, Estefan, Arras and Serrato, 2021; Bergeron, Tourek and Weigel, 2024) or official-centric (e.g., Khan, Khwaja and Olken, 2016, 2019; Weigel, Bessone, Bergeron, Tourek and Kabeya, 2024). Citizen interactions with frontline officials feature in public administration research (Döring et al., 2025), but we know of no economics work focused on the interaction itself except Mascagni, Scarpini, Mukama, Santoro and Hakizimana (2025), who vary hypothetical interactions through survey vignettes. We study actual interactions in the field, identifying how knowledge causally shapes their nature and participants' experiences.

Our focus on interactions connects to two further strands of research: officials' knowledge and taxpayer agency. As in our setting, prior work documents low procedural knowledge among local bureaucrats, including frontline officials in India (Carreri, 2021; Auerbach et al., 2025). We contribute by experimentally increasing officials' knowledge and studying its effects on their behavior in the field. On taxpayer agency, Henn, Paler, Prichard, Samii and Sanchez de la Sierra (2026) show that providing taxpayers in the DRC with access to private tax consultants and civil society protection from officials increases tax payments and state engagement. We complement this work by studying whether a lighter-touch education program can shape their engagement with the state, similarly to work on social transfers that studies if information is sufficient to empower citizens (see, for example, Banerjee et al., 2015).

The remainder of the paper is organized as follows. Section 2 describes the context for our experiment, and Section 3 provides a conceptual framework for the potential impacts of knowledge. Section 4 describes the intervention itself, the experimental design and the empirical specifications, while Section 5 describes the data. Section 6 provides the results on tax-related knowledge and talking, and section 7 provides the impacts on tax collection outcomes. Section 8 discusses mechanisms. Section 9 concludes.

Mengel and Traxler, 2020).

2 Context of the Intervention

2.1 Property Tax Collection in Ablekuma Central

We implemented our study in partnership with the local government of Ablekuma Central, an urban district in the Greater Accra Region of Ghana. Annual property tax collection in this setting involves multiple steps. First, the local government sets the tax rates levied on the property valuations (distinguished by property use and geographic area). A digital system then generates the tax bills for properties already registered with the local government. Frontline officials are assigned geographic areas (collection units) to deliver the bill, answer questions, collect payments, and follow up with taxpayers if necessary. These officials also create new accounts on the digital system, link properties to owners, and add properties that are not yet registered. Property owners can seek clarification of their tax liability, challenge a property assessment, and request adjustment to the bill amount or timing of payment. Property owners can also create an account and look up bills on the portal on their own, but few taxpayers have an account at baseline as the system was relatively new at the time of our intervention. The vast majority of property owners preferred to receive their physical, printed bill delivered by the frontline official. Property owners can pay their taxes either to a visiting tax official, pay directly at the municipal assembly, or through mobile money on the digital portal.

The tax collection process depends to a large degree on how tax officials allocate their time. Many households do not even reach the billing stage; across Ghana, less than half of the printed property tax bills are delivered (Dzansi et al., 2025). Officials face limited monitoring of their day-to-day activities and exercise discretion over which properties to visit and deliver bills to, how long to spend at a property, and how often to follow up with a property owner. Evidence from similar property tax settings to ours suggests tax officials strategically allocate their time using information about households' propensity to pay (Balán et al., 2022; Dzansi et al., 2025). This strategic targeting is partly determined by incentives, which can be important for tax outcomes (Khan et al., 2016). In our setting, tax officials receive a fixed base pay, a commission on collected property taxes, and incentive payments for property registrations in their assigned work areas.[6] At endline, 31% of control group respondents reported having received a property tax bill. Consistent

[6]While the base pay was higher than usual during our study, the existing incentives for registrations and tax collections remained the same.

with the importance of reaching households, a control group household is about 45 percentage points more likely to have received a bill at endline when recently visited by a tax official.

In Ghana, compliance with the property tax typically ranges between 5 and 15%, where compliance is defined as the share of registered taxpayers that have made any payment towards their bill. Interestingly, property owners perceive compliance to be larger than it actually is: at endline, control-group respondents estimated that 56% of residential property owners in the municipality pay property tax (Table 8), compared to 12% actually reporting a payment in the control group (Table 3). Nonpayment creates arrears that carry forward to the next year's bill. Although in principle the municipality can pursue delinquent taxpayers in the courts, impose fines, or seize properties, in practice, tax officials mainly rely on follow-up visits, reminding taxpayers of their obligation to pay, announce penalties, and make accommodations to pay in installments.

During focus groups with property owners, recurring themes included that tax officials appear uneducated about the tax system, unwilling to engage with the taxpayer, and explain very little about the tax collection process, ultimately leaving the taxpayers feeling discontented and disrespected.[7] Our survey evidence corroborates this: for instance, 50% of control group taxpayers who had recently been visited by a tax official at endline report that the tax official provided no information on how the property tax was determined or processed.

2.2 Taxpayer and Tax Official Knowledge at Baseline

Tax education refers to programs that governments can (realistically) undertake to increase current and potential taxpayers' knowledge about how taxation works, why it takes place, and what role(s) the taxpayers have. In principle, the educational content could focus narrowly on taxpayers' responsibilities—primarily the legal obligation to pay—with the intention of improving compliance. There is, however, a broader perspective, in which education puts equal weight on knowledge about the taxpayers' responsibilities and rights, aiming to foster genuine, productive taxpayer engagement and raising compliance in a more quasi-voluntary manner. The broader perspective is consistent with the approach that is most prevalent in policy circles (OECD, 2021) and is the one we consider as the basis of the education intervention in this paper.

[7] For instance, one participant said that "they [the tax official] will just give you the bill when they meet you and that is it. There are no explanations or interactions." Another participant stated that "[t]hey are government workers and do not respect us. They do not have the patience to interact with us. They just present the bill without any explanation."

In this context, two distinct dimensions of knowledge are relevant. The first is what the tax education literature refers to as *declarative knowledge*—the "knowing that" and the "knowing why". "Knowing that" involves understanding what the tax system is, what the rules and processes are, and what the rights and responsibilities of taxpayers are, based on the legal documents that underpin tax collection. "Knowing why" involves understanding why the government has the right to levy taxes. The second dimension is *practical knowledge*—the "knowing how", which refers to the ability to carry out tax-related tasks in practice.

These two dimensions of knowledge are naturally related, but not systematically linked—one does not imply the other, nor is one necessary for the other to be effective. For example, a taxpayer may know which actions can reduce or dispute their tax liability (strong practical knowledge), even without knowing the specific legal origins of their rights (limited declarative knowledge). Conversely, a taxpayer may know their rights as a property owner (strong declarative knowledge), but not how to exercise them in their specific circumstance (limited practical knowledge).[8]

In our context, both declarative and practical knowledge is low among taxpayers. On declarative knowledge, taxpayers have very limited statutory knowledge of the determinants of liability and the tax administration process. For instance, 92% of untreated households could not name any property characteristic that determines the property tax amount in the midline survey; 5% could only name one. Moreover, 93% could not name the institution responsible for property valuation. On practical knowledge, 93% (96%) of households could not name a single responsibility (right) of property taxpayers, as defined in the Local Governance Act, or any description that resembles one of the responsibilities (rights). Similar responses are found in other settings: in Pakistan, 93.7% of property owners do not know the formula for calculating the property tax (Khan et al., 2016).

Knowledge is also low among tax officials. 31% of tax officials who did not receive the education program could not name a property characteristic that determines property tax amount before the collection campaign started; 37.5% could only name one. 59% could not name the institution responsible for property valuation. 50% (69%) could not name a single responsibility (right) of property taxpayers, as defined in the Local Governance Act.

[8]In settings where governments have limited administrative capacity, citizens are often required to come forward and declare that they have certain rights, even if those rights exist under law. As such, having declarative knowledge about rights is a real margin of agency for taxpayers, even in the absence of strong practical knowledge.

3 Conceptual Framework

Baseline knowledge in the control group correlates positively with receiving a bill and making a payment at endline (Table A.21 panel D). In our context, how may knowledge impact tax payment? This section presents a simplified framework to think through the different channels of impact.

First, taxpayer education can increase process-related knowledge. This can lead to a better understanding among taxpayers of the various steps and responsibilities in the bill generating process (from property valuation and rate setting to bill delivery) and the role of the frontline official in this process. Such contextual knowledge could make the taxpayer more willing to engage in the collection process. At the same time, more knowledgeable taxpayers may know more about how to settle their tax bills independent of tax official visits.

Second, taxpayer education may affect quasi-voluntary tax compliance. More educated taxpayers may better understand what the revenue should be used for (i.e. local development), which may increase or decrease compliance depending on the taxpayer's belief about the use of these funds. In fact, in our sample, higher baseline knowledge is associated with lower satisfaction with public goods and lower perceived compliance by other property owners. Being more knowledgeable about taxpayer rights can help accommodate the taxpayers' circumstances—like paying in installments or lowering the bill amount due to financial hardship—and it might affect their understanding of when they are appropriate to demand. By increasing taxpayers' view of the procedural fairness of the collection process, knowledge could boost quasi-voluntary compliance.

Third, learning about the tax code may reveal tax planning instruments and legal grounds for reducing a bill or challenging a valuation (Craig and Slemrod, 2024). For example, knowledge may increase taxpayers' awareness of instances when the due process is not followed correctly (e.g. if tax rates are not gazetted properly at the local government), which provides a legal basis to reject the assigned tax bill. In our sample, we find that higher tax knowledge at baseline is associated with higher awareness of available actions to reduce or engage in tax avoidance (Table A.21 panels B and C). Knowledge may also lead to outright evasion. While more knowledgeable taxpayers could be more aware of the potential consequences of non-payment, they could also form updated beliefs about whether the local government can enforce penalties on evaders. Indeed, taxpayers with higher baseline knowledge in the control group are likely to perceive the local government to have lower enforcement capacity; since these perceptions are overestimated

in general, knowledgeable taxpayers have more accurate beliefs about the enforcement capacity of the local government which is quite limited in practice.

Fourth, these channels may shape interactions between frontline officials and property owners. Research in public administration documents that interactions between citizens and frontline officials (including in non-taxation policy areas) can range from obstruction, disengagement to collaboration (Raaphorst and Loyens, 2020; Nielsen et al., 2021; Döring et al., 2025). Collaborative encounters are considered most productive: despite differing interests, participants share an understanding of the encounter's purpose and engage in clarification and problem-solving rather than suspicion or coercion (Döring, 2021). Process-related knowledge may reduce taxpayers' questions and increase acceptance of officials' rights to access properties and information. Higher tax morale may facilitate cooperation, whereas tax planning and challenges may increase collection costs. Officials' targeting decisions may therefore depend on the expected returns to engaging knowledgeable taxpayers, their own perceived knowledge, their information about taxpayer knowledge. More knowledgeable officials may better explain obligations, follow due process, and respond to challenges. Knowledge on both sides may reduce uncertainty and perceived arbitrariness, strengthening quasi-voluntary compliance.

Control-group survey evidence highlights that interactions are challenging. Among control-group taxpayers visited at endline, 50% report receiving no explanation of how property tax was determined or processed. For 36%, officials could not answer their question in any interaction, while 31% describe officials as unpredictable during encounters. Ultimately, only 24% of taxpayers describe interactions as collaborative, and baseline knowledge correlates negatively with endline assessments of interactions (Table A.21, panel A). Officials' responses similarly indicate limited collaboration: only 13% describe at least half their taxpayer interactions as collaborative, and 90% describe taxpayers as unpredictable in some encounters.

In summary, taxpayer education may improve process-related knowledge and shape quasi-voluntary compliance, but it can also facilitate tax avoidance or evasion. Taxpayers' and officials' knowledge may affect their interactions and officials' targeting decisions. These channels can have counteracting effects on tax payment, making it important to test and document impacts on the underlying mechanisms, as our experiment is designed to do.

4 Intervention and Experimental Design

4.1 Design and Content of the Intervention

In collaboration with public and private sector actors, we developed educational content with the objective of enhancing taxpayers' declarative and practical knowledge of the local property tax. The education content was delivered to all randomly selected taxpayers in two steps: a summary was read by the enumerator at the end of the baseline survey, followed by an in-depth and in-person workshop.

The workshop is the core of the education program. All treated property owners—or the person in charge of paying the property tax—were invited to attend. It covered the following content:

- *The local government's mandate to raise property taxes to fulfill their responsibilities to their communities.* This module increased the "why" knowledge, emphasizing that the local government needs to raise property taxes to support their communities and increase development.
- *Taxation process; taxpayer rights and responsibilities.* This module enhanced declarative knowledge: the process, the tax formula, the rights and responsibilities as defined by law.[9]
- *Cases and FAQ.* This content improved practical knowledge by covering concrete cases where the taxpayer was confronted with a situation related to their property taxes and had to take appropriate action. It also included real-life 'stories' of taxpayers who had successfully undertaken these actions to increase taxpayers' perception that something "can be done".[10]

The full workshop presentation is available here. The invitation to the workshop was delivered by enumerators at the end of the baseline survey, along with a short explanation of the main workshop content.[11] Workshops were organized on different days and at different times of the day. Treated households (TG-treated) were reminded of the upcoming workshops via SMS and phone calls and informed that they would receive compensation to cover the cost of commuting

[9]Specifically, the workshop covered the following rights, all grounded in legal documents: right to information; right to participation; right to appeal; right to apply for a reduction in payment; right to refund; right to complain. Under responsibilities, the workshop covered: pay unless exempt; pay by due date; provide information as relevant; facilitate assessment and collection.

[10]Importantly, to increase local relevance, all content was drawn from Ablekuma Central, the local government where the intervention took place. For example, several of the stories were selected as the most frequently occurring issues raised in focus groups by local property owners.

[11]The trained enumerator did not present this as a condensed version of all the workshop material, making it unlikely that households who did not attend the workshop did so because they thought there was nothing additional to learn.

to the workshop if they attended. Finally, TG-treated households were also informed that they could call a hotline number to ask questions about local property taxes.[12]

The research team developed the content in close consultation with Accra's Institute of Local Government Studies, a quasi-government institution providing local governance training, and a local private-sector tax lawyer, who delivered the workshop. These consultations ensured technical accuracy, relevance, and a balance between rights and responsibilities consistent with OECD (2021). The content also aimed to be neutral, neither appearing government-designed to coerce compliance nor favoring tax minimization.[13]

Our design has two implications. First, the intervention is purely informational: it neither acts on participants' behalf nor provides administrative, financial, or material support. This resembles government taxpayer education campaigns more closely than intensive interventions, such as hiring lawyers to represent taxpayers, whose effects may reflect professional expertise rather than taxpayer behavioral changes. However, null results could reflect insufficient treatment intensity rather than the absence of causal links.

Second, the content deliberately avoided emphasizing the Ablekuma Central government. The lawyer adopted a neutral tone without strong references to taxpayer actions, and the content omitted actual government activities.[14] This reduces the risk that perceived political or administrative motives discourage engagement and learning. However, it limits external validity because education campaigns are typically government-led. Whereas our intervention seeks to isolate the effects of increased knowledge, government-led campaigns may additionally operate through participants' perceptions of the government's broader political and administrative intentions.

4.2 Experimental Arms

Our experiment consists of a pure control group (CG) and three treatment groups (TG1, TG2, TG3). About half of the surveyed property owners in each of the three treatment arms, respectively, received the education intervention described in section 4.1.

[12]We tracked the purpose of the calls made to the hotline. The only calls recorded were inquiries on the workshop details such as venue and times.

[13]Consistent with this objective, workshop attendance among treated households did not vary with baseline satisfaction with the Ablekuma Central government or preferences for legally minimizing tax liability (Table A.1 and section 4.6).

[14]Highlighting government performance could strengthen the fiscal contract but also induce negative updating about its capacity to provide public goods, worsening tax outcomes independently of knowledge. Instead, we explained the general rationale for local property taxes.

The treatment of the tax official varies across three treatment groups (cf. Figures C.2 and C.3).[15]

1. In the first treatment group (TG1), tax officials received no treatment compared to the status quo (which includes a short training focused on practical aspects of the property tax collection campaign). They were simply informed that an education campaign had recently been carried out in Ablekuma Central.[16] The officials received a map of their collection unit within Google Maps without coordinates or information on specific properties.
2. In the second treatment group (TG2), the collection unit map provided to a tax official highlighted two areas with a high density of TG-treated property owners (see Figure B.3 for an example). Treatment-dense areas typically include 3-4 treated properties (TG-treated; irrespective of whether they attended the workshop) with at most one untreated property (TG-untreated), but the map did not include coordinates or information on specific properties. This information was communicated to the officials in TG2 via a simple message.[17]
3. Finally, in the third treatment group (TG3), tax officials also received the educational intervention in addition to the map highlighting two areas with a high treatment density.

The experimental design allows us to study the effects of both taxpayer and tax official education. As discussed in section 3, ex-ante, it is unclear whether tax officials anticipate more educated taxpayers to be good or bad for their own objectives. Highlighting the treatment-dense areas in TG2 tests whether officials spend more or less time interacting with households in highlighted areas, revealing their beliefs about what taxpayer education implies. It also allows us to observe targeting strategies and facilitate learning for the tax official relative to TG1. It is similarly unclear ex-ante how educating tax officials changes their interaction with more or less educated taxpayers, and how outcomes depend on the education status of both sides, which TG3 allows us to study.

4.3 Collection Units

During a fiscal year, the local government assigns officials to designated geographical areas for six weeks at a time. The designated areas are called 'collection units' and are defined with

[15] Across all experimental arms, tax officials received a weekly base payment plus a commission based on taxpayer registrations and collected payments.

[16] Specifically, the message read: "An education campaign has very recently been conducted with some residential and mixed-use property owners in Ablekuma Central. The owners received information on property taxes as well as information on their rights and responsibilities as residential ratepayers."

[17] Officials in TG2 received the same message as in TG1 with the *additional* sentence: "On your map, two areas are highlighted where many property owners received this education."

geographical boundaries that create a cluster of physically adjacent properties. During each six-week campaign, officials are responsible for delivering bills and collecting payments from assigned property taxpayers in their unit. Each unit is only covered once during a fiscal year; after each campaign, the official is assigned to a new unit. The relatively short duration of each campaign results from the large number of properties relative to the limited number of officials. The duration is not specific to this local government, as property owners are legally required to pay within the six weeks that mark a campaign by Ghana's Local Governance Act.

Our experiment was conducted in a subset of the local government's property tax collection units in the fall of 2024. To observe each official in two distinct geographic areas within the period of our study, we reduced the size of the units and assigned each official to one unit in wave 1 (three weeks) and another unit in wave 2 (three weeks) with a one week gap in between. Each experimental collection unit is approximately half the size of a unit that an official would be assigned to for six weeks (under regular functioning) and included about 60 properties each.[18] Experimental units were selected such that no significant collection efforts had been exerted for the fiscal year prior to our intervention in these areas; we specifically excluded the wealthiest areas (where taxes had already been collected) and the poorest units (where the local government typically allocates fewer collection resources).

4.4 Randomization Design

Tax officials and taxpayers were assigned to the four experimental arms (CG, TG1, TG2, TG3) as follows. We summarize the procedure here while Appendix B provides details.

First, we randomly grouped the 48 collection units into 24 pairs (to match the number of tax officials; one unit for wave 1, one unit for wave 2), then randomly assigned pairs to one of the four experimental arms, stratifying by the median pair-level average property value.[19] Within each

[18] See Figure B.1 for an outline of the collection units. The local government's collection units in previous years are built off more granular collection unit blocks. We used these blocks to form the experimental collection unit. The partnering local government does not have a census of all properties. We initially created a property count based on the "property structures" recorded in the administrative records, which included all officially valued properties (by the Ghana Lands Commission) and those registered by the municipality. During the baseline survey, enumerator feedback suggested that the units were smaller than expected because different property structures sometimes belonged to the same compound property. Subsequently, we adjusted the units to cover additional properties. This was done before the collection campaign had begun and unit maps were shared with tax officials.

[19] To achieve better balance given the small number of pairs/units, we used re-randomization based on administrative data on the registered properties in the collection units (including officially valued properties and those added by the local government).

pair, we randomly assigned a unit to either wave 1 or 2 of the experiment. Since pairs are the unit of randomization, collection units within a pair are always in the same experimental arm.

Second, we randomly assigned each of the 24 tax officials to one of the collection unit pairs, who then inherited the treatment status of the assigned collection unit pair as well as the random sequence of the two units for waves 1 and 2 from step one. The 24 officials constitute the entire set of officials that were available to work at that time in the local government.[20]

We aimed to survey 25 property owners in each collection unit regardless of its treatment status (sampling strategy discussed below). In each collection unit of the three treatment groups, respectively, 12 property owners were randomly assigned at the end of the baseline survey to receive the education intervention.[21]

4.5 Balance of Randomization

Table A.2 shows balance of randomization for the tax officials. Treatment assignment is balanced across multiple baseline characteristics, both comparing TG3 to all other arms (columns 1 and 2) and comparing each arm to the control group individually (columns 3 to 6). Tax officials in TG3 have a less confident outlook for the next collection week relative to all other arms, statistically significant at the 10% level. Tax officials in TG2 and TG3 are less likely to speak the primary indigenous local language in the municipality (Ga) than officials in CG.[22]

Table A.3 shows balance of randomization for property owners. TG-treated and TG-untreated households are very similar to those in the control group (columns 2 and 3).[23] For TG-treated, 3 out of 37 baseline covariates are unbalanced (less knowledge of property tax liability determinants, less likely to view local tax collection as efficient, and less satisfied with public goods and services provision). For TG-untreated, 2 out of the 37 covariates are unbalanced (less knowledge of property tax liability determinants, less likely to view local tax collection as efficient). Comparing

[20]The tax official randomization also used re-randomization based on the baseline survey and local government data. Collection units were put into randomly formed pairs so that the two locations are served by one official. Assigning the pair to an arm keeps that official's treatment constant across waves and allowed us to stratify treatment groups by property values.

[21]This was done by pre-randomizing respondent IDs to the treatment status, unknown to the enumerator, who entered the IDs chronologically during the baseline survey and only learned about a respondent's treatment status at the very end of the survey.

[22]Since local language could affect taxpayer-official interactions, we show robustness of the main results to controlling for whether the tax official speaks Ga. Consistent with the fact that many languages are spoken locally—and the majority of people in Accra at least also speak Twi—we find that controlling for this does not alter our results.

[23]Table A.4 breaks this down further by the specific treatment groups.

TG-treated to TG-untreated households within the three treatment groups, balance is somewhat weaker, with statistically significant differences in 6 out of the 37 baseline covariates (column 4).[24]

Our primary comparisons—TG-treated vs. CG and TG-untreated vs. CG—show strong overall balance at baseline across individual characteristics, property characteristics, property tax history, property tax knowledge, property tax attitudes, and local government satisfaction.

4.6 Treatment Take-Up: Participation in the Education Workshop

Our analyses use an Intent-to-Treat specification relating taxpayer and tax official outcomes to their treatment assignment (section 4.7). Following our survey protocol and verified using survey timestamps, all TG-treated households received the concise summary of the workshop content at the end of the baseline survey. Enumerators did not know a respondent's treatment status until the very end of the baseline survey and no untreated households received this explanation. However, important for interpreting our effects, only about one in four TG-treated households (23%) attended a workshop. Reassuringly, only three uninvited households attended the workshop.

At endline, we asked invited respondents who did not attend for reasons why they did not attend. Non-attendance is not explained by a lack of interest: only four individuals (1.3% of the invited households) stated that they were not interested in this kind of information and that it would not make a difference. Instead, the most common answers included being busy or having to work, being away for travel, having felt unwell, and not knowing the date and time (everyone with valid numbers received reminder messages and calls). Moreover, 94.5% of those who attended the workshop said that they found the workshop helpful and 90.4% of all TG-treated property owners found the information they had received at baseline helpful.

Column (1) of Table A.1 examines selection into workshop participation along observable determinants of taxpayer behavior. We find no differential participation across four indices used throughout the analysis. The *income/wealth index* combines days short of cash for basic expenditures in a typical month, difficulty raising 300 GHS within four days, last month's household earnings, and ownership of motorbikes, cars or trucks, televisions, generators, sewing machines, and radios. The *property tax history index* captures bill receipt in recent years, receipt of the 2024

[24]These are the number of days in a month short of cash, the difficulty finding GHS 300 within four days, property ownership status, the property tax history index, reporting to have made a full property tax payment already, and the perceived likelihood that the tax official accepts unofficial payment.

bill before the intervention, and partial or full payment for 2024. The *tax planner index* combines agreement that taxes should be reduced because money belongs to its earners, acceptance of legal measures to reduce liability, and disagreement that civic duty precludes declining to pay. The *local government satisfaction index* combines satisfaction with personal dealings with local officials and public goods and services, alongside prior attendance at an assembly town hall meeting.

4.7 Empirical Specifications

Primary specifications for taxpayers We estimate the Intent-to-Treat (ITT) effects of the taxpayer education intervention using the following "pooled" specification:

$$y_{ij} = \alpha + \beta\, \mathbb{1}(\text{TG untreated}_{ij}) + \gamma\, \mathbb{1}(\text{TG treated}_{ij}) + \phi\, \mathbb{1}(V_j \geq V_{(50)}) + \varepsilon_{ij} \tag{1}$$

where y_{ij} is the outcome for taxpayer i in collection unit j, $\mathbb{1}(\text{TG untreated}_{ij})$ equals 1 if a taxpayer i lives in a treatment group collection unit j but did not receive the education intervention, $\mathbb{1}(\text{TG treated}_{ij})$ equals 1 if a taxpayer i lives in a treatment group collection unit j and received the intervention, and $\mathbb{1}(V_j \geq V_{(50)})$ indicates whether the average property value in the collection unit pair containing j is above the study-area median (the stratification variable). We cluster standard errors at the collection unit level. β is the ITT estimate that captures the average effect of living in a unit where some *other* taxpayers received the education intervention but not oneself, and γ is the ITT estimate that captures the average effect of directly receiving the intervention oneself.

We also estimate the disaggregated ITT effects for the taxpayers by treatment group using the following "saturated" specification:

$$y_{ij} = \alpha + \sum_{k=1}^{3} \beta_k \mathbb{1}(\text{TG untreated}_{ij=k}) + \sum_{k=1}^{3} \gamma_k \mathbb{1}(\text{TG treated}_{ij=k}) + \phi \mathbb{1}(V_j \geq V_{(50)}) + \varepsilon_{ij} \tag{2}$$

where $\mathbb{1}(\text{TG untreated}_{ij=k})$ equals 1 if a taxpayer i lives in a collection unit j of type k but did not receive the intervention; $\mathbb{1}(\text{TG treated}_{ij=k})$ equals 1 if a taxpayer i lives in a unit j of type k and received the education intervention. Remaining variables are analogous to equation (1). We cluster standard errors at the collection unit level. β_ks are the ITT estimates that capture the average effect of living in a collection unit of type k where *other* taxpayers received the intervention but not oneself, and γ_ks are the ITT estimates that capture the average effect of directly receiving the education intervention in a collection unit of type k.

Primary specifications for tax officials We estimate the ITT effects for tax officials using the following "pooled" specification:

$$y_{ijt} = \alpha_t + \beta \mathbb{1}(TG_{ij=3}) + \phi \mathbb{1}(V_j \geq V_{(50)}) + \varepsilon_{ijt} \tag{3}$$

where y_{ijt} is the outcome for tax official i in collection unit j at time t, $\mathbb{1}(TG_{ij=3})$ indicates whether the tax official was assigned to receive the tax education intervention, and $\mathbb{1}(V_j \geq V_{(50)})$ is defined as in equation (1). β is the ITT estimate that captures the average effect of being assigned to receive tax education. Given full compliance with the random treatment assignment to the education workshop among officials, the ITT is equivalent to the Treatment-on-the-Treated effect (TOT).

We also estimate the "saturated" ITT effects for tax officials using the following specification:

$$y_{ijt} = \alpha_t + \sum_{k=1}^{3} \beta_k \mathbb{1}(TG_{ij=k}) + \phi \mathbb{1}(V_j \geq V_{(50)}) + \varepsilon_{ijt} \tag{4}$$

where $\mathbb{1}(TG_{ij=k})$ indicates whether the official was assigned a collection unit of type k. The β_k-coefficients are the ITT estimates that capture the average effect of being assigned to a collection unit in treatment group of type-k.

The majority of questions were asked to tax officials across different weeks indicated by subscript t. For these variables, as expressed in equations (3) and (4), we pool the data across survey weeks and include time fixed effects α_t. For these tax official panel regressions, we cluster standard errors at the tax official level.

4.8 Minimum Detectable Effect Sizes

We calculate minimum detectable effects (MDEs) for the main endline outcomes using the control group's endline outcome distributions, with 80 percent power for a two-sided test at the 5 percent level. MDEs are about 0.16 standard deviations (s.d.) for the overall knowledge index over a control group mean of -0.08 sd and 0.24 s.d. for the taxpayer-action knowledge index over a control mean of -0.34 s.d. For intermediate outcomes, MDEs are 13 points for taxpayers' ratings of themselves in interactions with tax officials (control mean: 55.17), 22.35 points for ratings of officials' usefulness in these interactions (control mean: 16.44), and 6.5 points for perceived tax enforcement (control mean: 56). For taxpayer actions on bill reduction, MDEs are about 3–8

percentage points against control means of 1–8%. For tax morale, MDEs are 0.1–0.25 s.d. against control means of 0.2–0.8 s.d. MDEs are approximately 21 percentage points for receiving a bill and 11 percentage points for making any payment, relative to control means of 31 percent and 12 percent, respectively.

The experiment can thus detect small to moderate knowledge effects and moderate effects on some intermediate outcomes, including enforcement perceptions and interactions with officials, but only larger effects on other intermediate and tax compliance outcomes. At the same time, our mechanism analysis (Section 8) will reveal statistically significant effects on intermediate outcomes that have potentially offsetting implications for tax compliance, and which can conceptually reconcile increased knowledge with unchanged compliance. Although limited power prevents us from ruling out small compliance effects, our findings support a mechanism-based explanation for the null compliance effect reported in Section 7.

5 Data and Sample Description

We conducted multiple surveys of property owners and tax officials, which we complement with secondary data from the local government and GPS data recorded on the tax officials' work tablets.

Property owners Participants must be in charge of the property tax—either as the property owner or co-owner, or handle it on the owner's behalf (e.g., children or caretaker). The property must be used for residential or for mixed residential and commercial purposes; purely commercial properties were excluded. We conducted comprehensive baseline, midline, and endline surveys (see Figure C.1 for a timeline). The baseline and endline surveys were in-person and included all collection units in both waves. The midline was a phone interview shortly after the education workshop and before the tax collection for a random subset of property owners in wave 2.[25]

Table A.5 describes the baseline sample of 1,193 property owners (slightly below the target of 25 respondents for each of the 48 collection units). The sample is relatively evenly split between men and women, about 44% are at least 61 years old, one-third have completed secondary education, half are self-employed or work in a family business, and about 28% are retired. Property owners are generally cash-constrained with about 9 days short of cash in a typical month. Properties are

[25]The midline includes 319 out of 595 households (wave 2); to oversample directly treated households, we randomly sampled TG-treated households with probability 0.75 and TG-untreated and CG households with probability 0.4.

often co-owned by multiple family members and most are for residential use only.

Attrition from the baseline to endline is 7% and is balanced across treatment groups. Attrition is positively correlated with our income/wealth index (half a standard deviation higher among attriters), but not differentially so across the treatment groups. Attrition is also uncorrelated with the property tax history index, the tax planner index, and the local government satisfaction index.

Tax officials The tax officials were selected by the local government, recruited through the standard process, and all of them consented to participate in this study. We conducted a comprehensive in-person baseline survey. Shortly after the education workshop for treated officials, we administered an in-person "pen-and-paper" knowledge test (wave 1). During the collection campaign, we conducted detailed weekly phone surveys, including at the end, for a total of seven weeks. Surveys were voluntary; the response rate was 100% in all weeks except in weeks 1 and 5 (87.5% response rate). Attrition is balanced across treatment arms.

Table A.6 provides summary statistics for the 24 tax officials in the study. The majority of tax officials completed a tertiary degree. On average, they have already worked for the local government for 1.4 years and as a tax official for 2.4 years (including in other jurisdictions).

Other data sources We complement the survey data with high-frequency GPS data from officials' work tablets through the application *NREL OpenPATH*, allowing us to measure the time spent in a collection unit and the approximate properties visited.[26] From the local government, we obtained the universe of property structures recorded on their collection system prior to our intervention, data from tax officials' daily reports to their supervisors (number of property owners visited, number of property tax registrations), and background information on the tax officials.

6 Results on knowledge and talking

6.1 Knowledge: Directly treated

Table 1 shows the Intent-to-Treat (ITT) effects on knowledge for taxpayers at midline (Panel A) and endline (Panel B) as well as for tax officials at midline (Panel C). In this subsection, we focus

[26] In wave 1, 10 officials' tablets were randomly selected to record GPS data; in wave 2, all officials reported GPS data. The data was available to us with lags of 1-2 days. We followed up with tax officials with missing data for a given day, but the recorded data remain intermittent. For instance, officials may have spent time in the collection unit with the tablet off or in "airplane mode" to save battery. Reported data patterns are balanced across treatment groups.

on the directly treated households in Panels A and B, that is, the TG-treated property owners.

Column (1) of Panel A shows a large, positive treatment effect on the declarative knowledge index at midline. This measure is based on a combination of multiple choice and open-ended text questions, covering many aspects of declarative ('knowing that') knowledge. Columns (2) to (5) decompose the index into its components, showing that an important part of the increase in declarative knowledge comes from the increased knowledge of the property tax payment process; the effects on other domains of declarative knowledge—formula, responsibilities, rights—are positive and only slightly smaller in magnitude than for processes but more imprecisely estimated.

Panel B shows knowledge effects at endline. For the same set of pen-and-paper questions, the positive treatment effect on the declarative knowledge index persists at a magnitude similar to that at midline. Across domains, this positive effect is driven primarily by knowledge of the collection process and rights. At endline, we also measured practical knowledge; respondents reported whether they knew about actions they could take during the taxation process, including requesting a lower liability, requesting a revaluation of the property value, or making payments in installments. We observe a large treatment effect on self-reported knowledge of these actions (column 7), which is substantially larger than the treatment effect for the knowledge index. This suggests that the education intervention improved not only declarative knowledge ('knowing that') but also practical knowledge ('knowing how'), consistent with the design and objective of the content, which targeted both.

Finally, by endline we observe a large positive treatment effect on self-reported knowledge (column 6). This likely stems from increases in both declarative and practical knowledge. Interestingly, there was no treatment effect on self-perceived knowledge at midline, suggesting that households' perception of their own knowledge grows over time since the education intervention.[27]

In Panel C, we evaluate the impacts of providing the same education content to tax officials in TG3. Despite the small sample size,[28] we detect a large treatment effect on the index for pen-and-paper knowledge questions (p-value of 0.17), driven especially by statistically significant improvements in knowledge of the formula and rights, with qualitatively positive increases for process and

[27] Alternatively, the sense of one's own knowledge may only manifest once it is put into action through interactions with officials and no such interactions had occurred by midline. Yet another possibility is that self-perceived knowledge is primarily driven by practical rather than declarative knowledge, consistent with the fact that the practical knowledge effect appears only at endline while the declarative effect is present at both midline and endline.

[28] The pen-and-paper questions led to non-responses on some items. In Table 1, we limit the sample of tax officials in Panel C to those respondents who answered all of the knowledge questions. Results are the same (and statistically significant for the index in column 1) when we include all available data for each outcome individually.

responsibilities. We did not ask the questions on practical knowledge to the tax officials.

Though the education content is the same for both households and tax officials, the magnitudes of the declarative knowledge effects across the two sets of actors at the endline cannot be compared directly to each other. First, the outcomes are standardized with respect to their respective populations; a one standard deviation higher score for taxpayers is not necessarily the same as a one standard deviation higher unit for tax officials. Second, the baseline knowledge levels differ in these two populations and the return to education may be non-linear and diminishing. Third, the table reports ITT estimates and the taxpayer effects are a combination of the concise summary of the education content at the end of the baseline survey and the workshop participation.

Appendix Table A.7 shows knowledge results from the corresponding saturated specification. For TG-treated households, column (1) in Panel A shows that the pen-and-paper knowledge effects at midline—before any interactions with tax officials—are not confined to any one treatment arm. At endline, the positive effects on pen-and-paper knowledge are not confined to TG3, where officials are also educated; if anything, the effects are larger for directly treated households in TG1 and TG2 than in TG3. Similarly, the positive effects at endline on practical knowledge and own knowledge perception are not confined to TG3. These results support the interpretation that the knowledge effects among taxpayers are attributable to the taxpayer education intervention rather than learning spillovers through interactions with tax officials.

Table A.1 shows little systematic heterogeneity in knowledge effects across income/wealth, property tax history, tax planner, and local government satisfaction indices. This mirrors the workshop participation patterns, suggesting that while not everyone takes up educational content, both take-up and its knowledge benefits are uniform across household characteristics.

6.2 Knowledge: Indirectly treated through conversations

In this subsection, we turn to knowledge effects among untreated households in treatment groups (TG-untreated). These households did not receive the education intervention directly, but are more exposed to treated households nearby than households in the control group. In Panel B of Table 1, an interesting pattern emerges: TG-untreated households report a statistically significant increase in their self-perceived knowledge, relative to control households, driven entirely by a large increase in practical knowledge with no impact on declarative knowledge.

Two possible explanations for these knowledge effects on TG-untreated households can immediately be ruled out. First, the saturated specification in Appendix Table A.7 shows that the effects on practical knowledge among TG-untreated exist outside TG3 and are, if anything, larger in TG1 and TG2. This rules out the possibility that the pooled TG-untreated effect is solely driven by households in TG3 areas where the assigned tax officials are more educated and may have engaged in more conversations. Second, there are no treatment effects on visits and interactions in highlighted areas (Table 5, discussed in section 7 below), suggesting that the results for TG-untreated are not driven by those TG-untreated households that were captured in the areas highlighted to tax officials as containing more educated households (in TG2 and TG3).

Rather, we hypothesize that these indirect knowledge effects arise through conversations with directly treated (TG-treated) households nearby. Two results support this hypothesis. First, Table 2 shows that TG-untreated taxpayers are 11 percentage points more likely than taxpayers in the pure control units to speak to anyone about property taxes, a large increase in the rate of communication compared to the pure control group (275%).[29] Columns (2)-(3) show that this increase is primarily driven by conversations with relatives and friends, with a smaller and noisier increase in communication with neighbors.[30] TG-treated households are also much more likely than control households to speak to both relatives/friends and neighbors about taxes. The remaining columns of Table 2 show that a range of topics are discussed in these conversations between TG-untreated and TG-treated households. Importantly, mirroring their treatment effect on practical knowledge, TG-untreated households are indeed more likely to report discussing action-related topics (e.g., lowering liability, taxpayer rights).[31]

Our interpretation is that the treatment effect among TG-untreated households comes from their greater exposure to TG-treated households nearby. Since social networks (relatives, friends, neighbors) do not perfectly overlap with administrative boundaries of the experimental units, some households in control units will also have been exposed to TG-treated households. This motivates our second result, which tests whether the increase in conversations is driven by households' extent of exposure to TG-treated households.

[29] These particularly large effects on communication may partly reflect the fact that education campaigns, like our workshop event, are infrequently implemented by local governments.

[30] The survey did not make these two groups mutually exclusive; it is possible that respondents refer to neighbors as the subset of all neighbors that are not also their relatives or friends.

[31] Again, the saturated specification in Table A.8 confirms that these results are not limited to TG-untreated households in TG3.

Naturally, we cannot use a household's random assignment to one of the TG groups as an instrument for exposure to TG-treated households, since the unit-level assignment could affect conversations through channels other than proximity to TG-treated households. We therefore require a distinct source of exogenous variation. We leverage the randomization protocol, which induces random spatial variation in the exposure to nearby TG-treated households both across *and* within collection units: in a TG collection unit, each neighbor is treated with about 50% probability ex-ante, so by chance some households have more TG-treated neighbors than others. We can thus follow the "re-centering" approach of Borusyak and Hull (2023) to test whether not-directly-treated households are more likely to discuss property taxes when exposed to more TG-treated households than expected under the randomization protocol. Intuitively, we obtain the "unexpected exposure" to TG-treated households by subtracting the expected exposure (conditional on a unit's treatment status) from the realized exposure at varying radii. This isolates exogenous variation to TG-treated households even within a collection unit while allowing for exposure to TG-treated households across unit boundaries.

Formally, let $\mathcal{I}$ denote the set of surveyed properties. For household $i \in \mathcal{I}$, let $\mathcal{J}_{ik} \subseteq \mathcal{I} \setminus i$ be the set of properties in radius k of household i.[32] Let p_j be the probability that $j \in \mathcal{J}_{ik}$ is invited to the education workshop: $p_j = 12/25$ if $j \in TG$ and $p_j = 0$ otherwise. Define $n_{ik}^S := |\mathcal{J}_{ik}|$ as the number of surveyed households within radius k of household i and n_{ik}^T as the *realized* number of surveyed households invited to the education workshop in radius k of household i. The *expected* number of surveyed households invited to the workshop in radius k of household i is $\mathbb{E}_i[n_{ik}^T] = \sum_{j \in \mathcal{J}_{ik}} p_j$. The *deviation* from the expected exposure is $n_{ik}^{T\perp} = n_{ik}^T - \mathbb{E}_i[n_{ik}^T]$ which by construction is uncorrelated with TG_i. We then estimate $y_i = \alpha + \beta\, n_{ik}^{T\perp} + \varepsilon_{ik}$ which gives the causal effect of exposure to TG-treated households on conversations for untreated households (CG and TG-untreated).

Figure 1 displays the effect of one additional exposure (β) after re-centering the realized exposures by the expected exposures according to the randomization protocol. Since the identification strategy does not provide guidance on the choice of radius for exposure, we vary the radius and report the estimates for both conversations with friends/relatives and with neighbors. In both cases, and for different radii, an exogenous increase in exposure causes untreated households to be more likely to have conversations about property taxes. Recall that—since we asked about conversations with neighbors and relatives/friends jointly—it is possible that respondents consider

[32] This notation is flexible to allowing both TG and CG households in $\mathcal{J}_{ik}$.

neighbors to be the set of neighbors who are not also their friends or relatives.

To complete this analysis, we investigate how much of the treatment effect on conversations for TG-untreated households reported in Table 2 can be accounted for by the re-centered exposure estimates and the realized exposures. To compute this, we first obtain the predicted probability of talking for TG-untreated and CG households, respectively, by multiplying their average realized exposures to TG-treated households at each radius by the treatment effect of re-centered exposure on conversations.[33] The implied treatment effect on conversations for TG-untreated relative to CG is given by the difference in these two probabilities. Table A.9 presents the results. Comparing the actual treatment effect on conversations (first row) to the implied treatment effect (last row), we find that the difference in exposure between TG-untreated and CG households can account for most of the observed treatment effect on conversations: for neighbors, the implied treatment effect ranges from 0.018 to 0.027 while the actual treatment effect estimate is 0.021; for friends and relatives, the implied effect ranges from 0.036 to 0.063 against the actual effect of 0.069.[34]

These results imply that our education intervention features a social multiplier for knowledge above 1, as the intervention's total reach exceeds the directly treated households through social interactions. Specifically, we estimate a social multiplier of 1.6 for practical knowledge (Table 1, Panel B, column 7), based on a multiplier of 1.7 for property tax related conversations (Table 2, column 1).[35] These multipliers are a lower bound, as they would be larger if unsurveyed—and hence untreated—property owners in the TG units also experienced conversation spillovers from exposure to TG-treated households.[36] The large social multipliers have policy implications as they not only broaden the reach of education programs beyond those who participate, but also increase the cost effectiveness of such programs, and it may suffice for a subset of taxpayers to participate in such workshops (which matters in practice given participation constraints; see section 4.6).

[33] Specifically, the predicted probabilities of talking conditional on TG-untreated and CG status, respectively, are $1-(1-\beta)^n$, where β is the re-centered estimate from row two and n is the average number of TG-treated households within a given radius for TG-untreated and CG, respectively.

[34] In the latter case, we omit the implied treatment effect at 100m radius, since it is effectively null.

[35] To compute these social multipliers, recall that each TG unit contains 13 untreated and 12 treated households. The multiplier on conversations then is $(0.11 \times 13 + 0.16 \times 12)/(0.16 \times 12)$ and the multiplier on practical knowledge is $(13 \times 0.32 + 12 \times 0.54)/(12 \times 0.54)$.

[36] This is plausible based on our sampling strategy: if enumerators complied with the sampling protocol, which instructed enumerators to survey every fourth household, unsurveyed households in TG units should on average be physically closer to TG-treated households than the surveyed TG-untreated households. On the other hand, having been surveyed at baseline may make TG-untreated households more inclined to engage in conversations related to property taxes than unsurveyed households.

7 Results on tax outcomes

Our intervention successfully raised knowledge on both the taxpayer and the tax official side. For taxpayers, the effects extend beyond the directly treated households: TG-untreated households communicate with TG-treated neighbors and gain practical knowledge on taxation. We next look at the downstream effects of this increased knowledge on the main public finance outcomes.

Table 3 shows treatment effects on the main public finance outcomes of interest. We find null effects across all outcomes. Relative to control households, neither TG-treated nor TG-untreated households are more likely to have received a visit by a tax official, received their property tax bill, registered for the online tax portal, or made any tax payment.[37] The total amount paid also does not differ between control households and TG-treated or TG-untreated households. The null results occur in a setting where there was substantial scope to increase payments, given that the compliance rate among CG households is only 12%.

Table A.10 shows the results using the saturated specification. Again, we find null effects across all outcomes, for all treatment groups, and both for TG-treated and TG-untreated households including for those in TG3, suggesting that increasing knowledge simultaneously among taxpayers and tax officials is not, in this setting, sufficient to raise tax compliance. Table A.11 shows the results remain controlling for whether the tax official speaks the primary indigenous local language.

Finally, the average null result could mask significant heterogeneity, if education interacts with other household characteristics to determine compliance. In Table A.1, we use the same baseline characteristics as for the previous heterogeneity analyses (income/wealth; past tax history; tax planner motivations; local government satisfaction). We find no heterogeneous treatment effects on online portal registration, tax payment, and payment amount, mirroring the null heterogeneity results on knowledge and selection into workshop.

These null results are based on the self-reported household survey data, so response bias could be a concern for interpreting the levels of these variables. However, the rate of tax payments reported by households in the control group, at 12%, is very close to the actual rates of payment observed in Ghana and other settings with limited enforcement capacity.[38] We confirm the tax results using

[37] While the predominant mode of delivery is by the official in person, in principle, other options exist. Table A.13 shows null effects on bill receipt across all delivery modes, including accessing the bill online or by visiting the assembly.

[38] Similarly, the difference between the payment rate (12%) and the bill delivery rate (31%) suggests a likelihood of

two independent data sources.[39] First, administrative daily reports submitted by tax officials to their supervisors show no treatment effects on the number of visits or registrations (Table 4). The only exception is a negative effect on number of registrations in TG2, but this is not similarly observed in the household survey data (neither among TG2-treated nor TG2-untreated).

Second, we use the GPS data collected from the tax officials' work tablets, which continuously record tablet location in the field.[40] We assign tax officials' locations to households using property GPS coordinates from the household surveys with a 20-meter radius to balance measurement error against spatial resolution in Accra's urban environment. We confirm robustness for radii between 10-80 meters.[41]

Panel A of Table 5 shows that TG-treated households were no more likely to receive a visit by the tax official according to the GPS data, nor did they spend more time interacting with tax officials. There are also no effects on TG-untreated households. Panel A of Table A.12 confirms these null results across the three treatment groups, including TG3 where tax officials themselves received the education. Hence, across three independent data sources (household survey, daily official reports, continuous GPS records), we find no treatment effects on visits by tax officials.

Finally, Table 4 shows no treatment effects on multiple measures of field activities from the weekly tax official survey. Specifically, officials in TG1, TG2 or TG3 do not report working more days, working more hours per day, having more successful visits or being more satisfied with their job than CG officials. Officials in TG1 and TG2 do report less involved supervisors, which if anything should lead to negative treatment effects. On the whole, the absence of effects on broad measures of officials' activities is consistent with the null effects on tax outcomes.

Decisively then, our results show that while the education intervention raised knowledge, both among TG-treated and exposed TG-untreated households and among tax officials, there are no effects on downstream tax outcomes. In the following section, we investigate mechanisms which can potentially rationalize these two sets of results.

payment conditional on receiving a bill which is close to other studies in Ghana (e.g., Dzansi et al., 2025).

[39] The survey data is collected after the end of the six week campaign, which marks the official time where property owners were legally required to pay their property taxes.

[40] Using specifications (3) and (4), we check whether treatment affects the likelihood of reporting any GPS data (i.e. turning on the tablet and its location feature) and find no effect across any of the three treatment groups.

[41] The study used OpenPATH (NREL's open-source mobility platform) on the tax officials' work tablets with an accuracy of 10-20 meters. Moreover, in dense areas of Accra, residential plots can be relatively small, so a 20-meter radius is large enough to encompass entire properties while preserving accuracy

8 Mechanisms

This section investigates mechanisms that can explain the absence of effects on tax outcomes despite positive knowledge effects. First, the intervention may shift which households the officials decide to visit. Second, conditional on a visit, knowledge may affect the nature of interactions. Third, increased knowledge may shift households' tax morale or enforcement perceptions.

8.1 Targeting of visits by tax officials

In the first mechanism, the education intervention may have affected tax officials' field strategies. Far fewer than all property owners receive even a single visit, and targeting of visits has been found to be an important dimension of official behavior in Ghana (Dzansi et al., 2025). Concretely, in settings with limited enforcement, if officials know which household characteristics predict propensity to pay, targeting those households for visits and interactions can be a meaningful strategy for maximizing expected pay-off.

The previous section showed that tax officials in treated areas did not target TG-treated households for more interactions or visits (Table 3, columns 1-2; Table 5, Panel A). This result could reflect either an absence of targeting of educated households by tax officials, or limited knowledge among them of which households are educated. Our TG2 treatment arm was specifically designed to disentangle these two explanations by highlighting two areas on the tax official's collection unit map that contained a higher share of TG-treated households.

To test for targeting, we estimate whether tax officials systematically targeted or avoided areas displayed to them as high-density treatment areas. Formally, we estimate the equation

$$y_{ij} = \alpha + \beta\, \mathbb{1}(\text{Counterfactual}_{ij}) + \gamma\, \mathbb{1}(\text{Highlighted}_{ij}) + \phi \mathbb{1}(V_j \geq V_{(50)}) + \varepsilon_{ij} \tag{5}$$

where $\mathbb{1}(\text{Highlighted}_{ij})$ equals 1 if a taxpayer i lives in the highlighted area of treated collection unit j which was randomly assigned to TG2 or TG3. We construct $\mathbb{1}(\text{Counterfactual}_{ij})$ analogously: it equals 1 for a taxpayer i residing in a CG or TG1 unit who *would have been* highlighted had the unit been randomly assigned to TG2 or TG3. Including these counterfactually highlighted areas accounts for differences due to a higher density of properties rather than a higher *treated*-property density. β is the ITT estimate for the average effect of living in the "counterfactual" highlighted

area of a collection unit and γ is the ITT estimate for the average effect of living in an actually highlighted area.

We estimate equation (5) using GPS data from the tax officials' tablets. Panel B of Table 5 shows no treatment effects on visits or time spent interacting in areas highlighted in TG2 and TG3 as having more educated households. More precisely, being in an actually highlighted area has no causal effect on interactions relative to being in an area that is likely to be highlighted but is not actually highlighted (the counterfactual area). Panel B of Table A.12 confirms that these results hold across treatment arms, including TG3 where officials themselves were more educated.

Even if the intervention did not affect officials' targeting of educated households directly, it could alter other targeting strategies in ways that interact with the education level of the household. To account for the null effect on tax payments, the intervention would have to offset characteristics that are otherwise associated with higher propensity to pay taxes. For example, households more satisfied with the local government may be more likely to make a payment but the education intervention may make them more aware of flaws in the collection process reducing their propensity to pay taxes. Officials may then allocate fewer visits to treated households that are more satisfied and more visits to treated households that are less satisfied, resulting in null effects on both visits and payments among treated households.

Table A.14 investigates whether the education intervention causally shifts which type of households tax officials target for visits. As proxies for targeting characteristics, we use the same baseline household characteristics as in the heterogeneity analyses. For both visits and bill delivery, we find no differential targeting on any of these characteristics based on whether the household is TG-treated or TG-untreated. For example—and lending credence to the data—while past tax history (having received a bill or made a payment including in recent years) is associated with more targeting by tax officials, a household's treatment status does not alter the extent to which the official targets based on this characteristic.[42]

These results suggest that in a (somewhat) realistic setting, the level of education of property owners is not an important household characteristic that officials organize their fieldwork around, nor does it interact with their other targeting strategies. Thus, differential targeting cannot reconcile positive effects on knowledge with null effects on tax outcomes. Moreover, the absence

[42]Consistent with this, Table A.15 uses the weekly collector surveys and shows there are no systematic differences across treatment groups in the extent to which officials target any household characteristic, including knowledge.

of differential targeted visits means that any treatment effects on the nature of interactions, conditional on a visit, reflect how the intervention affects the interaction itself (rather than how the intervention affects the composition of interactions). We turn to this analysis in the next subsection.

8.2 Nature of interaction conditional on visit

A second mechanism through which the intervention could increase knowledge of households but not the likelihood of tax payments may be that the intervention changed the nature of interactions rather than their frequency. More knowledgeable treated households may now pose questions that tax officials, given their own limited knowledge, may not satisfactorily address, leaving these households unable or unwilling to act further. More generally, increased knowledge due to the intervention may not by itself generate sufficient motivation or engagement for either side to engage in genuinely collaborative interactions, which may be needed to unlock payments.

Table 6 provides the results on the nature of interactions.[43] Importantly, the questions on interactions were only answered by households if they reported any visit by tax officials; therefore, Table 6 conditions on the households that reported at least one such visit. Since the previous subsection showed no differential targeting of visits by officials, any effects on the nature of interactions during these visits reflect how the intervention affected the interaction itself (rather than selection into interactions).

Taxpayers' perceptions of the nature of interactions Column 1 of Panel A shows strong positive treatment effects on households' self-assessment, an index capturing how often during interactions the household felt satisfied, felt useful, did not feel tired, stressed or frustrated. Remarkably, the effect is almost as large for TG-untreated households as for TG-treated households. Moreover, Table A.17 shows that the effect for TG-untreated households is concentrated in TG1 and TG2, not TG3 where officials received more education themselves. These results suggest that education increases households' positive feelings of satisfaction and usefulness during interactions with officials, including among those receiving knowledge through social spillovers (TG-untreated).

In columns (2) and (3), both TG-treated and TG-untreated households report that the tax official was helpful and provided information. Column (2) measures the household-reported share of

[43] Table A.16 shows robustness of the results controlling for whether the tax official speaks the primary indigenous local language (Ga).

interactions where the tax official "made good arguments or arguments that should be taken seriously," "was able to answer questions or respond to requests" and "provided information which helped clarify how property taxes are determined or processed." Column (3) measures the household-reported share of interactions where officials "provided information on use of property taxes by the assembly" and "provided information about why property owners have to pay property taxes." Interestingly, Table A.17 shows that among TG-untreated households, these effects are again driven by TG1 and TG2 rather than TG3 where officials were actually more educated. This result suggests two things: first, these questions reflect more on the household making use of the tax official in the interaction for their own purposes than on the officials' engagement in the interaction; second, consistent with column (1), TG-untreated households are genuinely engaging in the interactions with specific objectives in mind.

In column (4), there are no treatment effects on whether the tax official exhibited certain "good" characteristics—as in, displaying authority, avoiding conflict and obstruction, behaving predictably, being eager to interact, and being cordial and respectful. This null result, combined with the positive effects in columns (2) and (3), could suggest that the positive treatment effects on dimensions of interactions are confined to the households' assessment of themselves in the interaction rather than the tax official.

In column (5), both TG-treated and TG-untreated households are more likely to report willingness to do only the minimum required to interact with officials. This may reflect that knowledgeable households, who are now better able to engage in interactions, have higher expectations from interactions and become frustrated by the official's limited ability or willingness to reciprocate. Similarly, the treatment effect may reflect that households with more knowledge assess, based on interacting with the tax official, that the process is not progressing and therefore prefer to seek alternative ways to complete the process (such as by going directly to the assembly office).

Consistent with these results, Panel B shows that the intervention did not move the needle on households' broader assessments of the nature of their interactions with tax officials. In column (6), there is no treatment effect on whether the household considers the interaction to be helpful for achieving their own objectives; in column (7), there is no treatment effect on future uncertainty over interactions or ability to improve them.[44]

[44]More specifically, the outcome in column (7) is the household's average agreement with the following statements: "it takes a long time to find evidence to respond to others' arguments", "interactions were challenging because of not having a good sense of future behavior" and "it was difficult to improve on the interactions" and "it is unclear how

Finally, columns (8) to (11) display results for four mutually exclusive categories of relationship discussed in the public administration literature on frontline bureaucrats (Raaphorst and Loyens, 2020; Nielsen et al., 2021; Döring et al., 2025). Each household was asked to characterize the relationship with officials as one of: obstruction, with the other party not prepared or willing to engage (column 8); the other party unresponsive, but the household has a clear sense of next steps (column 9); the other party motivated and willing to engage, with the household following up on their concerns (column 10); or collaborative, with both parties motivated and engaging in cordial interactions and coordinating on next steps (column 11).

The collaborative relationship was defined in Section 3, where we conjectured that the intervention could have shifted more interactions into this collaborative category, away from obstructive or non-responsive categories. We find no such effects; instead, consistent with Panel A, the only positive treatment effect is in column (9), indicating a larger share of households report having a clear sense of next steps even as the tax official is viewed as unresponsive. Also consistent with Panel A, the effects hold for both the TG-treated and TG-untreated households.

Tax officials' perceptions of the nature of interactions Part II of Table 6 investigates the same outcomes for tax officials. In the first column of Panel C, there are no treatment effects on the extent to which officials feel positively about themselves in the interactions with households, across all three treatment arms, including TG3 where officials are more educated. In the second column, we find positive treatment effects on officials' reported share of interactions where the household was "knowledgeable about property taxes" and "communicated clearly." This effect is large outside of TG3, which indicates that it genuinely reflects an official's improved assessment of more educated households.[45] In the third column, there is a positive effect in TG3 on the official's reported share of interactions where households "provided information which helped clarify how property taxes are determined or processed." Since there are also more educated households in TG1 and TG2, this may reflect greater engagement in interaction by educated officials in TG3 rather than improvement in information provided by households; indeed, the coefficients in TG1 and TG2 are weakly negative. In the fourth column, there are no effects on the official's perception of the households' helpful characteristics. In the fifth column, there are no meaningful effects on the official's willingness to do only the bare minimum in interactions. Finally, in Panel D, there

property tax laws and regulations should be interpreted and applied".

[45]The survey did not ask officials to identify specific households they had interacted with, so we cannot investigate if this effect comes from their interactions with TG-treated households, TG-untreated households, or some combination of the two.

are no treatment effects on any measure of the tax officials' overall assessment of the interactions with households, including a null effect on collaborative relationships.

The results from tax official surveys are therefore more muted but broadly consistent with the household results: educating officials increases their engagement with households, but does not shift their overall assessment of the interactions. Unlike on the household side, officials perceive the genuine improvement in knowledge among the households they interact with.

Taken together, these results show that the education intervention increased respondents' assessment of their own usefulness and competencies in the interactions. For households, this extended to TG-untreated households, suggesting that the increased sense of agency in the interactions can stem from practical knowledge and conversations rather than formal knowledge of the tax system. At the same time, neither the TG-treated households nor the treated tax officials shifted their assessment of the other party in the interaction and there was no move towards genuinely collaborative relationships. These partial effects on interactions can reconcile the positive effects on knowledge with the null results on public finance outcomes, if increasing tax payments requires a shift to interaction where both parties collaborate towards those objectives.

8.3 Tax planning, morale and enforcement

In this subsection, we explore a third mechanism which may account for the observed positive treatment effect on knowledge and the null effect on tax outcomes: the intervention may have shifted households' inclination for tax planning (making legal use of the tax code to reduce their tax burden), tax morale (non-pecuniary motivations for compliance), or enforcement perceptions.

Tax planning The education intervention may have made property owners more of a 'tax planner', that is, a taxpayer who seeks legal ways to reduce their tax liability or negotiate favorable terms of payment (Craig and Slemrod, 2024). In the context of property taxation in Ghana, three concrete actions are available to a taxpayer: request a bill reduction (due to economic hardship or errors in initial computation of liability); request a property revaluation (if the assessed value of the property is incorrect); request to pay the tax liability in installments.

For each action, Table 7 reports treatment effects on whether the household knows about the action, has begun to undertake the action, and is likely to undertake it in the future. For all three actions, and both for TG-treated and TG-untreated households, we find positive treatment effects

on knowing about the action but null results on undertaking the action—both in the present and future. In the saturated specification, Table A.18 shows that the effects for the TG-untreated households are not confined to TG3.

Table A.19 investigates why tax planning actions were not taken, and shows that households have sophisticated reasons for not engaging in tax planning. TG-treated households are more likely to say that they do not request bill reduction because they have an obligation to make the full payment or feel they can make the full payment—a sign of restraint. Both TG-treated and TG-untreated households are more likely to anticipate that the assembly will not grant their request—a sign of forward-looking reasoning. Both TG-treated and TG-untreated households are more likely to say that they do not request a property revaluation because they do not think the reasons for doing so are justified in their case—a sign of nuanced understanding of the taxation process. Finally, across all three actions, TG-treated households are more likely than control households to say that they do not take the action because they did not receive the bill—which also highlights a robust understanding of where these actions fit in the taxation process.

The education intervention therefore raised awareness of available tax planning actions, but it also improved households' understanding of why such actions may not be effective or justified in their specific case. These results provide an intuitive channel for the positive knowledge effect to translate into a null effect on tax payments.

Tax morale This subsection explores whether the education intervention affected households' tax morale, that is, the non-pecuniary motivations for tax compliance. The first five columns of Table 8 show that there are no treatment effects, neither for TG-treated nor TG-untreated, on five different components of tax morale. Table A.20 confirms this in the saturated specification.

We also showed earlier that tax morale at baseline was not a source of treatment heterogeneity for selection into the workshop, knowledge, differential targeting by tax officials, or ultimate tax outcomes. While policy reports frequently justify taxpayer education campaigns as a means of strengthening tax morale (Section 1), our results suggest that, in this setting, exposure to educational content neither improves tax morale nor interacts with baseline morale to increase tax compliance.[46] For the purposes of our mechanism investigation, the null effect on tax morale can

[46]While tax morale has generally been thought of in relation to the decision to comply with taxes, a potential interpretation of the tax planning results in Table A.19 would be that TG-treated households' signs of restraint are a behavioral expression of another dimension of morale. We thank Daniel Overbeck for pointing us to this interpretation.

help to account for the null effect on tax payment.

Enforcement capacity Finally, we explore whether the education intervention affected households' perception of local government's enforcement capacity. In Ghana, as in most developing countries, local governments have limited capacity to enforce taxes on property owners, and households with lower perceptions of this capacity may be less likely to comply. Intentionally, our education intervention was designed to provide no information on the local government's enforcement capacity—neither directly (e.g., disclosing the compliance rate in recent years) nor indirectly (e.g., commenting on the local government's tax collection performance).

Our intervention could nonetheless impact enforcement perceptions. For instance, treated households, who learned about the tax collection process, could become interested in following up and learning about how well and effectively the collection works in practice. By generating more conversations with others about taxation, the intervention may induce more sharing of information between people about enforcement practices and outcomes.

In column (6) of Table 8, we observe that both TG-treated and TG-untreated households assess the local government as having lower enforcement capacity than control households. Importantly, control households incorrectly believe that enforcement is stronger than it really is. In other words, TG-treated and TG-untreated households correctly update their beliefs about local enforcement. Table A.20 confirms that the effect among TG-untreated households is not confined to TG3, where officials themselves were more educated.

These results suggest that tax education campaigns may have unintended consequences from the government's perspective, as they can raise knowledge but in ways that could undermine the government's objectives (Craig and Slemrod, 2024). This is a cautionary finding in settings, such as local taxation in Ghana, where enforcement is already limited to begin with and education could backfire by further eroding government's perceived enforcement capacity. Previous work has shown that direct exposure to the enforcement process can affect perceptions of enforcement capacity (Kleven et al., 2011); our results on belief updating among TG-untreated households provide experimental evidence on indirect channels, where enforcement perceptions are shaped by social conversations with more knowledgeable taxpayers.[47]

The downwards revision in beliefs about enforcement capacity could in principle cause a reduction

[47]While our focus is on knowledge shocks and individual taxpayers, settings with firms show that enforcement shocks propagate through production and professional networks (Boning et al., 2020; Almunia et al., 2025).

in tax compliance. We observe no such effects (Table 3), but the shift in enforcement beliefs may not be large enough to produce a detectable effect on tax payments. For the purposes of our mechanism investigation, however, the negative effect on enforcement perceptions is consistent with the intervention causing an increase in knowledge but no increase in tax payment.

9 Conclusion

Taxpayer education programs worldwide are often designed based on the assumption that better-informed taxpayers are more likely to comply. Yet knowledge of rights and responsibilities may facilitate avoidance or evasion, and the ultimate effects of knowledge depend on interactions with officials who exercise discretion over the collection process. Our multi-arm property tax experiment randomly educated taxpayers, informed officials about educated households' locations, and educated officials themselves. Directly treated households gained declarative and practical knowledge; untreated households in treatment units gained practical knowledge through conversations with educated neighbors, implying a social multiplier for knowledge well above 1.

These gains produced no detectable improvements in tax outcomes. Knowledge improved taxpayers' self-assessments during interactions with officials but did not foster co-productive relationships potentially needed to improve tax outcomes. Households learned legal ways to reduce their tax burden without becoming more likely to use them, partly recognizing these actions as inapplicable or unlikely to succeed. Tax morale remained unchanged, while households revised downward their initially inflated beliefs about enforcement capacity.

Our findings illustrate both the potential of taxpayer education and the limits of its effects on compliance. Social transmission extends knowledge gains beyond direct recipients, and greater knowledge can increase taxpayers' ability and willingness to engage with the state. Over time, this could strengthen accountability and generate demand for better-trained officials, potentially improving quasi-voluntary compliance. Yet, knowledge is a double-edged sword: it enables taxpayers to engage more effectively with the state but also allows them to recognize the constraints on the state's enforcement capacity. Without improvements in enforcement capacity, it may erode the revenue gains that greater quasi-voluntary compliance could otherwise deliver. Taxpayer education may therefore be most effective as part of a broader reform agenda that simultaneously strengthens taxpayers' knowledge and the state's capacity to enforce taxation.

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Tables

Table 1: Treatment Effects on Taxpayer and Tax Official Knowledge

		Declarative Knowledge Questions					
	(1) Index Over (2)-(5)	(2) Formula	(3) Process [Index]	(4) Responsibilities	(5) Rights	(6) Self-Perceived Knowledge	(7) Taxpayer Action [Index]
Panel A: Taxpayer Midline Survey							
TG untreated	−0.08	−0.16	−0.09	−0.11	−0.07	0.07	
	(0.07)	(0.13)	(0.10)	(0.15)	(0.11)	(0.08)	
TG treated	0.20**	0.22	0.29**	0.22	0.19	0.06	
	(0.08)	(0.15)	(0.10)	(0.19)	(0.16)	(0.08)	
CG mean	-0.05	-0.02	-0.07	-0.07	-0.07	0.26	
Observations	319	319	319	319	319	313	
Panel B: Taxpayer Endline Survey							
TG untreated	0.01	−0.14	0.15	−0.10	0.05	0.11*	0.32***
	(0.06)	(0.14)	(0.11)	(0.13)	(0.09)	(0.06)	(0.12)
TG treated	0.16**	0.04	0.40***	0.04	0.18**	0.18***	0.54***
	(0.07)	(0.16)	(0.13)	(0.13)	(0.08)	(0.05)	(0.13)
CG mean	-0.08	0.05	-0.22	0.03	-0.11	0.24	-0.34
Observations	1108	1108	1108	1108	1108	1108	1108
Panel C: Tax Official Midline Survey							
TG3 (education)	0.38	0.78**	0.09	0.58	1.46***		
	(0.26)	(0.36)	(0.46)	(0.47)	(0.38)		
CG mean	-0.12	-0.28	-0.00	-0.18	-0.53		
Observations	21	21	21	21	21		

Notes: Panels A and B show ITT effects on taxpayers' knowledge estimated using equation (1) and including strata FE. Panel C shows ITT effects on tax officials' knowledge estimated using equation (3) among the sample that answered *all* relevant questions. Column (1) combines the knowledge scores in columns (2) to (5) into a combined knowledge index expressed as an overall Z-score. The outcomes in columns (2) to (5) are standardized scores for a set of open-ended knowledge questions covering the property tax formula, the tax collection process, taxpayer responsibilities, and taxpayer rights. These questions are manually graded on a scale from 0 to 10 and are then converted into Z-scores. Column (3) is itself an index over multiple open-ended questions, all of which are graded on a 0 to 10 scale and converted into Z-scores before they are averaged into the process-index. For the midline survey, the process-index combines open-ended questions on: the institution setting tax rates; the institution determining property valuations; what steps taxpayers can take when there are issues with the property tax liability; legal ways to seek a lower liability or easier payment process; and explanations for why property tax liabilities have increased compared to 2024. For the endline survey (due to fewer questions asked), the process-index combines the questions on the institutions for tax rate and property valuation, and the process in case of issues with property taxes. Column (6) asks about self-perceived knowledge on property taxes and is expressed as a binary outcome equal to 1 if the respondent considers themselves at least "somewhat" knowledgeable. Column (7) is a combined index for whether respondents know about possible actions they can take (request lower liability, request revaluation, make payments in installments). Standard errors in Panels A and B are clustered at the geographic collection unit level. Appendix Table A.7 shows the corresponding saturated specification. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table 2: Treatment Effects on Talking about Property Taxes

	Conversation Partner			Topic of Conversation					
	(1) Speak to Anyone	(2) Speak to Relative/Friends	(3) Speak to Neighbors	(4) Split Payment	(5) Gather Funds	(6) Lower Liability	(7) Process	(8) Taxpayer Duties	(9) Taxpayer Rights
TG untreated	0.11***	0.07**	0.02	0.02	0.03*	0.03***	0.05**	0.03**	0.05***
	(0.04)	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
TG treated	0.16***	0.10***	0.04***	0.03*	0.05**	0.03***	0.07***	0.04**	0.06***
	(0.04)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
CG mean	0.04	0.03	0.01	0.01	0.01	0.00	0.02	0.01	0.00
Observations	1108	1108	1108	1108	1108	1108	1108	1108	1108

Notes: Shows ITT effects estimated using equation (1) and including strata FE. Column (1) captures whether the taxpayer ever talked to anyone about property taxes in the past five weeks (apart from their interactions with a tax official). Column (2) captures whether the taxpayer talked to relatives or friends about property taxes and column (3) whether they talked to neighbors about property taxes. Column (4) captures whether the taxpayer talked about how to split the property tax bill within the family or people in charge, column (5) whether they talked about how to gather funds to make a payment, column (6) whether they talked about things they can do to lower a bill amount, column (7) whether they talked about the collection and payment process, column (8) whether they talked about the duties of property taxpayers, and column (9) whether they talked about the rights of property taxpayers. Standard errors are clustered at the geographic collection unit level. Appendix Table A.8 shows the corresponding saturated specification. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table 3: Treatment Effects on Public Finance Outcomes

	(1) Tax Official Visit	(2) Received Bill	(3) Account Tax Portal	(4) Made Any Payment	(5) Payment Amount
TG untreated	0.02	0.01	−0.01	−0.02	−17.39
	(0.06)	(0.08)	(0.03)	(0.04)	(24.38)
TG treated	0.03	−0.01	−0.01	0.01	−8.01
	(0.06)	(0.07)	(0.03)	(0.04)	(22.51)
CG mean	0.13	0.31	0.08	0.12	72.66
Observations	1108	1108	1108	1108	1108

Notes: Shows ITT effects estimated using equation (1) and including strata FE. Column (1) captures whether the taxpayer reports a visit from a tax official during the collection campaign, column (2) whether they received a bill, column (3) whether they [know they] have an account on the local government's digital tax portal, and column (4) whether they have made any payment. The outcome in column (5) is the amount of payment made (coded as 0 if no payment was made). Standard errors are clustered at the geographic collection unit level. All outcomes are from the taxpayer endline survey. Appendix Table A.10 shows the corresponding saturated specification. Appendix Table A.11 shows the pooled specification controlling for whether the tax official speaks the predominant local language. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table 4: Tax Officials' Activities

	Weekly Survey					Daily Reports	
	(1) Work Days	(2) Working Hours per Day	(3) % Visits Successful	(4) Satisfied with Work	(5) Supervisors Activity	(6) Number of Visits	(7) Number of Registrations
TG1	−0.20	−0.55	−10.79	−0.21	−1.57***	5.90	−3.50
	(0.25)	(0.83)	(8.09)	(0.76)	(0.54)	(7.47)	(4.69)
TG2	0.25	−1.51*	2.48	−0.55	−2.47**	17.51	−7.68***
	(0.16)	(0.83)	(11.86)	(0.93)	(1.04)	(13.74)	(2.01)
TG3	−0.03	−0.67	1.27	−0.25	−0.72	10.97	3.61
	(0.19)	(0.72)	(6.33)	(0.69)	(0.48)	(7.46)	(5.11)
CG mean	4.67	4.80	63.28	8.12	9.18	49.70	11.10
Observations	141	141	117	117	117	144	144

Notes: Shows ITT effects on tax officials' collection activities estimated using equation (4) and including week FE. Column (1) captures the number of working days, column (2) the average number of working hours per day, column (3) the percentage of successful visits, column (4) their satisfaction with work, and column (5) the involvement of supervisors. Column (6) records the official's reported number of visits to taxpayers (aggregated on the week) and column (7) captures the official's reported number of taxpayer registrations on the digital portal (aggregated on the week). Standard errors are clustered at the tax official level. p-values * ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01

Table 5: Targeting Taxpayers Based On Education Status

	(1) CSO Visited	(2) Minutes (20m)
Panel A: Main Specification		
TG untreated	0.025	−0.436
	(0.056)	(0.493)
TG treated	0.035	−0.282
	(0.056)	(0.528)
CG mean	0.131	1.478
Observations	1108	1629
Panel B: Highlighted Specification		
Counterfactual	−0.002	−0.439
	(0.052)	(0.404)
Highlighted	0.026	−0.230
	(0.032)	(0.334)
CG mean	0.131	1.478
Observations	1108	1629

Notes: Shows ITT effects estimated using equation (1) in Panel A and equation (5) in Panel B. Column (1) captures whether the taxpayer reports at the endline a visit from a tax official during the collection campaign. Column (2) captures time (in minutes) that the tax official spent per week within 20 meters of the taxpayer's property as captured by tax officials' tablet GPS. Section 7 describes the construction of the GPS-based data. Column (2) is at the household-by-week-level and includes week fixed effects. Standard errors are clustered at the geographic collection unit level. Appendix Table A.12 shows the corresponding saturated specification. p-values * ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01

Table 6: Perceptions of Nature of Interactions between Taxpayers and Tax Officials

I. Taxpayer Perceptions

Panel A: Taxpayers' Perceptions of Dimensions of Interactions

	(1) Feeling About Oneself	(2) Tax Official Helpful	(3) Tax Official Provided Information	(4) Tax Official Good Characteristics	(5) Self Only Willing to Do Bare Minimum
TG untreated	13.11***	16.26**	18.16**	4.63	30.70**
	(4.45)	(7.83)	(7.62)	(4.91)	(14.25)
TG treated	15.40***	19.42**	14.55**	3.37	27.18*
	(4.99)	(7.73)	(6.93)	(4.22)	(14.07)
CG mean	55.17	16.44	11.42	62.19	25.83
Observations	166	166	166	166	166

Panel B: Taxpayers' Overall Assessment of Interactions

			Public Administration Literature Interaction Measures			
	(6) Interaction Helpful for Own Objectives	(7) Uncertainty	(8) Tax Official Obstructive	(9) Not Responsive But Next Steps Clear	(10) Motivated & Engaged Followed Up Info/Requests	(11) Cordial & Respectful Together Next Steps
TG untreated	19.06	0.01	−0.02	0.11**	−0.01	0.02
	(12.93)	(0.09)	(0.09)	(0.05)	(0.12)	(0.10)
TG treated	18.23	0.04	−0.01	0.08**	−0.08	0.11
	(13.18)	(0.09)	(0.08)	(0.03)	(0.10)	(0.11)
CG mean	30.33	0.27	0.10	0.00	0.53	0.13
Observations	166	166	166	166	166	166

II. Tax Officials Perceptions

Panel C: Tax Officials' Perceptions of Dimensions of Interactions

	(1) Feeling About Oneself	(2) Taxpayer Helpful	(3) Taxpayer Provided Information	(4) Taxpayer Good Characteristics	(5) Self Only Willing to Do Bare Minimum
TG1	2.60	10.95***	−2.84	0.64	1.70
	(8.23)	(3.27)	(3.94)	(3.97)	(2.80)
TG2	−0.02	3.37	−17.58*	−2.59	3.94
	(8.92)	(4.94)	(9.32)	(3.35)	(4.66)
TG3	0.33	12.07***	13.27**	1.81	8.67*
	(6.52)	(2.10)	(4.74)	(2.88)	(4.39)
CG mean	67.34	51.58	44.83	60.17	52.83
Observations	141	141	141	117	141

Panel D: Tax Officials' Overall Assessment of Interactions

			Public Administration Literature Interaction Measures			
	(6) Interaction Helpful for Own Objectives	(7) Uncertainty	(8) Taxpayer Obstructive	(9) Not Responsive But Next Steps Clear	(10) Motivated & Engaged Followed Up Info/Requests	(11) Cordial & Respectful Together Next Steps
TG1	1.42	−0.06	−3.15	−3.49	1.41	0.98
	(5.91)	(0.08)	(3.46)	(2.45)	(3.78)	(4.19)
TG2	−0.73	−0.10	1.68	−0.30	1.08	0.69
	(6.63)	(0.06)	(3.89)	(2.36)	(3.07)	(4.10)
TG3	−1.36	−0.08	5.81	−2.04	2.95	0.03
	(5.31)	(0.07)	(3.72)	(2.55)	(3.77)	(3.39)
CG mean	70.83	0.42	19.60	22.20	31.50	33.10
Observations	141	141	141	117	141	141

Notes: Panels A and B show ITT effects for taxpayers estimated using equation (1) and including strata FE; Panels C and D show ITT effects for tax officials estimated using equation (4) and including time FE. All outcomes are indices unless noted and described in detail in section 8.2. Column (1) captures respondents' personal experiences of the interaction. Column (2) captures whether the other party was perceived as helpful. Column (3) captures whether the other party provided useful information. Column (4) captures whether the other party showed good characteristics in the interaction. Column (5) captures whether the respondent was only willing to do the bare minimum in the interaction. Column (6) captures whether the interaction was useful for one's own objectives. Column (7) captures whether uncertainty remained after the interaction. Column (8) captures whether the other party was obstructive. Column (9) captures whether one has a clear sense of next steps despite the other party not being responsive. Column (10) captures whether the other party was motivated and engaged and one followed up on the interaction. Column (11) captures whether the other party was motivated and the interaction was cordial and respectful. Standard errors are clustered at the geographic collection unit level. Table A.17 shows the corresponding saturated specification for Panels A and B. Table A.16 shows the pooled specification controlling for whether the tax official speaks the predominant local language. p-values * ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01

Table 7: Treatment Effects on Taxpayer Actions

	Request Bill Reduction			Request Property Re-valuation			Request Payment in Installments		
	(1) Knows About	(2) Requested	(3) Likely in Future	(4) Knows About	(5) Requested	(6) Likely in Future	(7) Knows About	(8) Requested	(9) Likely in Future
TG untreated	0.13**	0.03	−0.03	0.14***	0.01	−0.04	0.11*	−0.01	0.02
	(0.05)	(0.02)	(0.04)	(0.04)	(0.01)	(0.06)	(0.07)	(0.03)	(0.06)
TG treated	0.27***	0.02	−0.01	0.22***	0.01	−0.09	0.17**	−0.00	0.02
	(0.05)	(0.02)	(0.04)	(0.05)	(0.01)	(0.06)	(0.07)	(0.03)	(0.06)
CG mean	0.26	0.04	0.84	0.16	0.01	0.71	0.32	0.08	0.75
Observations	1108	1108	1108	1108	1108	1108	1108	1108	1108

Notes: Shows ITT effects estimated using equation (1) and including strata FE. Columns (1), (4) and (7) capture whether the taxpayer knows about action A; columns (2), (5) and (8) whether the taxpayer has requested action A; and columns (3), (6) and (9) whether the taxpayer is likely to request action A where $A \in \{$bill reduction, property revaluation, request payment in installments$\}$. Standard errors are clustered at the geographic collection unit level. Table A.18 shows the corresponding saturated specification. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table 8: Tax Morale, Enforcement and Corruption Views

	Tax morale				Views	
	(1) Satisfaction and Competence	(2) Attend Public Meetings	(3) Fund Local Government	(4) Favorable Views of Platform	(5) Corruption	(6) Enforcement
TG untreated	−0.018	0.018	−0.016	−0.006	−0.022	−6.010**
	(0.043)	(0.041)	(0.051)	(0.021)	(0.084)	(2.489)
TG treated	−0.028	0.027	−0.037	−0.011	−0.024	−4.394*
	(0.041)	(0.039)	(0.051)	(0.022)	(0.085)	(2.611)
CG mean	0.214	0.812	0.269	0.616	0.581	56.178
Observations	1108	1108	1108	1108	1108	822

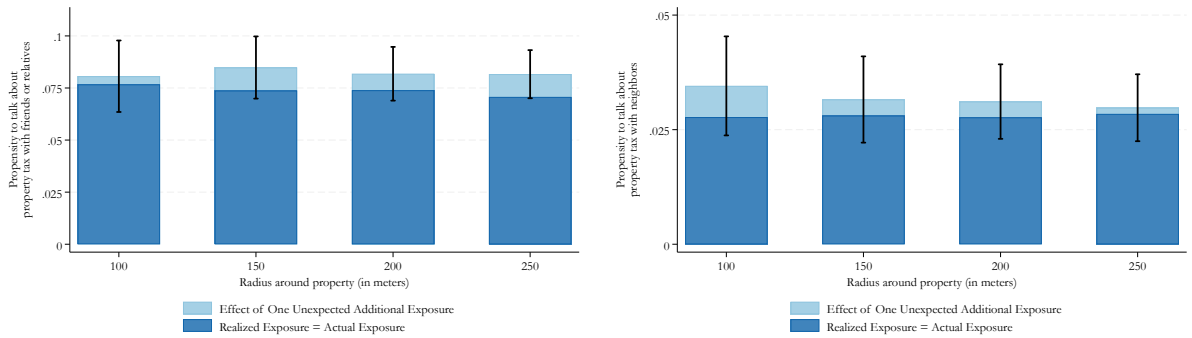
Notes: Shows ITT effects estimated following equation (1) and including strata FE. Column (1) reports the average of whether the "taxpayer is satisfied with the overall quality of public goods and services delivered by the assembly" and whether the "taxpayer finds the assembly competent". Column (2) captures whether taxpayer attends public meetings of the assembly. Column (3) captures the fraction of funds that the taxpayer allocates to the municipal assembly in a hypothetical vignette question. Column (4) reports the average of whether the taxpayer agrees with statements that "the digital system makes the collection process more efficient", "the digital portal will increase integrity and accountability of collection" and "[the respondent] prefer[s] the digital system because it allows for payments without the tax official." Column (5) reports whether the taxpayer thinks it is likely that a tax official in a neighborhood similar to theirs will offer to take an unofficial payment in order to not make return visits to the property. Column (6) reports the taxpayer-reported percentage of residential property owners in the assembly who pay the property taxes. Standard errors are clustered at the geographic collection unit level. Appendix Table A.20 shows the corresponding saturated specification. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Figures

Figure 1: Effect of Additional (Re-Centered) Exposure on Conversations about Property Taxes

(a) With Friends or Relatives

(b) With Neighbors



Notes: Shows the estimated propensity of property tax related conversations with friends or relatives (Panel A) and neighbors (Panel B) as a function of re-centered exposure to treated households (TG treated). Recentered exposure is computed following the randomization protocol and holding collection unit treatment statuses constant (see section 6.2 for details). The dark blue bars show the expected rate of conversations when the realized exposure equals the expected exposure. The light blue shows the treatment effect of an additional re-centered exposure. Vertical lines show the 95% confidence interval. Table A.9 shows results from the corresponding point estimates and a back-of-the-envelope calculation of the expected treatment effect on talking consistent with Table 2.

A Appendix Tables

Table A.1: Treatment Effect Heterogeneity on Knowledge and Tax Outcomes

		Taxpayer Knowledge			Tax Outcomes		
	(1) Workshop Participation	(2) Knowledge Index	(3) Taxpayer Actions Knowledge (Index)	(4) Self-perceived Knowledge	(5) Has Account Tax Portal	(6) Made Any Payment	(7) Payment Amount
Panel A: Income/wealth characteristics							
TG untreated	−0.00	0.02	0.32***	0.11*	−0.01	−0.02	−14.36
	(0.01)	(0.06)	(0.12)	(0.06)	(0.03)	(0.04)	(24.42)
TG untreated × Z	0.00	0.05	0.00	0.04	0.00	0.04	43.02**
	(0.00)	(0.04)	(0.11)	(0.05)	(0.03)	(0.03)	(20.93)
TG treated	0.22***	0.16**	0.54***	0.18***	−0.02	0.01	−8.89
	(0.03)	(0.07)	(0.13)	(0.05)	(0.03)	(0.04)	(22.59)
TG treated × Z	0.02	0.10**	0.05	0.03	−0.01	0.00	12.68
	(0.02)	(0.05)	(0.12)	(0.05)	(0.03)	(0.02)	(14.33)
CG mean	0.00	-0.08	-0.34	0.24	0.08	0.12	72.66
Observations	1108	1108	1108	1108	1108	1108	1108
Panel B: Property tax history							
TG untreated	−0.00	0.01	0.32***	0.11*	−0.01	−0.01	−11.47
	(0.01)	(0.06)	(0.12)	(0.06)	(0.03)	(0.04)	(23.95)
TG untreated × Z	0.00	−0.07	−0.01	0.04	0.03	−0.00	3.20
	(0.01)	(0.05)	(0.09)	(0.05)	(0.03)	(0.05)	(29.54)
TG treated	0.22***	0.16**	0.53***	0.17***	−0.02	0.00	−10.67
	(0.03)	(0.06)	(0.13)	(0.05)	(0.03)	(0.03)	(22.04)
TG treated × Z	0.03	−0.04	0.05	0.07	0.01	−0.02	−13.47
	(0.02)	(0.06)	(0.09)	(0.04)	(0.03)	(0.05)	(30.00)
CG mean	0.00	-0.08	-0.34	0.24	0.08	0.12	72.66
Observations	1104	1104	1104	1104	1104	1104	1104
Panel C: Tax planner measures							
TG untreated	−0.00	0.01	0.32***	0.11*	−0.01	−0.02	−17.04
	(0.01)	(0.06)	(0.12)	(0.06)	(0.03)	(0.04)	(24.29)
TG untreated × Z	0.00	0.04	−0.01	0.01	−0.03	−0.02	−10.99
	(0.00)	(0.03)	(0.06)	(0.04)	(0.02)	(0.03)	(19.98)
TG treated	0.22***	0.16**	0.54***	0.18***	−0.01	0.01	−7.99
	(0.03)	(0.07)	(0.12)	(0.05)	(0.02)	(0.04)	(22.18)
TG treated × Z	0.00	0.06	−0.15**	−0.00	−0.02	−0.01	6.59
	(0.02)	(0.04)	(0.07)	(0.04)	(0.02)	(0.03)	(17.54)
CG mean	0.00	-0.08	-0.34	0.24	0.08	0.12	72.66
Observations	1108	1108	1108	1108	1108	1108	1108
Panel D: Local government satisfaction							
TG untreated	−0.00	0.01	0.32***	0.11*	−0.02	−0.02	−16.19
	(0.01)	(0.06)	(0.12)	(0.06)	(0.03)	(0.04)	(23.58)
TG untreated × Z	0.00	0.03	0.09	0.05	0.02	0.00	−8.03
	(0.00)	(0.04)	(0.08)	(0.04)	(0.02)	(0.03)	(25.88)
TG treated	0.22***	0.16**	0.54***	0.18***	−0.02	0.01	−7.11
	(0.03)	(0.07)	(0.13)	(0.05)	(0.03)	(0.03)	(21.69)
TG treated × Z	0.01	0.02	0.10	0.01	0.02	−0.00	−15.25
	(0.02)	(0.04)	(0.08)	(0.04)	(0.02)	(0.03)	(23.80)
CG mean	0.00	-0.08	-0.34	0.24	0.08	0.12	72.66
Observations	1108	1108	1108	1108	1108	1108	1108

Notes: Shows treatment effect heterogeneity estimates augmenting equation (1) by interacting TG-untreated and TG-treated with a baseline index Z (standardized). Panel A uses the income/wealth characteristics index, Panel B the property tax history index, Panel C the tax planner measures index, and Panel D the local government satisfaction index. See Table A.3 for definitions of each index. Columns (5)–(7) show heterogeneous effects on tax outcomes. Standard errors are clustered at the geographic collection unit level. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table A.2: Balance of Randomization (Tax Officials)

	Pooled Specification		Saturated Specification				
	(1) TG3 = 0	(2) TG3 = 1	(3) CG	(4) TG1	(5) TG2	(6) TG3	(7) N
Experience working for local government	1.38	0.13	1.20	0.37	0.05	0.30	24
	(0.45)	(0.90)	(0.72)	(0.98)	(1.39)	(1.09)	
Experience working as a tax official	2.25	0.50	2.20	0.23	−0.20	0.55	24
	(0.47)	(0.83)	(0.95)	(1.18)	(1.34)	(1.19)	
Tertiary education	0.75	−0.13	0.60	0.26	0.15	0.02	24
	(0.11)	(0.21)	(0.24)	(0.28)	(0.34)	(0.30)	
Speaks local language	0.56	−0.31	0.80	−0.23	−0.55*	−0.55**	24
	(0.13)	(0.21)	(0.20)	(0.28)	(0.31)	(0.26)	
Connections to other collectors (up to 10)	5.88	−1.38	6.40	−1.69	0.85	−1.90	24
	(1.01)	(1.85)	(1.47)	(2.26)	(2.51)	(2.20)	
Confident self-reflection	2.25	1.13	2.40	−0.26	−0.15	0.98	24
	(0.51)	(0.70)	(1.01)	(1.35)	(1.23)	(1.13)	
Familiarity municipal public projects (up to 5)	0.56	−0.31	1.00	−0.57	−0.75	−0.75	24
	(0.28)	(0.32)	(0.76)	(0.82)	(0.80)	(0.78)	
Knowledge score local taxes (Z-score)	0.10	−0.29	0.27	−0.35	−0.08	−0.46	24
	(0.24)	(0.47)	(0.30)	(0.56)	(0.48)	(0.52)	
Knowledge score property tax determinants (Z-score)	0.17	−0.51	0.02	0.51	−0.27	−0.36	24
	(0.27)	(0.40)	(0.62)	(0.70)	(0.77)	(0.69)	
Knowledge score responsible ratable amount (Z-score)	0.03	−0.16	0.32	−0.09	−1.01**	−0.46	24
	(0.25)	(0.43)	(0.46)	(0.62)	(0.47)	(0.59)	
Knowledge score property tax rate (Z-score)	0.41	−0.56	0.16	0.59	−0.04	−0.31	24
	(0.20)	(0.43)	(0.39)	(0.48)	(0.53)	(0.56)	
Combined knowledge score (Z-score)	0.18	−0.38	0.19	0.16	−0.35	−0.40	24
	(0.18)	(0.32)	(0.32)	(0.46)	(0.41)	(0.42)	
Believed share pay property tax (out of 100)	44.56	10.44	48.33	−9.33	1.67	6.67	16
	(6.05)	(8.18)	(10.30)	(15.62)	(10.30)	(11.90)	
Extent agree with tax planning attitudes (0-3)	1.00	0.00	1.00	−0.14	0.25	0.00	24
	(0.18)	(0.37)	(0.31)	(0.41)	(0.55)	(0.46)	
Confident outlook to next collection week	2.31	−1.06*	1.80	1.06	0.20	−0.55	24
	(0.48)	(0.60)	(0.90)	(1.10)	(1.42)	(0.97)	

Notes: Shows tests of the balance of randomization for tax officials. Each row shows results from a separate regression with outcome variables at baseline described in each row. Columns (1) and (2) are estimated using equation (3); columns (3) to (6) are estimated using equation (4). p-values * $\leq$ 0.1, ** $\leq$ 0.05, *** $\leq$ 0.01

Table A.3: Balance of Randomization (Taxpayers)

	(1) CG mean	(2) TG untreated	(3) TG treated	(4) p-value (2) = (3)	(5) N
Panel A: Individual Characteristics					
Income/wealth characteristics [Index]	-0.02	−0.05	0.05	0.13	1108
		(0.15)	(0.15)		
Female	0.45	0.03	0.02	0.84	1108
		(0.05)	(0.06)		
Age 𝕀(61 years or older)	0.48	−0.07	−0.03	0.21	1108
		(0.04)	(0.04)		
Years lived in community	35.89	−3.52	−2.73	0.34	1108
		(2.22)	(2.19)		
Education (in years)	10.73	0.56	0.62	0.80	1108
		(0.60)	(0.59)		
Wage job	0.12	0.02	−0.00	0.32	1108
		(0.02)	(0.02)		
Number of days in month short of cash	8.96	1.11	−0.17	0.05	1108
		(1.50)	(1.48)	*	
Difficulties finding GHS 300 within 4 days	0.64	0.02	−0.04	0.05	1108
		(0.09)	(0.09)	*	
Total household earning in past month	1,902.50	445.60	513.12	0.67	1108
		(299.79)	(317.20)		
Asset holdings (Z-score)	0.29	−0.36	−0.46	0.58	1108
		(0.46)	(0.47)		
Panel B: Property Characteristics					
Respondent is family owner	0.53	0.00	0.03	0.41	1108
		(0.10)	(0.10)		
Respondent is not owner but is in charge	0.29	−0.02	−0.07	0.03	1108
		(0.10)	(0.10)	**	
Residential property	0.68	0.05	0.08	0.33	1108
		(0.05)	(0.06)		
Panel C: Property Tax History					
Property tax history [Index]	0.01	−0.10	0.06	0.03	1104
		(0.16)	(0.16)	**	
Received current year property tax bill	0.19	0.05	0.08	0.35	1108
		(0.06)	(0.06)		

Full property tax payment	0.13	−0.05	0.01	0.00	1108
		(0.04)	(0.04)	***	
Partial property tax payment	0.04	0.00	0.00	0.89	1108
		(0.03)	(0.03)		
Received bill in recent years	0.75	−0.08	−0.05	0.39	1104
		(0.06)	(0.05)		
Panel D: Property Tax Knowledge					
Knowledge Z-score	0.02	−0.04	−0.04	0.89	1108
		(0.17)	(0.17)		
Knows local taxes correctly	0.63	−0.03	−0.04	0.55	1108
		(0.06)	(0.05)		
Factors determining property tax (score)	0.22	−0.08	−0.08	0.95	1108
		(0.09)	(0.09)		
Correct factors determining property tax	0.56	−0.14**	−0.15**	0.84	1108
		(0.06)	(0.06)		
Knows who determines property value	0.14	−0.01	0.01	0.22	1107
		(0.04)	(0.04)		
Self-perceived knowledge at least 'somewhat'	0.36	−0.08	−0.05	0.20	1108
		(0.09)	(0.09)		
Panel E: Property Tax Attitudes					
Tax planner measures [Index]	0.01	0.02	−0.01	0.61	1108
		(0.11)	(0.12)		
Collector likely to accept unofficial payment	0.42	0.07	0.01	0.01	1108
		(0.06)	(0.06)	**	
Reduce tax money belongs to people earned it	0.82	−0.02	−0.02	0.89	1108
		(0.04)	(0.04)		
Legal measures tax code justified to reduce rate	0.85	−0.02	−0.01	0.70	1108
		(0.05)	(0.05)		
Believed share residents comply property tax	50.11	−8.23	−8.78	0.71	1108
		(5.43)	(5.46)		
Local government collecting tax efficiently	0.70	−0.18***	−0.15**	0.21	1108
		(0.06)	(0.06)		
Cannot decline to pay b/c civic duty	0.92	−0.04	−0.02	0.15	1108
		(0.04)	(0.04)		
Panel F: Local Government Satisfaction					
Local government satisfaction [Index]	0.16	−0.24	−0.23	0.85	1108
		(0.16)	(0.16)		

Satisfied w/ personal dealings w/ local officials	0.40	−0.01	−0.03	0.41	1108
		(0.08)	(0.08)		
Satisfied w/ level public goods and services	0.25	−0.12	−0.10	0.50	1108
		(0.07)	(0.07)		
Knowledge local development projects (Z-score)	0.28	−0.32*	−0.36*	0.56	1108
		(0.19)	(0.18)		
Approached assembly past tax or public goods	0.19	0.00	−0.01	0.71	1108
		(0.04)	(0.04)		
Attended town hall meeting by assembly in past	0.07	−0.03	−0.02	0.52	1108
		(0.02)	(0.03)		

Notes: Shows tests of the balance of randomization for taxpayers estimated using equation (1) and including strata FE. Each row refers to a separate regression with the outcome variable at baseline described in each row. Column (1) reports the control group mean. Columns (2) and (3) show the regression coefficients for TG-treated and TG-untreated. Column (4) shows p-values from testing $\beta_{\text{TG untreated}} = \beta_{\text{TG treated}}$. The *Income/Wealth Characteristics Index* combines information on the number of days short of cash for basic expenditures in a typical month, the difficulty of gathering 300 GHS in the next four days in a typical month, household earnings last month, and an asset index. The asset index combines ownership of motorbikes, cars or trucks, TV, electric generator, sewing machine, and radio (all of which are first standardized before being summed). The *Property Tax History Index* combines information on whether a property tax bill was received in recent years (2021-2023), whether a bill for the year of the intervention (2024) was already received prior to the start of the intervention, and whether a partial or full payment, respectively, for 2024 had already been made. The *Tax Planner Measures Index* combines views on whether taxes should be reduced because it belongs to people who earned it, that the use of legal measures in the tax code is justified to reduce rate, and disagreeing with the statement that one cannot decline to pay because it is one's civic duty to pay. The *Local Government Satisfaction Index* combines information on the respondent's satisfaction with personal dealings with local officials and the level of public goods and services provision, respectively, and whether they have attended a town hall meeting by the assembly in the past. The knowledge Z-score combines knowledge on the institution responsible for determining the property valuation, factors influencing the amount of property tax liability, and local vs. national taxes. Standard errors are clustered at the geographic collection unit level. Appendix Table A.4 shows the corresponding saturated specification. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table A.4: Balance of Randomization (Taxpayers × Saturated Specification)

	(1) CG mean	(2) TG1 untreated	(3) TG2 untreated	(4) TG3 untreated	(5) TG1 treated	(6) TG2 treated	(7) TG3 treated	(8) N
Panel A: Individual Characteristics								
Income/wealth characteristics [Index]	-0.02	0.05	−0.24	−0.04	0.08	0.07	0.02	1108
		(0.18)	(0.30)	(0.15)	(0.19)	(0.25)	(0.16)	
Female	0.45	−0.02	0.06	0.06	0.02	0.06	0.01	1108
		(0.06)	(0.08)	(0.06)	(0.06)	(0.09)	(0.06)	
Age 𝕀(61 years or older)	0.48	−0.03	−0.06	−0.11*	−0.03	−0.01	−0.04	1108
		(0.05)	(0.07)	(0.06)	(0.05)	(0.06)	(0.05)	
Years lived in community	35.89	−5.44**	−3.05	−2.10	−3.78	−4.18	−1.10	1108
		(2.51)	(2.32)	(2.72)	(2.57)	(2.56)	(2.54)	
Education (in years)	10.73	1.11*	0.73	−0.02	1.26**	1.11	−0.18	1108
		(0.66)	(0.92)	(0.67)	(0.58)	(0.69)	(0.73)	
Wage job	0.12	0.05	0.06*	−0.02	−0.00	0.02	−0.01	1108
		(0.03)	(0.03)	(0.02)	(0.04)	(0.07)	(0.03)	
Number of days in month short of cash	8.96	0.86	2.92	0.40	0.52	−0.15	−0.77	1108
		(1.92)	(2.03)	(1.79)	(2.01)	(1.53)	(1.74)	
Difficulties finding GHS 300 within 4 days	0.64	−0.04	0.12	0.02	−0.07	−0.00	−0.03	1108
		(0.10)	(0.12)	(0.10)	(0.10)	(0.12)	(0.10)	
Total household earning in past month	1,902.50	715.49*	329.20	272.55	505.95	807.25	372.51	1108
		(410.16)	(614.88)	(337.04)	(467.88)	(652.94)	(350.90)	
Asset holdings (Z-score)	0.29	−0.36	−0.41	−0.33	−0.26	−0.45	−0.64	1108
		(0.55)	(0.59)	(0.54)	(0.57)	(0.57)	(0.56)	
Panel B: Property Characteristics								
Respondent is family owner	0.53	0.03	−0.03	0.00	0.02	−0.04	0.08	1108
		(0.11)	(0.13)	(0.11)	(0.11)	(0.15)	(0.11)	
Respondent is not owner but is in charge	0.29	−0.00	−0.03	−0.03	−0.07	−0.05	−0.08	1108
		(0.11)	(0.12)	(0.11)	(0.11)	(0.13)	(0.11)	
Residential property	0.68	0.08	0.05	0.03	0.12**	−0.01	0.09	1108
		(0.06)	(0.08)	(0.08)	(0.05)	(0.08)	(0.08)	
Panel C: Property Tax History								
Property tax history [Index]	0.01	−0.14	−0.29*	0.04	0.04	−0.04	0.12	1104
		(0.19)	(0.17)	(0.19)	(0.18)	(0.19)	(0.20)	
Received current year property tax bill	0.19	0.07	0.01	0.06	0.05	0.07	0.11	1108
		(0.08)	(0.08)	(0.08)	(0.08)	(0.07)	(0.08)	

Full property tax payment	0.13	−0.05	−0.12***	−0.02	0.00	−0.01	0.03	1108
		(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	
Partial property tax payment	0.04	−0.01	−0.01	0.02	0.00	−0.02	0.01	1108
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Received bill in recent years	0.75	−0.12	−0.09	−0.04	−0.02	−0.05	−0.09	1104
		(0.08)	(0.09)	(0.07)	(0.06)	(0.09)	(0.06)	
Panel D: Property Tax Knowledge								
Knowledge Z-score	0.02	−0.02	0.01	−0.09	−0.02	−0.03	−0.05	1108
		(0.18)	(0.21)	(0.18)	(0.18)	(0.19)	(0.18)	
Knows local taxes correctly	0.63	−0.02	−0.10	−0.00	−0.04	−0.06	−0.04	1108
		(0.06)	(0.10)	(0.05)	(0.07)	(0.08)	(0.05)	
Factors determining property tax (score)	0.22	−0.07	−0.05	−0.11	−0.13	−0.03	−0.07	1108
		(0.09)	(0.13)	(0.09)	(0.09)	(0.11)	(0.11)	
Correct factors determining property tax	0.56	−0.18**	−0.09	−0.14*	−0.22***	−0.09	−0.11	1108
		(0.07)	(0.10)	(0.07)	(0.07)	(0.10)	(0.07)	
Knows who determines property value	0.14	0.03	0.00	−0.06	0.08	−0.04	−0.04	1107
		(0.05)	(0.06)	(0.04)	(0.05)	(0.05)	(0.05)	
Self-perceived knowledge at least 'somewhat'	0.36	−0.11	0.02	−0.11	−0.09	−0.01	−0.03	1108
		(0.10)	(0.13)	(0.10)	(0.10)	(0.12)	(0.09)	
Panel E: Property Tax Attitudes								
Tax planner measures [Index]	0.01	−0.04	0.13	0.01	0.06	0.02	−0.09	1108
		(0.16)	(0.16)	(0.14)	(0.15)	(0.17)	(0.15)	
Collector likely to accept unofficial payment	0.42	0.09	−0.01	0.09	0.05	−0.03	0.00	1108
		(0.08)	(0.09)	(0.08)	(0.09)	(0.09)	(0.08)	
Reduce tax money belongs to people earned it	0.82	−0.08	0.03	0.01	−0.03	−0.01	−0.01	1108
		(0.07)	(0.06)	(0.04)	(0.05)	(0.06)	(0.05)	
Legal measures tax code justified to reduce rate	0.85	−0.03	−0.04	−0.00	−0.01	−0.01	−0.02	1108
		(0.06)	(0.06)	(0.07)	(0.05)	(0.08)	(0.07)	
Believed share residents comply property tax	50.11	−10.88	−12.01*	−4.01	−14.77**	−9.89*	−3.02	1108
		(6.65)	(6.28)	(5.91)	(6.52)	(5.87)	(5.95)	
Local government collecting tax efficiently	0.70	−0.18**	−0.28***	−0.13	−0.13*	−0.24***	−0.12	1108
		(0.08)	(0.07)	(0.09)	(0.08)	(0.07)	(0.08)	
Cannot decline to pay b/c civic duty	0.92	−0.07	−0.08	−0.00	−0.07	−0.03	0.03	1108
		(0.06)	(0.08)	(0.04)	(0.06)	(0.07)	(0.03)	
Panel F: Local Government Satisfaction								
Local government satisfaction [Index]	0.16	−0.24	−0.38*	−0.16	−0.25	−0.31	−0.17	1108

		(0.19)	(0.21)	(0.18)	(0.19)	(0.20)	(0.17)	
Satisfied w/ personal dealings w/ local officials	0.40	−0.07	−0.05	0.07	−0.10	−0.03	0.03	1108
		(0.09)	(0.11)	(0.10)	(0.09)	(0.11)	(0.09)	
Satisfied w/ level public goods and services	0.25	−0.07	−0.17*	−0.13*	−0.08	−0.17**	−0.09	1108
		(0.09)	(0.09)	(0.08)	(0.09)	(0.08)	(0.08)	
Knowledge local development projects (Z-score)	0.28	−0.13	−0.44**	−0.43**	−0.28	−0.52**	−0.35*	1108
		(0.22)	(0.21)	(0.20)	(0.19)	(0.20)	(0.19)	
Approached assembly past tax or public goods	0.19	−0.01	0.04	−0.01	0.01	0.00	−0.04	1108
		(0.05)	(0.07)	(0.05)	(0.06)	(0.07)	(0.05)	
Attended town hall meeting by assembly in past	0.07	−0.03	−0.04	−0.02	−0.01	−0.01	−0.03	1108
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	

Notes: Shows tests of the balance of randomization for taxpayers estimated using equation (2) and including strata FE. Each row refers to a separate regression with the outcome variable at baseline described in each row. Column (1) shows the control group (CG) mean. Columns (2)–(4) show the estimated difference between TG-untreated households in TG1, TG2, and TG3 and the control group, respectively. Columns (5)–(7) show the estimated difference between TG-treated households in TG1, TG2, and TG3 and the control group, respectively. See Table A.3 for index definitions and the corresponding pooled specification. Standard errors are clustered at the geographic collection unit level. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table A.5: Summary Statistics of Property Owners

	(1) Mean	(2) Median	(3) Min	(4) Max	(5) SD	(6) N
Panel A: Socio-demographic characteristics						
Female	0.46	0	0	1	0.50	1,193
Age $\in [41, 60]$	0.42	0	0	1	0.49	1,193
Age ≥ 61	0.44	0	0	1	0.50	1,193
Years lived in community	33	31	1	100	19	1,193
Secondary education highest degree	0.36	0	0	1	0.48	1,193
Tertiary education highest degree	0.24	0	0	1	0.43	1,193
Panel B: Economic characteristics						
Self-employed or in family business	0.49	0	0	1	0.50	1,193
Retired	0.28	0	0	1	0.45	1,193
Number of days short of cash	9.3	7.0	0	30	8.5	1,193
Difficult finding 300 GHS in 4 days	0.63	1	0	1	0.48	1,193
Earnings past month (winsorized 99th pctile)	2,294	1,500	0	15,000	2,612	1,193
Panel C: Property characteristics						
Owns property together with family	0.54	1	0	1	0.50	1,193
Not property owner but in charge	0.25	0	0	1	0.43	1,193
Property usage — residential (share)	0.73	1	0	1	0.44	1,193
Panel D: Property tax						
Received bill for FY at baseline	0.25	0	0	1	0.43	1,193
Made full property tax payment for FY	0.12	0	0	1	0.32	1,193
Made partial payment for FY	0.04	0	0	1	0.20	1,193

Notes: Shows summary statistics for the property owners at baseline.

Table A.6: Summary Statistics of Tax Officials

	(1) Mean	(2) Median	(3) Min	(4) Max	(5) SD	(6) N
Completed tertiary education	0.71	1	0	1	0.46	24
Work experience in local government in years	1.4	0.50	0	5.0	1.9	24
Work experience as tax official in years	2.4	1.5	0	5.0	1.9	24
Performance rating by local government	8.0	8.0	6.0	10	1.3	22

Notes: Shows summary statistics for the tax officials at baseline.

Table A.7: Treatment Effects on Taxpayer Knowledge (Saturated Specification)

		Declarative Knowledge Questions					
	(1) Index Over (2)-(5)	(2) Formula	(3) Process [Index]	(4) Responsibilities	(5) Rights	(6) Self-Perceived Knowledge	(7) Taxpayer Action [Index]
Panel A: Taxpayer Midline Survey							
TG1 untreated	−0.01	−0.05	0.03	−0.09	−0.02	0.19*	
	(0.09)	(0.15)	(0.14)	(0.15)	(0.12)	(0.11)	
TG2 untreated	−0.01	−0.15	0.09	−0.13	−0.14	0.05	
	(0.08)	(0.16)	(0.14)	(0.16)	(0.10)	(0.09)	
TG3 untreated	−0.17**	−0.27**	−0.29**	−0.11	−0.08	−0.02	
	(0.08)	(0.12)	(0.12)	(0.15)	(0.14)	(0.09)	
TG1 treated	0.04	0.12	0.04	0.08	−0.01	0.03	
	(0.09)	(0.15)	(0.12)	(0.19)	(0.15)	(0.10)	
TG2 treated	0.36**	0.24	0.41*	0.65**	0.60**	0.07	
	(0.14)	(0.25)	(0.20)	(0.26)	(0.25)	(0.08)	
TG3 treated	0.24**	0.28	0.43***	0.10	0.12	0.08	
	(0.09)	(0.18)	(0.10)	(0.25)	(0.23)	(0.10)	
CG mean	-0.05	-0.02	-0.07	-0.07	-0.07	0.26	
Observations	319	319	319	319	319	313	
Panel B: Taxpayer Endline Survey							
TG1 untreated	0.10	−0.03	0.21	0.06	0.20	0.12	0.34**
	(0.09)	(0.16)	(0.14)	(0.21)	(0.22)	(0.08)	(0.16)
TG2 untreated	0.09	−0.14	0.35**	0.00	0.05	0.01	0.34*
	(0.08)	(0.18)	(0.17)	(0.22)	(0.10)	(0.08)	(0.19)
TG3 untreated	−0.10*	−0.24	0.00	−0.30***	−0.08	0.14*	0.30*
	(0.06)	(0.15)	(0.11)	(0.10)	(0.05)	(0.08)	(0.16)
TG1 treated	0.16*	0.02	0.27*	0.22	0.24	0.17**	0.64***
	(0.09)	(0.18)	(0.16)	(0.19)	(0.15)	(0.08)	(0.19)
TG2 treated	0.34***	−0.08	0.84***	0.19	0.41**	0.13*	0.51**
	(0.10)	(0.17)	(0.28)	(0.21)	(0.17)	(0.08)	(0.19)
TG3 treated	0.07	0.11	0.28**	−0.19	0.01	0.21***	0.48***
	(0.08)	(0.21)	(0.13)	(0.11)	(0.07)	(0.07)	(0.18)
CG mean	-0.08	0.05	-0.22	0.03	-0.11	0.24	-0.34
Observations	1108	1108	1108	1108	1108	1108	1108

Notes: Panels A and B show ITT effects for taxpayers estimated using equation (2) and including strata FE. See Table 1 for detailed descriptions and the corresponding pooled specification following equation (1). Standard errors are clustered at the geographic collection unit level. p-values * $\leq$ 0.1, ** $\leq$ 0.05, *** $\leq$ 0.01

Table A.8: Treatment Effects on Talking about Property Taxes (Saturated Specification)

	Conversation Partner			Topic of Conversation					
	(1) Speak to Anyone	(2) Speak to Relative/Friends	(3) Speak to Neighbors	(4) Split Payment	(5) Gather Funds	(6) Lower Liability	(7) Process	(8) Taxpayer Duties	(9) Taxpayer Rights
TG1 untreated	0.10**	0.05*	0.01	0.00	−0.00	0.03*	0.04*	0.05	0.06**
	(0.05)	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
TG2 untreated	0.03	0.01	0.01	−0.01	−0.01	0.01	0.02	−0.01	0.02
	(0.04)	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)	(0.02)
TG3 untreated	0.17**	0.12**	0.03	0.05**	0.09**	0.05**	0.08*	0.04*	0.06**
	(0.07)	(0.05)	(0.03)	(0.02)	(0.04)	(0.02)	(0.04)	(0.02)	(0.02)
TG1 treated	0.14***	0.08***	0.04***	0.02	0.02	0.01	0.05*	0.03*	0.03**
	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
TG2 treated	0.10**	0.06	0.02	−0.01	0.00	0.03	0.01	−0.01	0.03
	(0.05)	(0.04)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)
TG3 treated	0.22***	0.14***	0.05**	0.05**	0.10**	0.05***	0.12***	0.07**	0.09***
	(0.07)	(0.04)	(0.02)	(0.02)	(0.05)	(0.02)	(0.04)	(0.03)	(0.03)
CG mean	0.04	0.03	0.01	0.01	0.01	0.00	0.02	0.01	0.00
Observations	1108	1108	1108	1108	1108	1108	1108	1108	1108

Notes: Shows ITT effects estimated using equation (2) and including strata FE. See Table 2 for details and the corresponding pooled specification. Standard errors are clustered at the geographic collection unit level. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table A.9: Back-of-the-envelope calculation on predicted treatment effect on conversations

	Talking to neighbors				Talking to friends/relatives			
	100m	150m	200m	250m	100m	150m	200m	250m
β talk on TG untreated	0.021	0.021	0.021	0.021	0.069	0.069	0.069	0.069
β talk on recentered exposure	0.007	0.004	0.003	0.001	0.004	0.011	0.008	0.011
Average exposure TG untreated	6.319	12.222	18.835	25.965	6.319	12.222	18.835	25.965
Average exposure CG	2.096	6.297	12.511	20.424	2.096	6.297	12.511	20.424
Predicted Pr(talk | TG untreated)	0.042	0.042	0.063	0.035	0.025	0.127	0.139	0.249
Predicted Pr(talk | CG)	0.014	0.022	0.042	0.027	0.008	0.068	0.095	0.201
Predicted β talk on TG untreated	0.028	0.020	0.021	0.007	0.016	0.060	0.044	0.047

Notes: Shows a back-of-the-envelope calculation of the predicted treatment effect on talking to neighbors and friends/relatives based on re-centered exposure to treated property owners (TG-treated). The first row shows the treatment effect on talking from Table 2. The second row shows the effect of an additional re-centered exposure on talking (Figure 1). The third and fourth rows show the average realized exposure to TG treated households for TG untreated and CG households, respectively. The predicted probabilities of talking conditional on TG untreated and CG, respectively, are computed as $1-(1-\beta)^n$, where β is the re-centered estimate from the second row and n is the average exposure for TG untreated and CG, respectively.

Table A.10: Treatment Effects on Public Finance Outcomes (Saturated Specification)

	(1) Tax Official Visit	(2) Received Bill	(3) Account Tax Portal	(4) Made Any Payment	(5) Payment Amount
TG1 untreated	0.07	0.07	−0.02	−0.01	−22.14
	(0.07)	(0.10)	(0.03)	(0.04)	(26.27)
TG2 untreated	0.03	−0.10	−0.00	−0.05	−15.95
	(0.08)	(0.09)	(0.04)	(0.06)	(50.21)
TG3 untreated	−0.01	0.00	−0.01	−0.01	−13.95
	(0.06)	(0.08)	(0.03)	(0.04)	(24.98)
TG1 treated	0.03	0.03	−0.04	−0.01	−21.13
	(0.07)	(0.09)	(0.03)	(0.04)	(24.90)
TG2 treated	0.05	−0.10	0.02	−0.00	−22.08
	(0.09)	(0.09)	(0.04)	(0.05)	(27.58)
TG3 treated	0.04	0.00	−0.01	0.03	10.27
	(0.06)	(0.09)	(0.03)	(0.04)	(25.15)
CG mean	0.13	0.31	0.08	0.12	72.66
Observations	1108	1108	1108	1108	1108

Notes: Shows ITT effects estimated using equation (2) and including strata FE. See Table 3 for details and the corresponding pooled specification. Standard errors are clustered at the geographic collection unit level. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table A.11: Treatment Effects on Public Finance Outcomes (Language Control)

	(1) Tax Official Visit	(2) Received Bill	(3) Account Tax Portal	(4) Made Any Payment	(5) Payment Amount
TG untreated	0.04	0.01	−0.01	−0.03	−21.64
	(0.05)	(0.08)	(0.03)	(0.04)	(25.85)
TG treated	0.05	−0.01	−0.01	0.00	−12.23
	(0.05)	(0.08)	(0.03)	(0.04)	(24.89)
CG mean	0.13	0.31	0.08	0.12	72.66
Official language control	Yes	Yes	Yes	Yes	Yes
Observations	1108	1108	1108	1108	1108

Notes: Shows ITT effects estimated using equation (1), including strata FE, and controlling for whether the tax official speaks the most predominant local language (Ga). See Table 3 for details and the main specification. Standard errors are clustered at the geographic collection unit level. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table A.12: Targeting Taxpayers Based On Education Status (Saturated Specification)

	(1) CSO Visited	(2) Minutes (20m)
Panel A: Main Specification		
TG1 untreated	0.066	−0.713
	(0.072)	(0.524)
TG2 untreated	0.031	−0.150
	(0.077)	(0.633)
TG3 untreated	−0.014	−0.338
	(0.060)	(0.606)
TG1 treated	0.026	−0.600
	(0.067)	(0.599)
TG2 treated	0.046	−0.191
	(0.091)	(0.793)
TG3 treated	0.036	−0.058
	(0.059)	(0.674)
CG mean	0.131	1.478
Observations	1108	1629
Panel B: Highlighted Specification		
TG 1	0.056	−0.656
	(0.068)	(0.536)
TG 23	0.010	−0.102
	(0.060)	(0.588)
TG 23 Highlighted	0.036	−0.355
	(0.034)	(0.355)
TG 1 Counterfactual	−0.038	−0.011
	(0.041)	(0.199)
Control Counterfactual	0.000	0.000
	(0.000)	(0.000)
CG mean	0.131	1.478
Observations	1108	1629

Notes: Panel A shows treatment effect estimates following equation (2). Panel B is estimated using

$$y_{ij} = \alpha + \sum_{k \in 1,2/3} \beta_k \mathbb{1}(\text{TG}_{ij=k}) + \gamma_0 \mathbb{1}(\text{TG counterfactual}_{ij=0}) + \gamma_1 \mathbb{1}(\text{TG counterfactual}_{ij=1}) + \gamma_{23} \mathbb{1}(\text{TG highlighted}_{ij=2/3}) + \phi \mathbb{1}(V_j \geq V_{(50)}) + \varepsilon_{ij}$$

where $\mathbb{1}(\text{TG}_{ij=k})$ indicates whether a taxpayer i lives in collection unit j of type k (of type 1, or of type 2 or 3), $\mathbb{1}(\text{TG counterfactual}_{ij=0})$ indicates whether taxpayer i lives in an area of treated collection unit j of type 0 (control) that would have been highlighted had the unit been randomly assigned to TG2 or TG3, $\mathbb{1}(\text{TG counterfactual}_{ij=1})$ indicates whether taxpayer i lives in an area of treated collection unit j of type 1 (control) that would have been highlighted had the unit been randomly assigned to TG2 or TG3, $\mathbb{1}(\text{TG highlighted}_{ij=2/3})$ takes values 1 if a taxpayer i lives in the highlighted area of treated collection unit j which was randomly assigned to TG2 or TG3. β_ks are the ITT estimate for the average effect of living in the non-highlighted areas of a collection unit of type k, γ_0 and γ_1 are respectively the ITT estimates for the average effect of living in a "counterfactual" highlighted area of a collection unit j of type 0 and 1, and γ_{23} is the ITT estimate for the average effect of living in a highlighted area of a collection unit j of type 2 or 3.

Table 5 shows the results from the corresponding pooled specification. Column (1) captures whether the taxpayer reports at the endline a visit from a tax official during the collection campaign and column (2) captures time (in minutes) that the tax official spent in a week within 20 meters of the taxpayer's property as captured by their tablet GPS. Column (2) also includes week fixed effects. Standard errors are clustered at the geographic collection unit level. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table A.13: Details of Bill Received

	(1) Received Bill	(2) Accessed Bill Online	(3) Bill Received Tax Official	(4) Bill Received Assembly	(5) Bill Amount
TG untreated	0.007	0.003	0.022	0.000	19.078
	(0.076)	(0.006)	(0.070)	(0.013)	(49.295)
TG treated	−0.008	0.008	−0.000	−0.003	25.289
	(0.075)	(0.006)	(0.069)	(0.013)	(49.250)
CG mean	0.310	0.014	0.859	0.070	579.577
Observations	1108	1108	1108	1108	1108

Notes: Shows ITT effects estimated using equation (1) and including strata FE. Column (1) reports whether the taxpayer received a tax bill. Column (2) captures whether the taxpayer received their tax bill online. Column (3) captures whether the taxpayer received their tax bill from a tax official. Column (4) reports whether the taxpayer received their tax bill from the local government. Column (5) reports the tax bill amount. Columns (2)–(5) take value 0 if the taxpayer reports not receiving a tax bill in column (1). Standard errors are clustered at the geographic collection unit level. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table A.14: Targeting Taxpayers Based on Taxpayer Characteristics

	(1) Income/wealth index	(2) Property tax history index	(3) Tax planner index	(4) Local government satisfaction index
Panel A: Visited by tax official				
Visited	0.22	−0.03	−0.14	0.29
	(0.23)	(0.15)	(0.23)	(0.28)
Visited × TG untreated	−0.22	−0.14	0.21	−0.10
	(0.26)	(0.17)	(0.26)	(0.34)
Visited × TG treated	−0.16	−0.05	0.16	−0.17
	(0.27)	(0.20)	(0.25)	(0.31)
TG untreated	Yes	Yes	Yes	Yes
TG treated	Yes	Yes	Yes	Yes
Strata	Yes	Yes	Yes	Yes
CG mean	-0.02	0.01	0.01	0.16
Observations	1108	1104	1108	1108
Panel B: Taxpayer received bill				
Bill received	−0.00	0.55**	−0.12	−0.07
	(0.17)	(0.24)	(0.10)	(0.22)
Bill received × TG untreated	0.09	0.15	−0.00	−0.04
	(0.19)	(0.27)	(0.16)	(0.24)
Bill received × TG treated	−0.10	−0.04	−0.05	−0.02
	(0.21)	(0.27)	(0.16)	(0.25)
TG untreated	Yes	Yes	Yes	Yes
TG treated	Yes	Yes	Yes	Yes
Strata	Yes	Yes	Yes	Yes
CG mean	-0.02	0.01	0.01	0.16
Observations	1108	1104	1108	1108

Notes: Shows results on whether tax officials systematically targeted taxpayers on observables. The targeting outcome in Panel A is whether the household was visited by a tax official. The targeting outcome in Panel B is whether the household received a bill. Control mean shows the unconditional sample mean of the index (expressed in the column title) for the control group. Information on whether taxpayer was visited by tax officials and whether a bill was obtained in *prior* years was asked during endline. p-values * ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01.

Table A.15: Tax Officials' Targeting Preferences

Panel A: Often focusing on or avoiding areas and properties								
	(1) Commercial centers	(2) Paid tax past year	(3) Property appears high value	(4) Informed tax duties	(5) Target Knowledgeable	(6) Avoid Know Rights	(7) Satisfied Public Services	(8) Area w/ Resources
TG 1	−0.07	−0.15	−0.07	−0.19	−0.11	0.01	0.02	0.04
	(0.15)	(0.21)	(0.19)	(0.22)	(0.23)	(0.07)	(0.25)	(0.23)
TG 2	0.04	−0.19	0.06	−0.07	0.06	0.08	−0.01	0.13
	(0.25)	(0.27)	(0.16)	(0.26)	(0.25)	(0.10)	(0.22)	(0.22)
TG 3	−0.02	−0.33*	−0.13	−0.32	−0.19	0.07	−0.17	−0.17
	(0.15)	(0.19)	(0.17)	(0.21)	(0.20)	(0.09)	(0.22)	(0.17)
CG mean	0.28	0.56	0.36	0.60	0.52	0.08	0.48	0.40
Observations	117	117	117	117	117	117	117	117

Panel B: Targeting composition of property owners (out of 100)						
	(9) Well-versed law	(10) Not knows laws	(11) History not paying	(12) History paying	(13) Location middle-income	(14) Location high-income
TG 1	1.25	−2.25	−4.96	0.64	4.05*	1.26
	(4.45)	(2.40)	(3.67)	(1.48)	(2.28)	(1.67)
TG 2	−0.44	0.29	−5.93	3.69**	1.92	0.46
	(4.49)	(1.97)	(3.68)	(1.78)	(3.04)	(1.69)
TG 3	−1.16	−0.11	−4.25	0.22	2.39	2.92
	(4.31)	(2.11)	(3.82)	(1.84)	(2.19)	(2.35)
CG mean	23.60	14.60	20.40	15.68	14.92	10.80
Observations	117	117	117	117	117	117

Panel C: Targeting preferences when facing trade-offs in property owner characteristics			
	(15) Rights over responsibilities	(16) Small but certain resources	(17) Attentive law than not aware
TG 1	−0.15	0.04	0.01
	(0.09)	(0.09)	(0.12)
TG 2	0.01	−0.04	0.27***
	(0.18)	(0.14)	(0.05)
TG 3	−0.03	−0.00	0.17**
	(0.09)	(0.12)	(0.08)
CG mean	0.36	0.72	0.52
Observations	117	117	117

Notes: Shows ITT effects on tax officials' targeting preferences estimated using equation (4). Panel A shows whether the official indicates that they 'often' or 'very often' use a particular strategy to decide which properties/areas to focus their efforts on within their collection unit. Panel B shows the targeting composition in the scenario that the tax official is presented with information about properties and the respective property owners (officials were asked to hypothetically allocate 100 visits across households with different characteristics). Panel C shows the propensity to choose between one of two property characteristics at the margin. Data at the tax official-week level. Standard errors clustered at the tax official level. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table A.16: Perceptions of Nature of Interactions between Taxpayers and Tax Officials (Language Control)

I. Taxpayer Perceptions

Panel A: Taxpayers' Perceptions of Dimensions of Interactions

	(1) Feeling About Oneself	(2) Tax Official Helpful	(3) Tax Official Provided Information	(4) Tax Official Good Characteristics	(5) Self Only Willing to Do Bare Minimum
TG untreated	8.18*	6.08	10.36	3.66	22.86
	(4.28)	(7.56)	(7.21)	(4.68)	(15.07)
TG treated	10.84**	10.01	7.34	2.47	19.93
	(5.34)	(8.21)	(6.56)	(4.41)	(15.26)
CG mean	55.17	16.44	11.42	62.19	25.83
Observations	166	166	166	166	166

Panel B: Taxpayers' Overall Assessment of Interactions

			Public Administration Literature Interaction Measures			
	(6) Interaction Helpful for Own Objectives	(7) Uncertainty	(8) Tax Official Obstructive	(9) Not Responsive But Next Steps Clear	(10) Motivated & Engaged Followed Up Info/Requests	(11) Cordial & Respectful Together Next Steps
TG untreated	6.31	0.03	0.04	0.17***	−0.09	−0.02
	(12.68)	(0.09)	(0.09)	(0.05)	(0.09)	(0.08)
TG treated	6.43	0.05	0.05	0.13**	−0.16*	0.08
	(13.55)	(0.08)	(0.08)	(0.05)	(0.09)	(0.08)
CG mean	30.33	0.27	0.10	0.00	0.53	0.13
Observations	166	166	166	166	166	166

II. Tax Officials Perceptions

Panel C: Tax Officials' Perceptions of Dimensions of Interactions

	(1) Feeling About Oneself	(2) Taxpayer Helpful	(3) Taxpayer Provided Information	(4) Taxpayer Good Characteristics	(5) Self Only Willing to Do Bare Minimum
TG1	1.48	11.98***	−3.48	1.37	2.88
	(8.00)	(3.30)	(3.91)	(3.86)	(2.89)
TG2	−2.55	5.69	−19.02*	−0.97	6.61
	(9.49)	(4.68)	(10.44)	(3.44)	(3.97)
TG3	−2.23	14.41***	11.81**	3.45	11.37***
	(6.27)	(2.85)	(5.65)	(2.92)	(3.49)
CG mean	67.34	51.58	44.83	60.17	52.83
Observations	141	141	141	117	141

Panel D: Tax Officials' Overall Assessment of Interactions

			Public Administration Literature Interaction Measures			
	(6) Interaction Helpful for Own Objectives	(7) Uncertainty	(8) Taxpayer Obstructive	(9) Not Responsive But Next Steps Clear	(10) Motivated & Engaged Followed Up Info/Requests	(11) Cordial & Respectful Together Next Steps
TG1	1.67	−0.06	−2.47	−3.38	0.12	0.65
	(5.94)	(0.08)	(3.43)	(2.33)	(3.48)	(4.13)
TG2	−0.17	−0.10	3.22	−0.05	−1.83	−0.04
	(6.33)	(0.06)	(4.82)	(2.33)	(3.72)	(4.76)
TG3	−0.79	−0.08	7.37	−1.80	0.02	−0.71
	(5.00)	(0.07)	(4.31)	(2.47)	(3.84)	(3.51)
CG mean	70.83	0.42	19.60	22.20	31.50	33.10
Observations	141	141	141	117	141	141

Notes: Panels A and B show ITT effects for taxpayers estimated using equation (1), including strata FE and controlling for whether the tax official speaks the local language; Panels C and D show ITT effects for tax officials estimated using equation (4), including time FE and controlling for whether the tax official speaks the local language. See Table 6 for details and the main specification. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

Table A.17: Taxpayer Perceptions of Nature of Interactions with Tax Official (Saturated Specification)

Panel A: Taxpayers' Perceptions of Dimensions of Interactions

	(1) Feeling About Oneself	(2) Tax Official Helpful	(3) Tax Official Provided Information	(4) Tax Official Good Characteristics	(5) Self Only Willing to Do Bare Minimum
TG1 untreated	15.93**	17.95	23.93**	4.82	39.03**
	(6.11)	(11.52)	(11.23)	(6.87)	(16.55)
TG2 untreated	14.48***	20.62**	17.61**	11.45	25.32
	(5.12)	(9.59)	(7.56)	(10.23)	(16.22)
TG3 untreated	7.72	10.28	9.35	−0.82	21.38
	(4.61)	(10.14)	(8.73)	(4.77)	(16.40)
TG1 treated	10.68	13.76	12.27	−0.18	31.33*
	(7.90)	(11.07)	(8.68)	(4.82)	(15.99)
TG2 treated	19.21***	30.36**	25.80**	7.16	33.73**
	(4.96)	(11.38)	(9.98)	(4.70)	(14.74)
TG3 treated	17.24***	18.19*	10.26	4.08	19.94
	(6.35)	(10.20)	(9.23)	(6.04)	(16.74)
CG mean	55.17	16.44	11.42	62.19	25.83
Observations	166	166	166	166	166

Panel B: Taxpayers' Overall Assessment of Interactions

			Public Administration Literature Interaction Measures			
	(6) Interaction Helpful for Own Objectives	(7) Uncertainty	(8) Tax Official Obstructive	(9) Not Responsive But Next Steps Clear	(10) Motivated & Engaged Followed Up Info/Requests	(11) Cordial & Respectful Together Next Steps
TG1 untreated	28.84*	0.04	−0.01	0.17*	0.01	−0.00
	(15.33)	(0.09)	(0.09)	(0.09)	(0.15)	(0.10)
TG2 untreated	18.97	−0.09	−0.11	−0.01	−0.04	0.18
	(15.53)	(0.12)	(0.08)	(0.02)	(0.30)	(0.18)
TG3 untreated	3.63	0.03	0.03	0.12	0.00	−0.08
	(14.61)	(0.10)	(0.12)	(0.07)	(0.09)	(0.08)
TG1 treated	12.34	0.08	0.01	0.15	−0.07	0.00
	(19.10)	(0.09)	(0.10)	(0.10)	(0.14)	(0.11)
TG2 treated	34.94***	0.04	0.02	0.05	−0.08	0.23
	(11.11)	(0.10)	(0.13)	(0.07)	(0.22)	(0.22)
TG3 treated	13.91	0.00	−0.04	0.04	−0.09	0.14
	(15.86)	(0.10)	(0.09)	(0.04)	(0.10)	(0.12)
CG mean	30.33	0.27	0.10	0.00	0.53	0.13
Observations	166	166	166	166	166	166

Notes: Shows ITT effects estimated using equation (2) and including strata FE. See Table 6 for details and the corresponding pooled specification in equation (1). p-values * $\leq$ 0.1, ** $\leq$ 0.05, *** $\leq$ 0.01.

Table A.18: Taxpayer Actions (Saturated Specification)

	Request Bill Reduction			Request Property Re-valuation			Request Payment in Installments		
	(1) Knows About	(2) Requested	(3) Likely in Future	(4) Knows About	(5) Requested	(6) Likely in Future	(7) Knows About	(8) Requested	(9) Likely in Future
TG1 untreated	0.18**	0.06**	−0.09	0.14**	0.02	−0.13*	0.09	−0.04	−0.01
	(0.07)	(0.03)	(0.06)	(0.06)	(0.02)	(0.08)	(0.09)	(0.03)	(0.08)
TG2 untreated	0.12	−0.01	−0.02	0.16**	−0.01	−0.03	0.13	0.01	0.05
	(0.09)	(0.03)	(0.05)	(0.08)	(0.01)	(0.09)	(0.08)	(0.06)	(0.10)
TG3 untreated	0.10	0.02	0.02	0.14**	0.00	0.03	0.12	−0.00	0.04
	(0.07)	(0.03)	(0.04)	(0.06)	(0.01)	(0.07)	(0.08)	(0.03)	(0.07)
TG1 treated	0.32***	0.01	−0.04	0.24***	0.01	−0.22***	0.21**	0.00	−0.02
	(0.07)	(0.02)	(0.05)	(0.08)	(0.02)	(0.08)	(0.09)	(0.04)	(0.07)
TG2 treated	0.26***	0.05*	0.02	0.23***	0.03	−0.03	0.12	−0.03	0.07
	(0.07)	(0.03)	(0.04)	(0.08)	(0.02)	(0.08)	(0.10)	(0.04)	(0.06)
TG3 treated	0.23***	0.01	−0.01	0.19**	−0.00	−0.02	0.16*	0.01	0.03
	(0.07)	(0.02)	(0.04)	(0.08)	(0.01)	(0.07)	(0.09)	(0.03)	(0.07)
CG mean	0.26	0.04	0.84	0.16	0.01	0.71	0.32	0.08	0.75
Observations	1108	1108	1108	1108	1108	1108	1108	1108	1108

Notes: Shows ITT effect estimated using equation (2) and including strata FE. See Table 7 for details and the corresponding pooled specification. Standard errors are clustered at the geographic collection unit level. p-values * $\leq$ 0.1, ** $\leq$ 0.05, *** $\leq$ 0.01

Table A.19: Treatment Effects on Elicited Reasons for NOT Taking Action

	(1) Obligation full payment	(2) Can make full payment	(3) Reasons not justified	(4) Assembly would ignore request	(5) Assembly would not grant request	(6) Partial payment commonly acceptable	(7) Not received bill
Panel A: Request Bill reduction							
TG untreated	0.011	0.020	0.008	−0.008	0.019**	0.003	0.023
	(0.012)	(0.016)	(0.011)	(0.032)	(0.009)	(0.007)	(0.032)
TG treated	0.056***	0.058***	0.037**	−0.018	0.010**	0.003	0.072**
	(0.020)	(0.021)	(0.014)	(0.032)	(0.005)	(0.007)	(0.033)
CG mean	0.023	0.027	0.014	0.059	0.000	0.005	0.041
Observations	1036	1036	1036	1036	1036	1036	1036
Panel B: Request Property Re-valuation							
TG untreated	0.005	0.025**	0.022*	0.003	0.002	0.004	0.036
	(0.016)	(0.011)	(0.012)	(0.021)	(0.002)	(0.003)	(0.022)
TG treated	0.021	0.032**	0.038***	−0.002	0.005	0.007*	0.028
	(0.019)	(0.015)	(0.011)	(0.020)	(0.003)	(0.004)	(0.022)
CG mean	0.022	0.009	0.004	0.031	0.000	0.000	0.022
Observations	1085	1085	1085	1085	1085	1085	1085
Panel C: Request Payment in Installments							
TG untreated	−0.004	0.021	0.009*	0.009	−0.001	−0.057	0.035
	(0.019)	(0.025)	(0.005)	(0.008)	(0.005)	(0.060)	(0.027)
TG treated	0.022	0.031	0.012**	0.018*	0.005	−0.046	0.061**
	(0.022)	(0.024)	(0.005)	(0.010)	(0.007)	(0.060)	(0.026)
CG mean	0.033	0.067	0.000	0.005	0.005	0.100	0.033
Observations	1023	1023	1023	1023	1023	1023	1023

Notes: Shows ITT effects estimated using equation (1) and including strata FE. Each column is an indicator for whether the taxpayer chose that reason for not taking the action as defined in each panel (multiple answers possible per respondent). Standard errors are clustered at the geographic collection unit level. p-values * $\leq$ 0.1, ** $\leq$ 0.05, *** $\leq$ 0.01

Table A.20: Tax Morale, Enforcement and Corruption Views (Saturated Specification)

	Tax morale				Views	
	(1) Satisfaction and Competence	(2) Attend Public Meetings	(3) Fund Local Government	(4) Favorable Views of Platform	(5) Corruption	(6) Enforcement
TG1 untreated	0.018	0.006	−0.004	−0.031	−0.079	−5.324*
	(0.055)	(0.053)	(0.060)	(0.034)	(0.096)	(3.123)
TG2 untreated	0.000	0.015	0.008	−0.038	0.023	−2.441
	(0.055)	(0.050)	(0.071)	(0.028)	(0.122)	(6.509)
TG3 untreated	−0.059	0.030	−0.038	0.033	0.005	−8.172**
	(0.054)	(0.053)	(0.061)	(0.023)	(0.093)	(3.242)
TG1 treated	−0.065	0.001	−0.037	−0.075**	−0.063	−4.218
	(0.045)	(0.051)	(0.060)	(0.037)	(0.099)	(3.772)
TG2 treated	0.002	0.022	0.005	0.003	0.054	1.073
	(0.056)	(0.059)	(0.061)	(0.026)	(0.110)	(4.955)
TG3 treated	−0.011	0.052	−0.057	0.037	−0.029	−7.224*
	(0.054)	(0.044)	(0.058)	(0.024)	(0.095)	(3.850)
CG mean	0.214	0.812	0.269	0.616	0.581	56.178
Observations	1108	1108	1108	1108	1108	822

Notes: Shows ITT effects estimated using equation (2) and including strata FE. See Table 8 for details and the corresponding pooled specification. Standard errors are clustered at the geographic collection unit level. p-values * ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01

Table A.21: Baseline knowledge and endline outcomes

Panel A: Nature of interaction [Index]

	(1) Taxpayer dimensions	(2) Taxpayer overall	(3) Official dimensions	(4) Official overall
Knowledge index (baseline)	0.062	0.314	-0.174^{***}	-0.559^{***}
	(0.205)	(0.206)	(0.063)	(0.101)

Panel B: Tax planning [Index]

	(5) Knows about actions	(6) Requested actions	(7) Likely to request
Knowledge index (baseline)	0.217^{***}	-0.083	0.039
	(0.074)	(0.077)	(0.087)

Panel C: Tax morale and views

	(8) Satisfied with public goods	(9) Assembly competent	(10) Attend public meetings	(11) Fund local government	(12) Favorable views of platform	(13) Corruption	(14) Enforcement
Knowledge index (baseline)	-0.071^{**}	-0.002	0.012	0.001	0.022	-0.088^{*}	-4.2^{*}
	(0.036)	(0.040)	(0.036)	(0.037)	(0.015)	(0.045)	(2.5)

Panel D: Public finance outcomes

	(15) Received bill	(16) Made any payment	(17) Payment amount
Knowledge index (baseline)	0.066	0.049	28.4
	(0.043)	(0.030)	(22.6)

Notes: Shows the correlation between baseline taxpayer knowledge and the outcomes measured at endline. Standard errors are clustered at the geographic collection unit level. p-values $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$

B Randomization Design

Collection Units

The initial randomization was conducted for 50 collection units. After the initial randomization, but before tax officials were assigned to collection units, one tax official dropped out of the study due to personal matters. Prior to assigning tax officials to collection units, we then randomly dropped one collection unit pair as described below to account for the one fewer tax official.

Each of the initial 50 collection units $j \in \mathcal{J}$ was randomly assigned to one of the four experimental arms $\{CG, TG1, TG2, TG3\}$ in the following process.

1. Randomly create 25 pairs of two collection units; unit of randomization is these pairs.
2. Compute pair-level average property valuations based on administrative data for the collection units. Assign collection unit pairs to greater $(>)$ or less than or equal to $(\leq)$ the median of the average property valuation of these pairs for stratification (let λ denote the strata).
3. We target the following composition of pairs across the experimental arms: $n_{CG} = 5$, $n_{TG1} = 7$, $n_{TG2} = 5$, $n_{TG3} = 8$. Given the odd number and desired stratification, we randomly pick between the below-above median compositions
   - $n_{CG,below} = 2$, $n_{TG1,below} = 4$, $n_{TG2,below} = 3$, $n_{TG3,below} = 4$
   - $n_{CG,below} = 3$, $n_{TG1,below} = 4$, $n_{TG2,below} = 2$, $n_{TG3,below} = 4$
4. Randomly assign pairs based on a new random number ordering to $\{CG, TG1, TG2, TG3\}$.

We repeat this randomization 1000 times and after each iteration perform balance tests on the structure-, collection unit-, and pair-level. For each iteration, we keep the lowest p-value across the structure-, unit-, and pair-level tests. Out of all iterations, we keep the assignment corresponding to the largest p-value.

Within each pair, collection units were randomly assigned to wave 1 and wave 2.

Adjustment to tax official dropout. Before the officials were assigned to their collection unit pair, one official dropped out of the study (as they were no longer available to work during the study period). We thus randomly dropped one collection unit pair in the following way. First, we randomly selected one pair to be dropped. Then, we conduct the same balance test as in the initial assignment. We perform this procedure five times and drop the collection unit pair which

maintained the highest p-value.

The local government still assigned a tax official to the collection unit pair removed for study purposes. Taxpayers and the tax official kept the treatment status they would have had as per the original assignment. We conducted all surveys with respondents and the tax official for these two collection units and did not disclose that the pair was removed from the study. While all data was collected as if the area was part of the study, we do not include it in our analysis.

Adjustment to increase property coverage. As described in section 4.3, we combined collection units of the local government with the goal of a unit covering about 60 properties. This was challenged by the absence of a property census. We created the property count based on the number of property structures in the local government's tax collection system. During the first days of the baseline survey in wave 1, enumerators' feedback suggested the collection units covered somewhat fewer than 60 properties due to some compound properties containing several structures. Since most collection units were not adjacent to another collection unit for at least one edge, we consequently expanded the collection units to increase the property coverage. The re-randomization was done based on the initial collection units whereas the eventual intervention and surveys took place in the (slightly) expanded units.

Figure B.1 shows the assignment of collection units to the experimental arms.

Taxpayers

In the control group (CG), no taxpayer was assigned to receive the education program. For respondents in any of the treatment arms (TG1, TG2, or TG3), half of the surveyed property owners received the education program. Respondents were randomly chosen as follows.

1. A list of respondent IDs was created prior to the baseline survey. On the collection unit level, a set of IDs was then pre-randomized to receive the intervention (unknown to the enumerator).
2. During the baseline interview, the enumerator filled in IDs in chronological order of conducting the surveys. Until the end of the interview, the enumerator did not know whether the household would receive the education program or not. At the end of the interview, the survey software automatically triggered the dedicated part for the education program based on the pre-randomized list of IDs.

Figure B.2 shows the assignment of survey participants to the education intervention.

Tax Officials

The tax official assignment to collection unit pairs was conducted *after* one of the initial tax officials dropped out. The tax official assignment was thus conducted for 24 tax officials, who were assigned to the already reduced set of 24 pairs.

Tax officials are randomly assigned to a collection unit pair. By assigning a tax official to a collection unit pair, the tax official inherits the treatment status corresponding to this pair. For instance, if the tax official is assigned to TG3, they receive the education program.

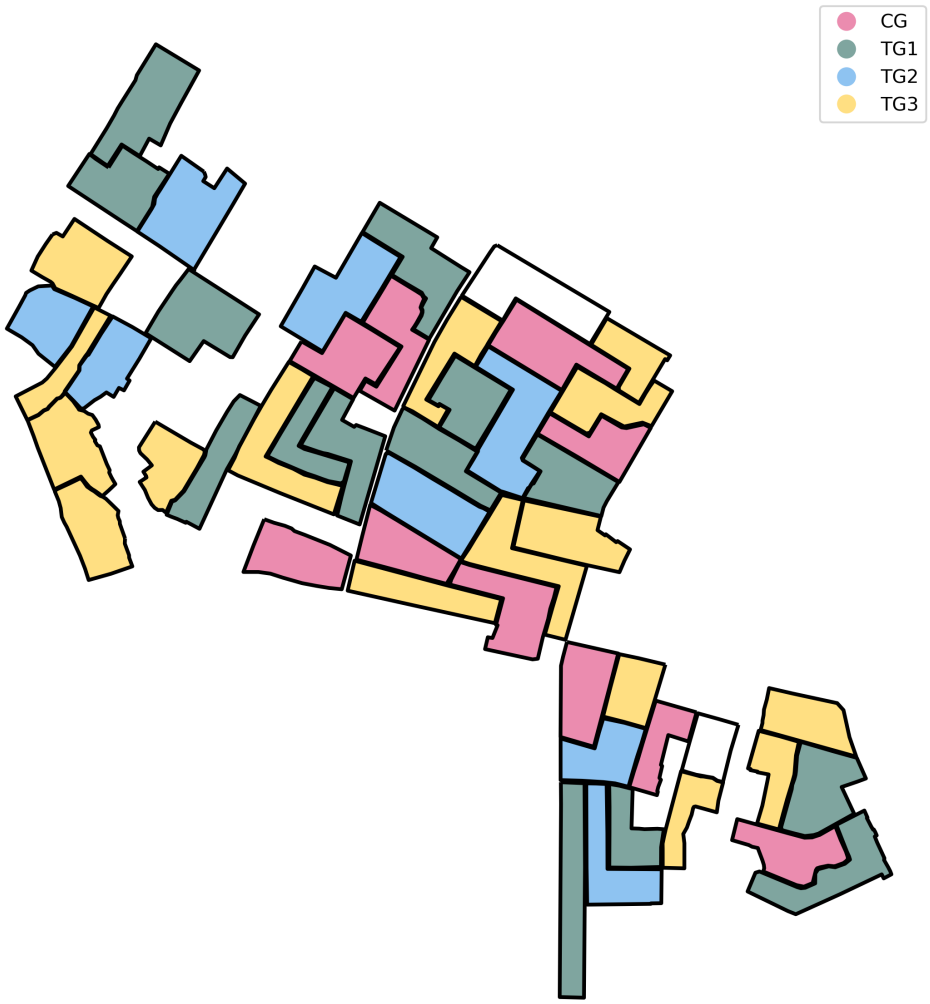
The tax official randomization used baseline information provided from our partner (e.g., on number of years and areas of work experience, performance score, tertiary education) as well as data from a baseline survey conducted with officials prior to their training. The treatment assignment was done after the training and not communicated during the tax official training. From the baseline survey, we created indices for tax knowledge as well as for views towards taxpayer behavior to lower their tax liability.

Tax officials were randomly assigned to pairs. We randomly draw a number for each official, sort them by these numbers, and assign pairs 1-24 in the corresponding order. We then perform balance tests—(i) within treatment arms across covariates and (ii) for each variable across treatment arms—and use the assignment out of 1000 iterations maximizing the min. p-value across these tests in a given iteration.

Highlighted Areas with High Treatment Density

After the taxpayer randomization and before the start of the tax collection, we manually created two areas within a collection unit that exhibited a high density of households who randomly received the education program. While these areas were only displayed to tax officials in TG2 and TG3, we also created these areas for TG1 for counterfactual comparisons. Moreover, we also created counterfactual areas for the control group (CG) to account for density of nearby properties itself. All of these areas were constructed irrespective of whether the respondent attended the education workshop.

Figure B.1: Assignment of Collection Units to Experimental Arms



Notes: The two blank areas correspond to the collection unit pair dropped from the initial set of collection units.

Figure B.2: Taxpayer Assignment to the Education Intervention

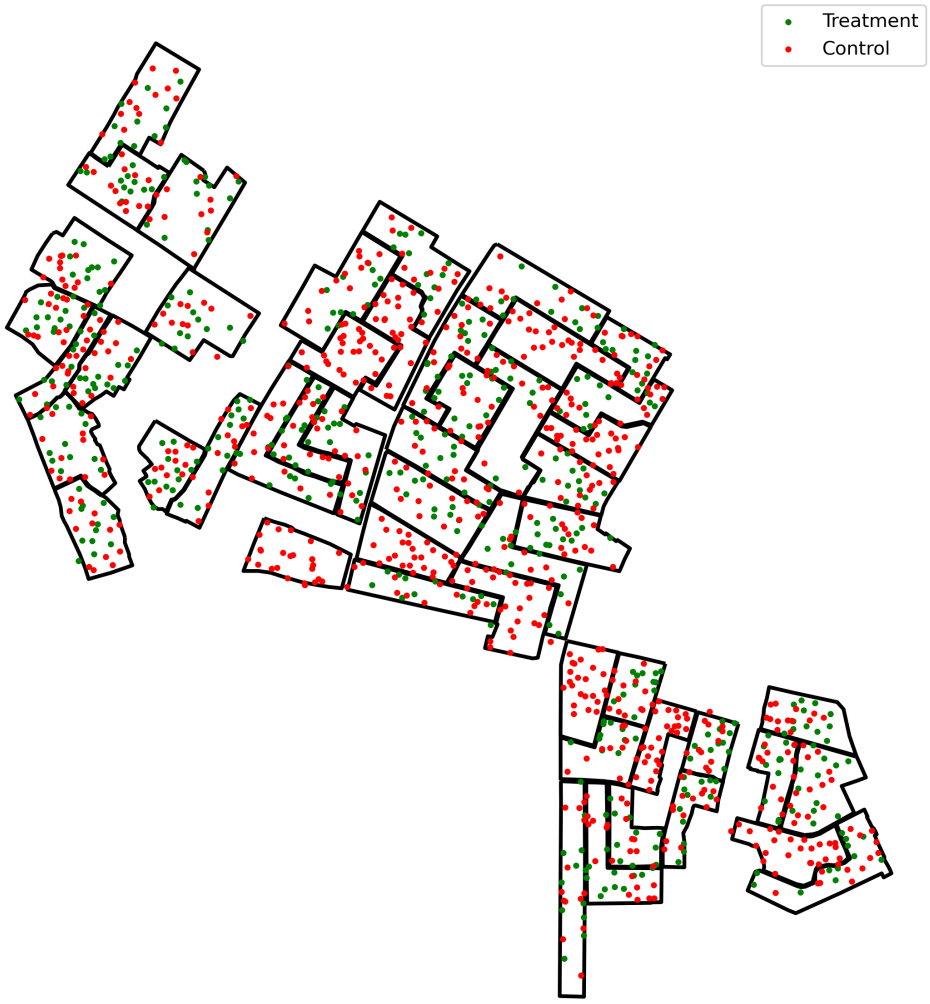
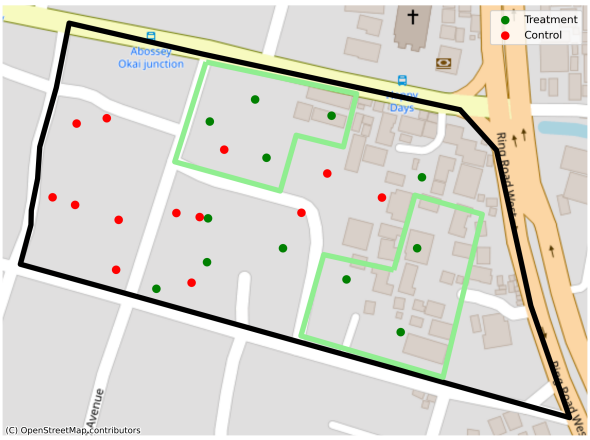
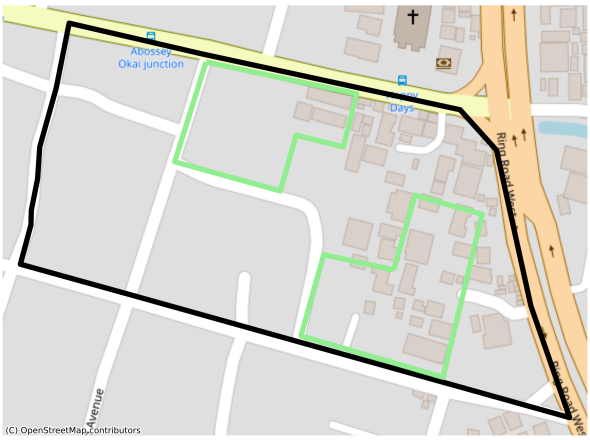


Figure B.3: Highlighted Treatment Areas in TG2 and TG3

(a) Assignment of Areas



(b) Displayed to Tax Official



C Timeline and randomization flowchart

Figure C.1: Sequence of household surveys, education, and collection in the two waves

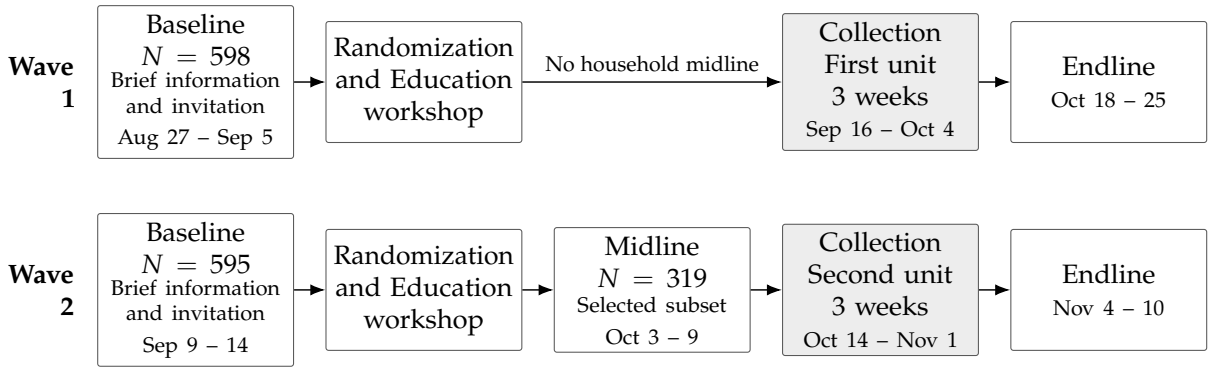


Figure C.2: Randomization assignment of property owners

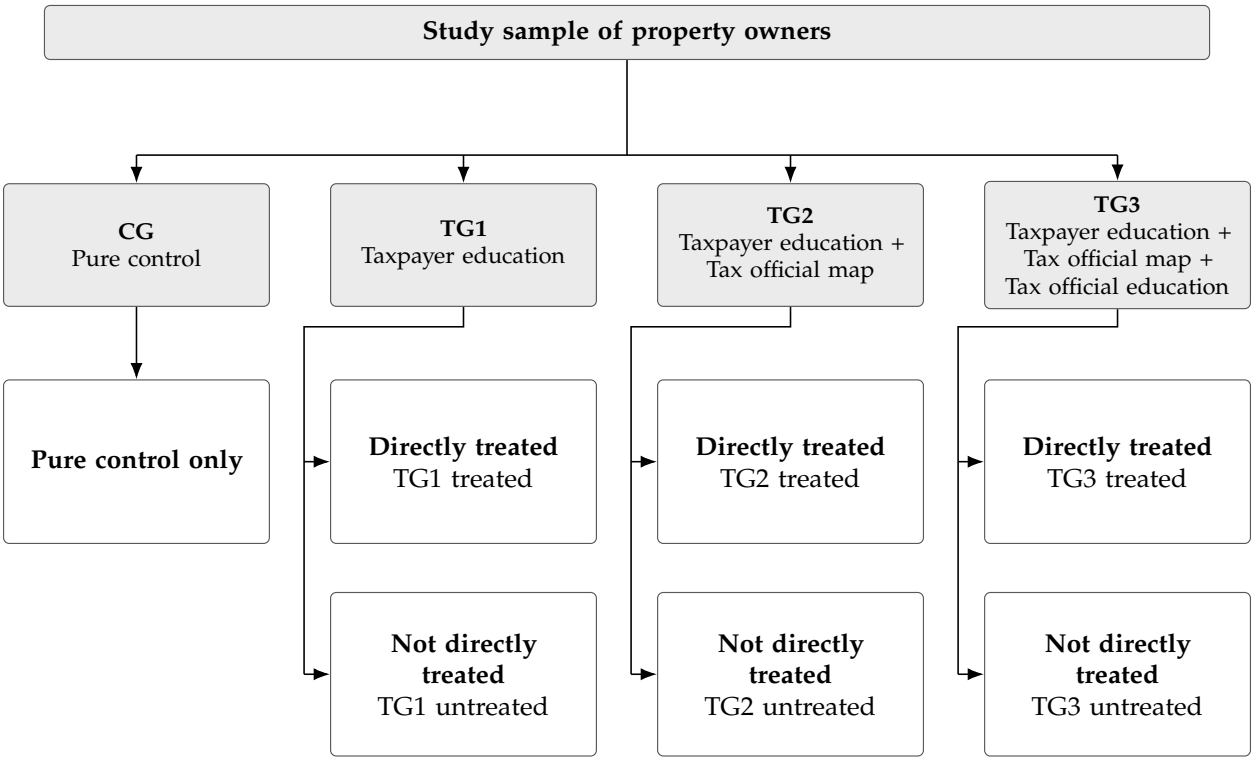


Figure C.3: Random assignment of tax officials

