Tax Knowledge in Equilibrium: Evidence from an Education Program in Ghana[*]

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June 2026

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Abstract

A property tax experiment in Ghana randomly provides tax education to property owners and officials. The intervention raises tax knowledge and generates spillovers to untreated households through conversations with more knowledgeable neighbors but has no ultimate impact on tax payments. Educated taxpayers and officials become more engaged in their interactions, but do not shift toward collaborative relationships. Taxpayers revise downward their beliefs about enforcement capacity and become more aware of legal ways to reduce their tax burden. These results highlight that tax knowledge spreads through social networks, and that its effects on tax outcomes are mediated by its ability to shape citizen–state interactions.

[*]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.

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1 Introduction

The taxation process is highly interpersonal in many developing countries. Despite the increasing use of technology in tax administrations, digital and automated processes are currently reserved for larger firms and well-off individuals. In contrast, the vast majority of taxpayers depend upon personal interactions with frontline government officials (Overbeck and Lungu, 2025), both to fulfill their tax responsibilities (register, file, pay) and exercise their tax rights (demand information, appeal, complain).[1] These interactions are often unsystematic, contingent and personalized.

When the tax collection process is interpersonal and discretionary, tax-related knowledge has the potential to shape the interaction between taxpayers and tax officials, and ultimately influence key tax outcomes. A key constraint, however, is that knowledge among both taxpayers and tax officials can be low. In our setting, property taxes in an urban municipality in Ghana, 92% of property owners could not correctly name a single characteristic that determines their property's tax liability, and 5% could only name one.[2] Among frontline tax officials, who deliver the property tax bill, assist property owners, and collect payments, 31% could not name a single such characteristic, and 37.5% could name only one. Beyond our context, studies from many settings have observed that citizens and bureaucrats are ill-informed about the tax and transfer policies that are relevant to their own circumstances (de Jantscher, 1992; Chetty, Looney and Kroft, 2009; Aiko and Logan, 2014; Liebman and Luttmer, 2015; Stantcheva, 2021; Mascagni, Santoro and Mukama, 2024).

Perhaps in recognition of these challenges, tax education is rapidly becoming a strategic line of operations for many tax administrations (OECD, 2015, 2021), as part of a broader organizational trend where tax authorities pivot from enforcement to service provision. This trend is based on the intuition that educational outreach may stimulate civic engagement and accountability. There is, however, very little rigorous evidence on the impacts of tax education campaigns. Is knowledge elastic to a 'typical' campaign? Is knowledge power in an interpersonal tax process, and if so, how? Does information about taxpayers' knowledge impact tax officials' field strategies? Is increasing taxpayers' knowledge sufficient to improve the interaction with officials and impact tax outcomes, or does it require the simultaneous increase in knowledge on both sides? We make progress on these questions by embedding a multi-arm experiment in a property tax campaign in

[1] In Zambia, for instance, more than half of the registered firms interact personally with tax officials on a regular basis to discuss their tax liability and negotiate a formal payment (Overbeck and Lungu, 2025).

[2] Such characteristics include, for instance, the location of the property (which matters for the tax rate), the use of the property, and various property attributes that influence property valuations.

a local government in Ghana that introduces random exposure to tax education for both taxpayers and tax officials.

Our experiment was implemented in partnership with the local government of Ablekuma Central, a district in the Greater Accra Region of Ghana. The local government hires tax officials who deliver the tax bill to property owners, assist them with queries, collect payments, and conduct follow-up enforcement if needed. In collaboration with the Institute of Local Government Studies, a quasi-government institution to train public officials, and a private-sector tax expert, we designed educational content to improve both 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 series of workshops, conducted by an independent tax expert hired by our research team. All treated property owners received an invitation to attend the workshop. Moreover, at the time of the invitation, trained enumerators (also hired by our team) delivered a brief presentation of the education content.

Our experiment consists of three treatment groups (TG1, TG2, TG3) and a pure control group (CG). The treatment groups share a common household-level intervention: within each TG tax collection unit, a random subset of households receive the educational content described above. In all three TGs, an equal number of households were randomly assigned to treatment; we refer to them as TG-treated and to the remaining untreated households in the TG units as TG-untreated (who allow us to study spillovers of the intervention). The three groups differ only on the tax official side. In TG1, officials receive no additional information or training. In TG2, officials additionally receive spatial information identifying areas with a high density of educated households (due to their TG-treatment status), allowing us to test whether officials strategically respond to the presence of educated taxpayers in their assigned work area. In TG3, officials receive both the spatial information and the same educational content as treated households, allowing us to test whether increasing knowledge on both sides alters the nature of interactions and tax outcomes. Geographical clusters of property owners were randomly paired with officials, and those pairs were then randomly assigned to a treatment or control group. In turn, a random subset of property owners in the three treatment groups were assigned to the household-level intervention.

We begin by discussing two results on knowledge. First, we find significant effects 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 tax officials were treated, suggesting these effects are not driven by spillovers from a more educated tax official on the taxpayers in their work area. We also find positive knowledge effects for TG3 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 find that both TG-treated and TG-untreated households are more likely to have conversations about property taxes than CG households, including on practical topics. By leveraging our randomization protocol, 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. The magnitudes suggest 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 knowledge of 1.6.

The intervention successfully increased knowledge of both taxpayers and tax officials. What happens to the downstream outcomes? 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 TG1, TG2 and TG3, and remain fairly precisely estimated across multiple dimensions of household heterogeneity.

We frame our mechanism analyses as investigating if the intervention had effects on intermediate outcomes that could reconcile the positive treatment effect on knowledge with the null effect on tax outcomes. The first potential mechanism is that officials strategically avoid educated households, for example by visiting them less. 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.

The second potential mechanism is that, conditional on an interaction, our intervention changes its nature. While interactions could become more fruitful—e.g., due to taxpayers' knowledge of their

responsibilities—leading to more tax payments, they may also become more confrontational—e.g., due to taxpayers' knowledge of rights—potentially disengaging the households from the taxation process, and even lowering compliance. In the endline survey, households in treated units rate themselves more positively and are more actively engaged in the interactions. Remarkably, these effects are almost as large for TG-untreated households as for TG-treated ones, and they exist outside TG3. However, there are no treatment effects on households' rating of tax officials in the interaction, including in TG3. As a result, we see no movement towards more co-productive, collaborative relationships; instead, more households in treated units report having a clear sense of their own objectives while assessing the tax official to be unresponsive. Tax officials were asked nearly identical questions and we find qualitatively similar but more muted effects among TG3 officials. Knowledge, then, increased taxpayers' self assessment and engagement during interactions but taxpayers who become more able and willing to engage did not find a corresponding willingness or ability to engage on the official's side.

The third potential mechanism is that the education intervention shifts taxpayers' perceptions and beliefs about the tax process and government. We find support for some changes here, but not for others. First, we observe no effects on tax morale among either TG-treated or TG-untreated households. Second, both TG-treated and TG-untreated households are more likely than CG households to know about actions that can minimize their tax payments (reflected in the practical knowledge gain), but they are no more likely to have undertaken any of them. In turn, this may be because they hold more accurate views about these actions: more often than CG households, they perceive that the circumstances justifying such actions do not apply in their case or that the government may not take them seriously. Third, households in treated units perceive the local government as having lower enforcement capacity than CG households do; this change in perception is correct as CG households overestimate enforcement capacity. This belief updating happens for both TG-treated and TG-untreated households, highlighting how educational content propagates among taxpayers through social interactions. These changes in views on tax minimization and enforcement were likely not large enough to reduce compliance but they can reconcile the positive effect on knowledge with the null effect on tax payments.

Taken together, the results suggest that taxpayers are critically engaged consumers of educational content: they engage substantively with the material, allow it to inform their beliefs and actions, but maintain evaluative distance from the local government. In other words, educational content

stimulates cognition and agency, but not trust in or outright acceptance of the tax process. Through social interactions, the educational campaign generates a set of engaged taxpayers beyond those directly treated with an implied social multiplier well above 1. At the same time, increasing knowledge on the tax official side does not alter these dynamics.

We conclude by discussing whether the educational campaign was ultimately successful. As a multi-purposed intervention, there are several dimensions to this question. From a narrow evaluation perspective, property owners' knowledge is elastic to a relatively light-handed educational campaign. From a government revenue perspective, the returns seem low; in fact, the effects on enforcement capacity perceptions and tax planning knowledge may even suggest that the intervention backfired. From a broader societal perspective, however, the increased knowledge and engagement among taxpayers may eventually trigger the kinds of policy responses, including more comprehensive and sustained training among officials, that ultimately enhance accountability and build quasi-voluntary compliance.

Related literature We contribute to the literature on taxpayer knowledge and education programs. While many 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), 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 provides a direct experimental evaluation of a taxpayer education campaign in a developing country, with a design that allows us to study the equilibrium effects on information from both sides of the tax official-taxpayer interaction.[4]

We find that the education intervention has a large social multiplier for knowledge, driven by interactions between treated and nearby untreated taxpayers. To our knowledge, this is among the first direct experimental evidence on the spread of tax-related knowledge through social networks, consistent with findings from the EITC in the US based on taxpayer mover designs

[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.

(Chetty, Friedman and Saez, 2013).[5] These spillover effects further underscore that education's ultimate impacts are best understood in equilibrium and are imperfectly captured by the partial equilibrium effects among directly treated taxpayers.

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). While the nature of the interaction between citizens and frontline officials has been studied in the public administration literature (Döring et al., 2025), we know of no work in economics that focuses on the interaction itself, with the exception of Mascagni, Scarpini, Mukama, Santoro and Hakizimana (2025) who use a survey experiment with treatment vignettes to vary the nature of a hypothetical interaction and find that interactions impact taxpayers' morale and views on government. We complement this work by studying actual interactions in the field and the causal impacts of knowledge on the experience and nature of these interactions.

We find that increased knowledge makes taxpayers more agentic and engaged with the state. Scholars have long debated the benefits and costs of citizen agency. On the one hand, greater agency can underpin the emergence and persistence of the fiscal contract and foster a state that is accountable (Levi, 1988; North and Weingast, 1989; Acemoglu and Robinson, 2019). On the other hand, increased agency can also allow taxpayers to more effectively shield themselves from tax collection; while this may be beneficial if the collection process is overly extractive, it can also strain already scarce public resources when shielding facilitates evasion under limited enforcement capacity (Craig and Slemrod, 2024).

Our results suggest that empowering taxpayers may provide a first step towards a robust fiscal contract, with limited downside in terms of evasion or avoidance. The only other work on empowerment and interactions in the tax domain (Henn, Paler, Prichard, Samii and Sanchez de la Sierra, forthcoming) finds that providing citizens in the DRC with private tax consultants and protection by civil society against government officials increases tax payments and engagement

[5]Our work is also complementary to studies which show how non-knowledge tax shocks, primarily related to enforcement, travel through social and professional networks: Boning, Guyton, Hodge and Slemrod (2020) in US, Almunia, Henning, Knebelmann, Nakyambadde and Tian (2025) in Uganda, Lediga, Riedel and Strohmaier (2026) in South Africa, Drago, Mengel and Traxler (2020) for TV license in Austria.

with the state. We complement this work by asking whether a lighter intervention, an education campaign that could realistically be implemented by governments themselves, can move the needle on citizen empowerment and shape taxpayers' engagement with the state. In this sense, our work is close in spirit to studies on social transfers that seek to empower citizens through light information interventions on eligibility and program details (such as for subsidized rice programs in Banerjee, Hanna, Kyle, Olken and Sumarto, 2015).

The remainder of the paper is organized as follows. Section 2 describes the intervention. Section 3 describes the experimental arms and empirical strategy. Section 4 describes the data and sample. Section 5 describes the impact on tax-related knowledge and talking, and section 6 the impact on tax collection outcomes. Section 7 discusses mechanisms before section 8 concludes.

2 Intervention: Scope, Context and Design

This section describes our intervention, which seeks to induce random variation in both taxpayers' and tax officials' knowledge about local property taxes. We first conceptualize knowledge in the context of taxpayer education and then contextualize our intervention by providing evidence of limited property tax-related knowledge at baseline. We then proceed to describe the contents of our education program and discuss take-up.

2.1 Scope: Knowledge in the context of taxpayer education

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, 2015, 2021) and is the one we consider as the basis of the education intervention in this paper.

In the context of such a taxpayer education, 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).[6]

2.2 Context: Limited knowledge at baseline

Knowledge among taxpayers and tax officials at baseline 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 self-report that they do not know the formula for calculating the property tax (Khan et al., 2016).

Knowledge is similarly 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.

[6]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.

Knowledge and interactions Limited knowledge likely constrains taxpayers and officials in their ability to interact productively, which in turn may reduce ability and willingness to comply. This is possibly one of the mechanisms through which education, by enabling more productive interactions, may foster a culture of engagement and quasi-voluntary compliance.

Qualitative evidence from pilot focus groups links limited knowledge to the experiences of interactions. Many taxpayers complained that tax officials do not engage with them when distributing bills. 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." Participants were under the impression that officials themselves were not well informed about property taxation. Participants reported that tax officials typically provide no explanations when distributing bills and collecting payments on why one has to pay, how amounts are determined, why liability changed from previous years, or what the funds are used for. One participant stated that "we [the taxpayers] usually lament about the way they come for property tax without educating the public on what they are going to use the money for. They present the bill to us without any notification or recourse to explain to us in detail about how they come by that amount." Other taxpayers said: "the only explanation I got is that it is something that they have been paying since time immemorial. There was no further clarification about it after that and so I don't really know why we pay."; "When the bill was generated, the bill had shot up to over 100%. When I asked him why it is so, he said that, that is what we are being asked to pay now. But he didn't go into detail explaining to us about the rateable value and the other technicalities pertaining to the bill."

These interactions leave taxpayers feeling discontented and disrespected, as one participant summarized: "[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."

Survey evidence corroborates that tax officials' limited knowledge may impact the nature of these interactions. 50% of 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.[7] Similarly, 59% and 54% never provided information about why property owners have to pay or what the property tax is used for, respectively. For 36% of taxpayers, the official was unable to answer their question in any interaction. 31% of visited taxpayers describe the tax official's

[7]These numbers are computed for taxpayers from the control group (CG) who were visited by tax officials.

behavior as "unpredictable" in at least some interactions; analogously, 90% of tax officials (in the control group) describe the taxpayer behavior as "unpredictable" in at least some interactions.

The qualitative and survey evidence point to a setting in which limited knowledge inhibits collaborative encounters. In public administration research, collaborative encounters are viewed as the most productive form of state–citizen interaction (Döring, 2021). They are characterized not by identical interests, but by a shared understanding of the purpose and basis of the encounter, with interactions structured around clarification, explanation, and problem-solving rather than confusion, suspicion, or coercion. In our control group, only 24% of taxpayers describe their interactions with tax officials as collaborative, and only 13% of tax officials describe at least half of their interactions with taxpayers as collaborative. The education intervention may therefore increase compliance by shifting encounters in the collaborative direction. More knowledgeable officials may be more able to explain the legal and procedural basis for the tax demand, while better-informed taxpayers may be more able to ask questions and raise concerns. This can reduce uncertainty, make the tax demand appear less arbitrary, and create the conditions for quasi-voluntary compliance.

2.3 Design: Components of Interventions

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.[8]

[8]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

- *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".[9]

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.[10] 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 informed that they would receive compensation to cover the cost of commuting 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.[11]

The education content was developed by the research team in close consultations with the Institute of Local Government Studies in Accra (a quasi-government institution established to build capacity for effective local governance through education and training) and a local private sector tax lawyer. The objective of these consultations was to ensure that the content balanced rights and responsibilities in accordance with OECD (2015, 2021), and was technically correct and relevant. By incorporating revisions suggested by both the public and private sector actors, we aimed to deliver credible content while striking a neutral tone: the content should not appear to participants to be designed by their local government to coerce compliance (e.g., downplaying rights, emphasizing the responsibility to pay); conversely, the content should not be tilted towards tax minimization (e.g., downplaying responsibilities, emphasizing legal avenues to reduce the tax burden).[12] The workshop itself was delivered by the consulting local private sector tax lawyer.

There are many degrees of freedom involved in designing taxpayer education content. The first implication of our design choices is that the intervention is not heavy-handed. The content remains

responsibilities, the workshop covered: pay unless exempt; pay by due date; provide information as relevant; facilitate assessment and collection.

[9]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.

[10]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.

[11]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.

[12]Consistent with the neutral tone objective, treated households at baseline that expressed stronger satisfaction with the Ablekuma Central government or that were more inclined towards minimizing tax liability within the law were not more or less likely to attend the workshop (see Table A.1 and section 3.4).

purely informational in nature; the intervention does not directly execute actions on participants' behalf, or provide administrative, financial or material support. One upside of this choice is that the intervention more closely mimics an actual taxpayer education campaign that governments might undertake. Our design contrasts with more heavy-handed interventions, such as hiring tax lawyers to act on taxpayers' behalf. Though such intense interventions may have large effects on tax outcomes, the concern is that the results arise simply due to the lawyers' ability rather than any behavioral changes within taxpayers. On the downside, from an ex-ante perspective, there is a risk that null results reflect a lack of treatment intensity rather than the absence of causal links.

A second implication is that the content was purposefully designed to not emphasize the local government of Ablekuma Central. For example, we asked the private sector expert to lead the workshop, but in a neutral tone without making strong references to taxpayer actions. Similarly, the education content never referred to actual activities of the Ablekuma Central government.[13] The upside is that participants are less likely to perceive the content to be politically or administratively tainted, which could distort their willingness to engage with it and learn from it. The downside is that our intervention is less externally valid, since education campaigns are typically implemented by a specific local or national government. In other words, while our education intervention may isolate the effects of a genuine knowledge increase, the effects of a government-led education intervention will be further driven by participants' perception of the government's broader political and administrative intentions.

2.4 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 3.4). 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. Importantly, enumerators did not know a respondent's treatment status until the very end of the baseline survey and no untreated households received this explanation. 23% of the invited property owners (TG-treated) attended one of the workshops. At endline, we asked invited respondents who did not attend for reasons why they did not attend;

[13]The fiscal contract may have been more stimulated if the content highlighted the local government's actual performance; at the same time, highlighting the local government's activities may also have triggered negative updating among participants on the government's perceived capacity to deliver public goods, worsening tax outcomes for reasons unrelated to knowledge. Our intervention instead sought to stimulate the fiscal contract by explaining why local property taxes exist in general.

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). When interpreting our effects, it is important to keep in mind that only about one in four TG-treated households attended a workshop. Reassuringly, only three uninvited households attended the workshop.

Differential selection into workshop participation along determinants of taxpayer behavior would be a major concern in interpreting our results. Column (1) of Table A.1 investigates heterogeneous workshop participation and we find no differential selection on important observable characteristics, which we return to frequently throughout the analysis:

- An *income/wealth index*, which combines information on the number of days short of cash for basic expenditures in a typical month, difficulty of gathering 300 GHS in the next four days in a typical month, household earnings last month, and an asset index (ownership of motorbikes, cars or trucks, TV, electric generator, sewing machine, and radio);
- A *property tax history index*, which combines information on whether the household received a property tax bill in recent years, whether the household already received a bill for the year of the intervention (2024) prior to the start of the intervention, and whether they made a partial or full payment for 2024;
- A *tax planner index*, which combines views on whether taxes should be reduced because the money belongs to people who earned it, whether use of legal measures to reduce liability are justified, and disagreeing with the statement that one cannot decline to pay because it is one's civic duty to pay; and,
- A *local government satisfaction index*, which combines information on the respondent's satisfaction with personal dealings with local officials and satisfaction with public goods and services provision, respectively, and whether the respondent has ever attended a town hall meeting by the assembly.

Among treated households that receive an invitation to the workshop, those with higher values on these indices may be more likely to want to attend. However, Column (1) of Table A.1 shows no heterogeneous selection on any of these household characteristics. While selection into workshop does not correlate with these household characteristics, attendees have found it useful: 94.5% of those who attended the workshop said that they found the workshop helpful.[14]

[14] 90.42% of those who received information at baseline also said they found the information helpful.

3 Experimental Arms and Empirical Strategy

This section provides details on the implementation of our intervention, including the experimental arms, the collection units, and the randomization design. We then discuss the main empirical specifications for estimations at the taxpayer and tax official level, respectively.

3.1 Experimental Arms

Our experiment consists of a pure control group (CG) and three treatment groups (TG1, TG2, TG3). Half of the surveyed property owners in each treatment arm received the education intervention described in section 2.

The treatment of the tax official varies across the three treatment groups.[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 highlighted map containing two areas with a high treatment density.

The experimental design allows us to study the effects of both taxpayer and tax official education.

[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."

Ex-ante, it is unclear whether tax officials anticipate more educated taxpayers to be good for their own objectives (e.g., because such a taxpayer is more likely to know that they have a legal responsibility to pay) or bad (e.g., because such a taxpayer is more likely to seek lengthy clarifications or try to minimize their payment). 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.

3.2 Collection Units

During a fiscal year, the local government assigns collectors to designated geographical areas for six weeks at a time. The designated areas are called 'collection units' and are defined with geographical boundaries that create a cluster of physically adjacent properties. During each six-week campaign, collectors 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 collector 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 collectors. 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 (section 154). The property tax is only due once a year. In our control group, the likelihood that a property owner makes a property tax payment is 12% (Table 3), which is comparable both to other settings in Ghana (Dzansi et al., 2025), and other settings in the region.

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 collector to one unit in wave 1 (three weeks) and another unit in wave 2 (three weeks). Each experimental collection unit is approximately half the size of a unit that a collector would be assigned to for 6 weeks and included about 60 properties each.[18] Experimental units were selected such that no significant

[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

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).

3.3 Randomization Design

Tax officials and taxpayers were assigned to the four experimental arms (CG, TG1, TG2, TG3) in two steps. 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. 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). Within each 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. The 24 officials constitute the entire set of officials that were available to work at that time in the local government. The tax official then inherited the treatment status of the collection unit pair from step one. This randomization also used re-randomization based on the baseline survey and local government data. 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 collectors in CG. Since this 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

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.

controlling for this does not alter our results.

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

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).[19] 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 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).[20]

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.

3.4 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

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

[20] 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.

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.

3.5 Power and Minimum Detectable Effect Size

To benchmark the precision of the experiment, we report minimum detectable effects for the main endline outcomes. We calculate these using the endline distribution of outcomes in the control group; reported MDEs correspond to 80 percent power for a two-sided test at the 5 percent level.

Our calculations imply that the experiment is powered to detect moderate effects on knowledge and communication outcomes, but only relatively large effects on tax payment outcomes. For example, the MDE is about 0.16 standard deviations (s.d.) for the overall knowledge index and about 0.24 s.d. for the taxpayer-action knowledge index. By contrast, the MDEs are approximately 21 percentage points (pp) for receiving a bill and 11 pp for making any payment, relative to control group means of 31 percent and 12 percent, respectively. We should therefore interpret the null effects on tax payment outcomes cautiously: statistically, the null estimates do not rule out smaller positive or negative changes in compliance. However, the overall pattern of results and qualitative insights still point to the intervention increasing knowledge and tax-related discussions without generating a correspondingly large change in payment behavior.

4 Data and Sample Description

This section describes the primary and secondary data used in the analyses. Our primary data include surveys of property owners and tax officials conducted before, during, and after the intervention and tax collection campaign. Secondary data come from research partners (the local government and their service provider for a digital property tax administration platform) and GPS data recorded on the tax officials' work tablets during the collection campaign.

Property owners We conducted comprehensive baseline, midline, and endline surveys with property owners. The baseline and endline surveys were in-person and included all collection units in both waves. The midline was a phone interview conducted shortly after the education workshop and before the tax collection began, for a random subset of property owners in wave 2 (319 out of 595 households in wave 2).[21] The surveys covered a range of topics including socio-demographic information, property tax compliance, property tax knowledge, interactions with tax officials, and broader view of and engagement with the state. We also recorded property locations during the baseline and endline.

The first criterion for a respondent to be part of our study was that they 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., a daughter, son or caretaker). For ease of reference, we refer to them as property owners or taxpayers interchangeably. The second criterion was that the property must be used for residential or for mixed residential and commercial purposes; purely commercial properties were excluded.

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 42% of the respondents are between 41 and 60 years old, and about 44% are at least 61 years old. The average respondent has lived in the local government district for 33 years. About one-third of them have completed secondary education (one-fourth for tertiary). About half of the respondents 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. The average (winsorized) earnings are about GHS 2,300, or USD 140 at the time of the study. Somewhat more than half of the properties are co-owned by multiple family members, and most properties are for residential use only. At baseline, about one in four property owners report having received a property tax bill for the corresponding fiscal year, about half of whom say they have already made a full payment.[22]

Attrition from the baseline to endline is 7% and is balanced across treatment groups (both in the pooled and saturated specifications). Attrition is positively correlated with our income/wealth index (half a standard deviation higher among attriters), but not differentially so across the

[21] To include sufficient households who received the workshop invite, we oversampled from collection units assigned to the TGs, randomly sampling treatment group households with probability 0.75 and control group households with probability 0.4.

[22] It is possible property owners are mistaking the property tax with other local levies such as rent taxes or business licenses for mixed-use properties.

treatment groups. Attrition is also uncorrelated with the property tax history index, the tax planner index, and the local government satisfaction index.

Tax officials We conducted a comprehensive in-person baseline survey with tax officials. Shortly after the education workshop for treated officials, we administered an in-person "pen-and-paper" knowledge test (wave 1). During the tax collection campaign, we conducted weekly surveys (including at the end of the campaign; total of seven weeks) with recurring questions covering tax knowledge, views of the local government, perceptions of property owners in their unit, property owner interactions, challenges experienced in their work, collection strategy and preferences. Responding to the surveys was voluntary; the response rate was 100% in all weeks except in weeks 1 and 5 when the response rate was 87.5%. Attrition is balanced across treatment arms.

The tax officials were selected by the local government in collaboration with their third-party service provider for property tax collection (a local firm providing digital government services). The officials were selected through the standard process and all of them consented to participate in this study.[23] 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*. This data allows us to measure the time spent in a collection unit and the approximate properties visited.[24] The GPS data was available to researchers with lags of 1-2 days. We followed up with tax officials with missing data for a given day and reminded them to keep their tablet's GPS enabled while working, but the recorded data remain intermittent. For instance, tax officials may have spent time in the collection unit with the tablet off or in "airplane mode" to save battery. We conducted several tests to ensure that reported data patterns are balanced across treatment groups.

The local government administers property taxes through a digital portal provided through a local firm. From the portal, we obtained the universe of property structures recorded on their system prior to our intervention as well as data from tax officials' daily reports to their supervisors on the

[23] As part of the study, tax officials received a higher base payment than usual. Incentive-based pay for property registrations and tax collection was unchanged. Tax officials were also instructed that they would only receive payment for properties within their assigned work unit, and only for residential and mixed-use properties.

[24] In wave 1, 10 officials' tablets were randomly selected to record GPS data; in wave 2, all officials' tablets reported GPS data.

number of property owners visited and the number of property tax registrations carried out each day. Finally, the local government provided background information on the tax officials including their work experience, languages spoken, and a pre-campaign performance assessment.

5 Results on knowledge and talking

5.1 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 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 the increase in declarative knowledge at midline is primarily driven by 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 about property taxes (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.[25]

In Panel C, we evaluate the impacts of providing the same education content to tax officials in TG3. Despite the small sample size,[26] 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 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 results from the corresponding saturated specification for taxpayer knowledge. 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 investigates heterogeneity in effects on knowledge using the same dimensions as

[25] 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.

[26] 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 questions on 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.

those used for selection into workshop participation (income/wealth index, property tax history index, tax planner index, local government satisfaction index). We find no strong patterns of heterogeneous treatment effects.[27] This parallels the absence of heterogeneity in workshop participation. Together, these results suggest that while not everyone takes up educational content, both take-up and its knowledge benefits are uniform across household characteristics.

5.2 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 6 below), suggesting that the results for TG-untreated are not driven by those TG-untreated household 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%).[28] Columns (2)-(3) show that this

[27] The only heterogeneity that is significant is for higher income/wealth individuals on the pen-and-paper knowledge index. However, there is no corresponding heterogeneity for practical knowledge or for self-perceived knowledge. These mixed results caution us against concluding that there is genuine heterogeneity by the household's income/wealth.

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

increase is primarily driven by conversations with relatives and friends, with a smaller and noisier increase in communication with neighbors.[29] 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).[30]

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.

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 units' 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

[29] 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.

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

set of properties in radius k of household i.[31] 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 relative/friends jointly—it is possible that respondents consider 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.[32] 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.[33]

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

[32] Specifically, the predicted probability 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.

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

In summary, we find positive effects on knowledge for TG-untreated households when compared to control households, though they are confined to improvements in practical knowledge and self-perceived knowledge, with no effects on declarative knowledge. Additional analyses support the interpretation that TG-untreated households talk more because of a higher exposure to TG-treated households, and that these exposure-induced conversations generate practical knowledge gains for TG-untreated households.[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 important 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 faced by property owners; see section 2.4).

6 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

[34] Our re-centering exercise is unlikely to be powered to directly estimate the impact of exogenous exposure on TG-untreated households' knowledge, so we do not attempt to use the re-centered exposure as an instrument to estimate the effect of conversations on knowledge among TG-untreated households.

[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.

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 in the pooled specification 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 investigate heterogeneity, using 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 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

[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.

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

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.

7 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.

[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

7.1 Targeting of visits by tax officials

In the first mechanism, the education intervention may have affected tax officials' field strategies. Far fewer than all of property owners receive even a single visit, and targeting of visits has been found to be an important dimension of official behavior in the field (Dzansi et al., 2025). Concretely, in a setting like ours 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 exactly 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. Formally, we estimate selection regressions of the form

$$y_{ij} = \alpha + \phi \mathbb{1}(\text{Targeted})_i + \beta \mathbb{1}(\text{Targeted})_i \cdot \mathbb{1}(\text{TG treated})_{ij} + \mu \mathbb{1}(\text{Targeted})_i \cdot \mathbb{1}(\text{TG untreated})_{ij} + \varepsilon_{ij} \quad (6)$$

where y_{ij} is a baseline household characteristic that can be targeted, and $\mathbb{1}(\text{Targeted})_i$ is the measure of targeting (e.g. receiving a visit, receiving a tax bill). ϕ captures the (not causally identified) association between the targeting variable and the household characteristic among control officials (indicating whether households with higher values of y_{ij} are more or less likely to be visited or receive a tax bill). β and μ capture whether being a TG-treated or TG-untreated household causally alters the extent to which that household characteristic is targeted. As proxies for potential targeting characteristics y_{ij}, we use the same baseline indices as in the heterogeneity analyses (income/wealth, property tax history, tax planner measures, local government satisfaction).

For both visits (Panel A) and bill delivery (Panel B), Table A.14 shows no differential targeting 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.

Two additional results support the absence of differential targeting strategies. First, there are no heterogeneous effects on selection into workshop participation or on knowledge effects along these same dimensions—otherwise, results on differential targeting could have reflected heterogeneous extent of knowledgeable households across dimensions. Together, the absence of heterogeneous

treatment effects on knowledge, targeting and public finance outcomes paints a consistent picture.

Second, Appendix Table A.15 shows treatment effects on tax officials' targeting preferences from weekly surveys. While officials generally target households with past tax compliance, knowledge of taxpayer duties, and public service satisfaction, they do not express a strong preference to avoid or seek out households that know more about taxpayer rights. Mirroring the null results based on the household survey (Table A.14), there are no systematic differences across treatment groups in the extent to which tax officials target any household characteristic, including knowledge. These null results hold even for TG3 officials, who are more educated than those in TG1 and TG2.

These results have two implications. First, while a more heavy-handed intervention may have shifted strategies, our 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. Moreover, increasing the education level of the officials (in TG3) does not change their targeting strategies either. Thus, differential targeting cannot reconcile positive effects on knowledge with null effects on tax outcomes. Second, the absence of differential targeting 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.

7.2 Nature of interaction conditional on visit

A second mechanism through which the intervention increased 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 nature of interactions.[42] Nearly all questions related to interactions were asked identically to both households and tax officials, allowing us to assess how the

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

intervention affects each side's assessment of themselves and the other side in the interaction. Part I of the table reports household responses, part II reports official responses. Within each part, the first Panel (Panels A, C) focuses on specific aspects of interactions, while the second Panel (Panels B, D) covers the respondent's overall assessment of the interaction.

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 indirectly (TG-untreated) by communicating with the TG-treated households. Since TG-untreated households showed null results on declarative knowledge questions but positive effects on conversations about taxation and awareness of available actions, their increased sense of satisfaction and usefulness does not derive exclusively from formal declarative knowledge.

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 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 column (2) and (3), suggests 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.[43]

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

[43]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 property tax laws and regulations should be interpreted and applied".

interactions and coordinating on next steps (column 11).

The collaborative relationship was defined in Section 2.2, 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.

In summary, the results from part I suggest that education increased the households' sense of usefulness and competency in interactions with officials, and gave them a clear sense of their objectives, but did not shift their overall assessment of interactions with officials in a positive direction. These effects extend to TG-untreated households, who likely gained practical knowledge through conversations with TG-treated households even without gains in formal knowledge.

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.[44] 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 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.

[44]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.

In summary, the results from tax official surveys are 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.

7.3 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 the property owners more of a 'tax planner', that is, a taxpayer who uses increased knowledge about taxation to seek 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 a certain action was not taken. For a given action and reason, we define a dummy variable equal to 1 if the respondent did not take the action for that specific reason. In other words, the outcomes combine any treatment effect on taking the action with any effect on the reason for not taking it. Since Table A.18 shows no effects on taking the action, we cautiously interpret the results in Table A.19 as reflecting the intervention's impact on the reasons for not taking actions.

The results in Table A.19 show that households have sophisticated reasons for not engaging in tax planning. For bill reduction, TG-treated households are more likely to say that they do not take this action 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 household are more likely to anticipate that the assembly will not grant their request—a sign of forward-looking reasoning. For requesting a property revaluation, both TG-treated and TG-untreated households are more likely to say that they do not take the action because they do not think the reasons for requesting one are justified in their specific 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.

These results indicate that the intervention did not lead to more households behaving like tax planners. We also found, in the earlier sections, that being a tax planner at baseline did not differentially affect participation in the workshop, knowledge, differential targeting by tax officials, or ultimate tax outcomes.

In summary, the education intervention neither interacted with baseline tax planning inclinations nor transformed households into active tax planners. While it raised awareness of available tax planning actions, it also improved households' understanding of why such actions may not be effective or justified in their specific case. Thus, these results provide an intuitive channel for the positive knowledge effect to translate into 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 report the effects on different components of tax morale: satisfaction with and perceived competence of the local government (column 1); interest in attending public meetings at the local government headquarters (column 2); the share of a hypothetical GHS 10,000 allocated to the local government versus religious and community organizations (column 3); views on the government's efforts to digitalize tax registration, delivery, and payment (column 4); and perceived corruption (column 5). While the education content could in principle have affected these components of morale, we observe no treatment effects, neither for TG-treated nor TG-untreated households. Table A.20 confirms this in the saturated specification.

Thus, the education intervention did not shift tax morale in either direction. Moreover, as shown earlier, 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. For investigation of mechanisms, we conclude that: the null effects on tax morale may help account for the observed absence of effects on tax outcomes; the null results also indicate that exposure to educational tax content, directly or indirectly through communication with those directly exposed, does not by itself cause significant improvements in tax morale.[45]

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, a channel particularly relevant for TG-untreated households. In column (6) of Table 8, we observe that both TG-treated

[45]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.

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: education raises 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.[46]

In summary, the education intervention caused households to correctly revise downward their perception of government's enforcement capacity. This revision could in principle reduce tax compliance among TG-treated and TG-untreated households relative to control households; we do not observe a significant change in payment likelihood, but the shift in perceptions of enforcement may not be large enough to produce a detectable effect on tax payments. More specifically, for the purposes of our mechanism investigation, the negative effect on enforcement perceptions is consistent with the intervention causing an increase in knowledge but no increase in tax payment.

8 Conclusion

In developing countries, most taxpayers engage with the tax system through personal, discretionary interactions with tax officials. Knowledge has the potential to shape the nature and experience of these interactions, and ultimately determine key tax outcomes. In this study, we embedded a multi-arm experiment in a property tax campaign in urban Ghana to study whether and how tax education affects knowledge, collection strategies, interactions, and tax payments.

[46]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).

The education intervention successfully increased both declarative and practical knowledge among directly treated households with effects that were meaningfully large given that workshop attendance was only 23%. It also generated substantial spillovers: untreated households showed significant knowledge gains, driven by conversations with directly treated neighbors, implying a social multiplier for knowledge of 1.6. Moreover, among tax officials who received tax education, we also find positive effects on knowledge.

However, we find consistent null effects across the main tax outcomes—visits by tax officials, tax bill delivery, and payment of property taxes—in all treatment arms. Officials did not strategically avoid or seek out educated taxpayers. Instead, the intervention changed the nature of interactions: households in treated areas became more engaged and rated themselves more positively in interactions with officials, and educated tax officials similarly perceived themselves to be more engaged. Yet neither side's overall assessment of the other improved, even in collection units where both received educational content. Each side's increased engagement failed to register with the other, and the intervention did not produce the shift toward collaborative, co-productive relationships that might have been required to improve tax outcomes.

These findings suggest that knowledge alone may be insufficient to alter tax outcomes in settings where interactions remain discretionary and unsystematic. Moving toward collaborative relationships that underpin quasi-voluntary compliance may require reforms that go beyond improving tax knowledge. This raises a natural question: what complementary reforms might allow knowledge to translate into improved compliance? One possibility is that knowledge must be paired with improvements in enforcement capacity; in our setting, the education intervention led households to correctly revise their enforcement perceptions downward, potentially offsetting any compliance-enhancing effects of greater engagement. Similarly, educated households became more aware of actions that could minimize their tax payments. While they did not undertake these actions, owing to more realistic views about their applicability, the combination of downward-revised enforcement perceptions and increased awareness of tax planning options suggests that the education intervention may, on net, have worked against compliance.

A second consideration concerns program design: the large social multiplier means that the reach of education extends well beyond directly treated individuals, but the content that propagates through social networks may not uniformly favor compliance. Identifying complementary policies that harness these more agentic, engaged taxpayers as a result of tax education, while addressing

the structural barriers that prevent knowledge from improving tax outcomes, remains an important challenge for education campaigns. At the same time, there is reason for optimism: the engaged, knowledgeable taxpayers that the intervention led to—extending well beyond those directly treated—may themselves create demand for the kinds of policy responses, including more comprehensive and sustained training of tax officials which may ultimately enhance accountability and promote quasi-voluntary compliance.

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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 * $\leq$ 0.1, ** $\leq$ 0.05, *** $\leq$ 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 6 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 * $\leq$ 0.1, ** $\leq$ 0.05, *** $\leq$ 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 7.2. 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

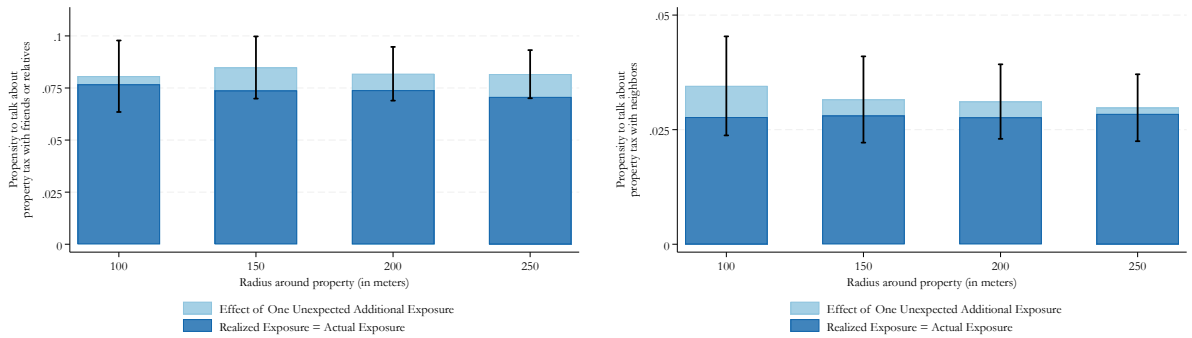
Notes: Shows ITT effects estimated following equation (1) and including strata FE. Column (1) reports the average of whether the "taxpayer is satisfied with the overall quality of public goods and services delivered by the assembly" and whether the "taxpayer find the assembly competent". Column (2) captures 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 5.2 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 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 are 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 row show the average realized exposure to TG treated households for TG untreated and CG households, respectively. The predicted probability 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 estimated using equation (6). 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 * ≤ 0.1, ** ≤ 0.05, *** ≤ 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 the each panel (multiple answers possible per respondent). Standard errors are clustered at the geographic collection unit level. p-values * ≤ 0.1, ** ≤ 0.05, *** ≤ 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

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 3.2, 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.

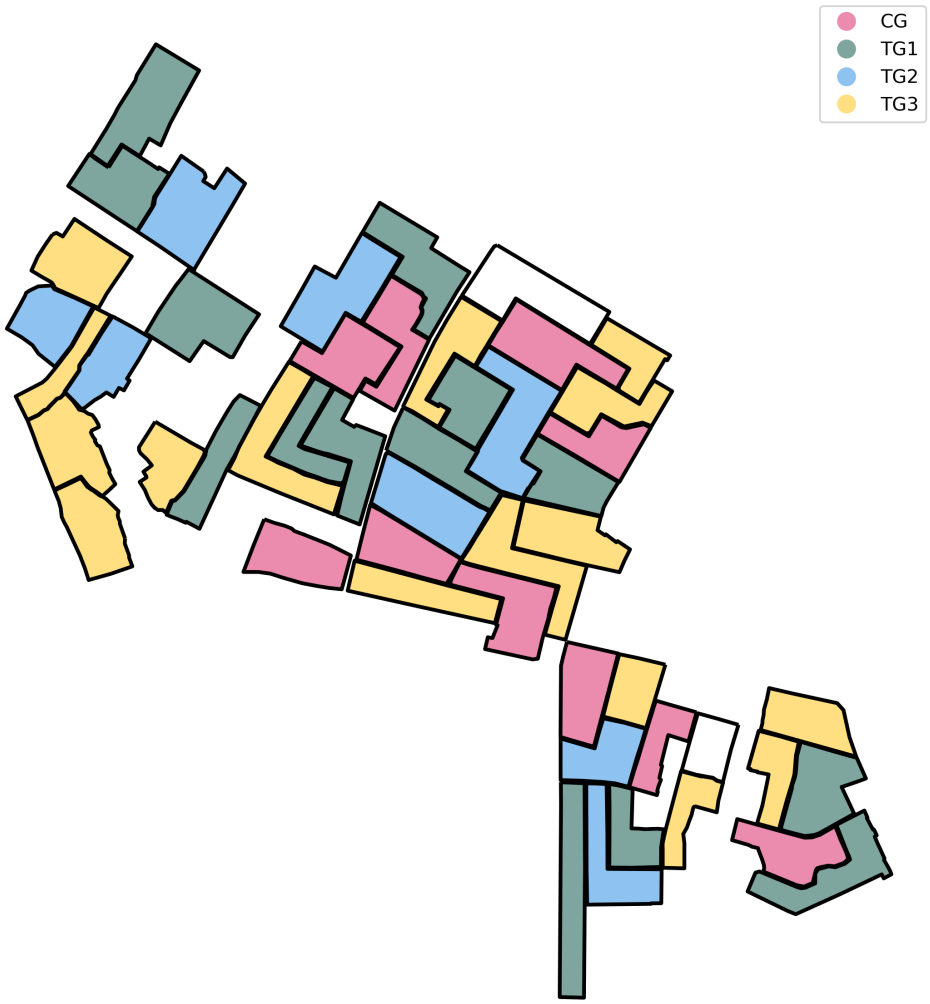
The tax official randomization used baseline information provided from our partner (e.g., on number of years and areas of work experience, performance score, tertiary education) as well as data from a baseline survey conducted with officials prior to their training. The treatment assignment was done 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

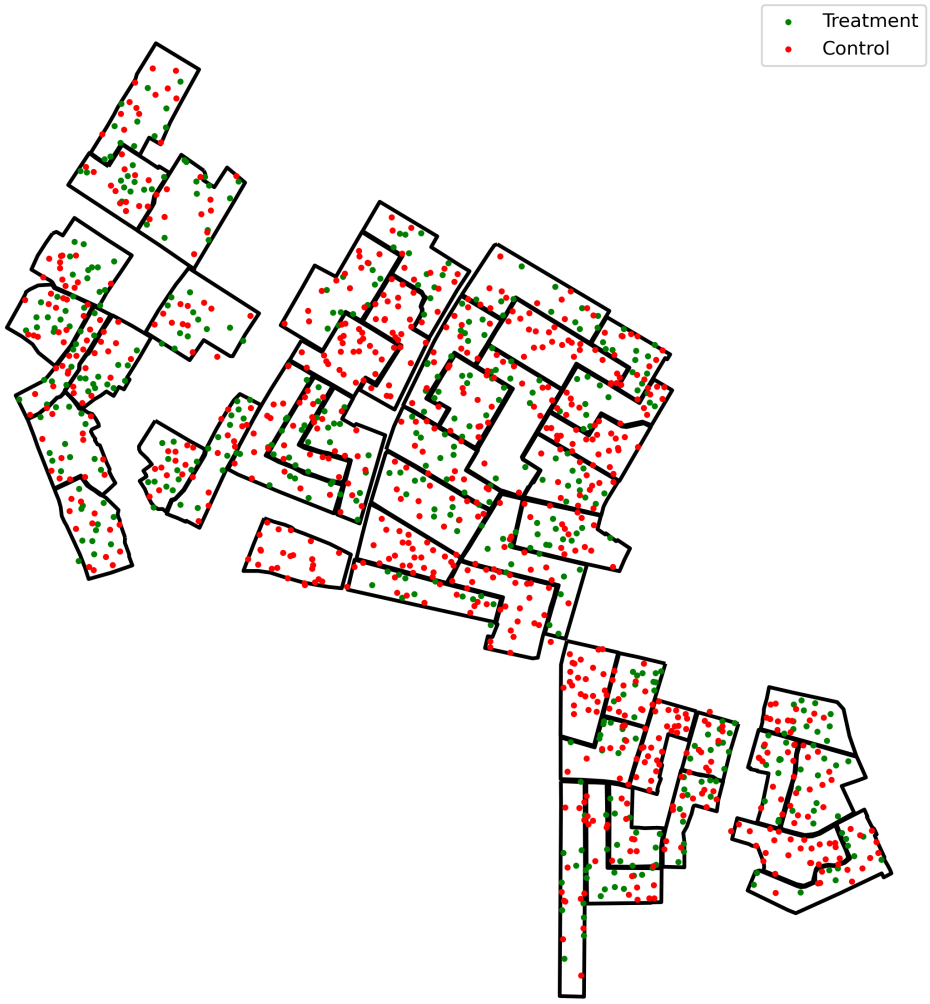
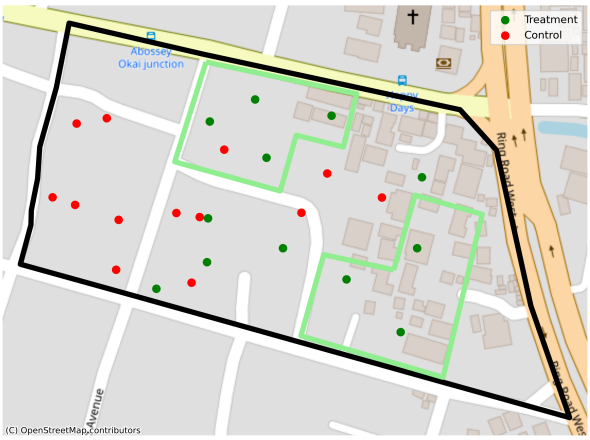
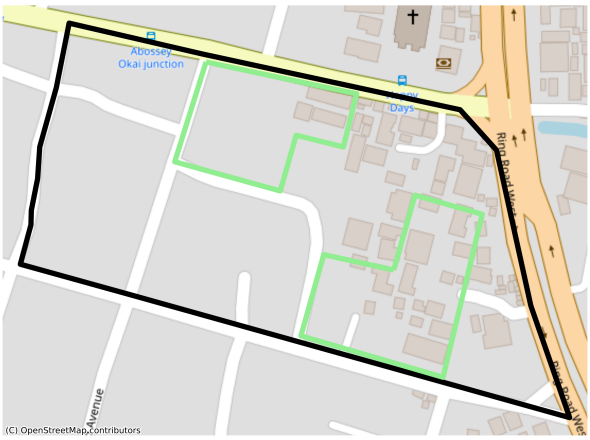


Figure B.3: Highlighted Treatment Areas in TG2 and TG3

(a) Assignment of Areas



(b) Displayed to Tax Official