Working paper UGA-20088 August 2023

Public disclosure and tax compliance

Evidence from Uganda

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August 8, 2023

Abstract

Public disclosure policies have potential to raise tax compliance where alternative enforcement capacity is limited. We study the effects of reporting delinquents and recognizing compliers, and provide evidence on the social determinants of tax compliance. Our results are consistent with a model in which being publicly known as tax-eligible is costly but social sanctions for delinquency are limited. Further, disseminating information on tax behavior reduces the compliance of recipients by causing their beliefs to be updated down toward the true compliance rate. Overall, these policies are limited at raising revenue and less effective than simple enforcement reminder nudges.

JEL codes: O18, H30, H26
Keywords: property tax, tax morale, public disclosure, shaming

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Raising tax revenue is a challenge in many low-income countries where, despite widening tax nets, collection rates remain low and means of enforcement are limited (Besley & Persson 2014). For instance, only around 10% of taxpayers pay their property taxes in sub-Saharan African cities (Okunogbe 2019, Bergeron et al. 2019, Cogneau et al. 2020, Ahabwe et al. 2020). With limited state capacity, there is demand for policies that aim to increase 'voluntary' compliance through channels of 'tax morale' (Luttmer & Singhal 2014).

Publicly disclosing the tax behaviour of individuals is one common policy tool that aims to leverage social dimensions of tax morale to raise tax compliance.[1] These policies involve publicizing information on the payment behavior of individual taxpayers in the form of shaming tax delinquents, recognizing tax compliers, or indiscriminately publishing taxpayer behavior.[2] The ability to implement such programs despite limited administrative capacity makes them particularly appealing to governments in low-income countries. This is despite the fact that conventional policy instruments such as financial penalties are considered the primary determinants of tax compliance (Slemrod 2019).

However, most evidence on such programs is based on research in high-income, high-compliance contexts which may not be externally valid for low-income countries where, among other things, low rates of compliance may shape norms around tax evasion (Traxler 2010, Besley et al. 2022).[3] As such, it is important to ask: what are the effects of public disclosure policies on tax compliance in a low-income setting, and how do these compare to alternative policies to raise compliance? Further, what can we learn about the social dimensions of tax morale? Do taxpayers expect to face social sanctions if it is known that they do not pay taxes, or social gains to being seen as a complier? How do beliefs about compliance of the wealthy, or the general public, affect motivations to pay?

To answer these questions, we study the impact of publicly disclosing property tax behavior in the city of Kampala where incomes are low, baseline tax compliance is minimal, and administrative capacity is relatively weak. We design two cross-randomized, multi-armed, field experiments with over 65,000 taxpayers to test the effects of delinquent disclosure and complier recognition policies on tax compliance, and how they compare to more conventional enforcement nudges. To understand the role of mechanisms, we develop an extension of the Allingham & Sandmo (1972) model of tax evasion that includes a set of social payoffs. We use the structure of the model to interpret our estimates

[1]The OECD (2017) cites disclosure as the fourth most used instrument of tax debt enforcement behind: obtaining a lien over assets; initiating bankruptcy or liquidation; and imposing a liability on company directors for company tax debts, and ahead of: temporarily closing a business or withdrawing a license; denying access to government services; and imposing restrictions on international travel.

[2]Nakayama (2021) reviews a selection of public disclosure policies, listing eight countries that engage in shaming, or what they call 'targeted' disclosure, and four countries that engage in recognition, or what they call 'honouring'.

[3]There is a long literature on public disclosure policies in developed countries (Perez-Truglia & Troiano 2018, Reck et al. 2022, Bø et al. 2015, Hasegawa et al. 2013, Angaretis et al. 2022, Lenter & Slemrod 2003, Alm et al. 2017, Dwenger & Treber 2022).

others' tax behavior can have an effect on one's tax morale and therefore their decision to comply (Del Carpio 2022, Bicchieri & Xiao 2009, Schultz et al. 2007).

We formalize these conceptual mechanisms as social payoffs in the framework of the seminal Allingham & Sandmo (1972) model of tax evasion, and use this to demonstrate how public disclosure policies affect equilibrium tax compliance. Public disclosure policies can change the probability that peers believe an individual to own properties, as well as the conditional probability that peers believe an individual to be in compliance with the property tax. Further, public dissemination of compliance information can change recipients' beliefs about their peers' compliance and therefore their compliance behavior as well. Under a set of simplifying assumptions, we apply this structure to help interpret our empirical estimates as products of the underlying mechanisms.

We conduct surveys with taxpayers at baseline and endline to provide measures not available in administrative data. The baseline survey provides us with descriptive statistics on taxpayer values and beliefs around tax compliance and norms, whilst in the endline survey, we focus on a narrow set of values and beliefs for an expanded set of taxpayers. This allows us to estimate treatment effects on outcomes such as beliefs about compliance rates of different peer groups, and other taxpayer sentiments.

There are five key takeaways from our study. First, we find evidence of both direct and knock-on effects of publicly reporting tax delinquents. Our estimates of the direct effects are positive; that is, taxpayers are more likely to comply when they are warned that they will be disclosed as delinquents if they do not pay. Our estimates of the knock-on effects are – perhaps unsurprisingly – negative; that is, taxpayers are less likely to comply when they receive reports of delinquents. This demonstrates an important unintended consequence of public disclosure policies: that the dissemination of tax behavior may lower morale more generally.

Second, we find evidence of both direct and knock-on effects of publicly recognizing compliers. However, the effects differ substantially from those found when publicly reporting delinquents. Our estimates of the direct effects of recognizing compliers are negative; that is, taxpayers are less likely to comply when they are promised that they will be honored as compliers if they pay. Our estimates of the knock-on effects are also negative; that is, taxpayers are less likely to comply when they receive lists of compliant taxpayers. Together, these suggest that publicly recognizing tax compliance actually backfires.

Third, we use the structure of our model and our empirical estimates of the direct effects of reporting and recognition to disentangle the likely mechanisms. Our estimates of direct effects can be rationalized in the framework of our model only if there is both a privacy cost of being revealed as a property owner and no shame or pride in tax compliance behavior. In other words, the mechanism that explains the direct effects of public disclosure policy is that of incurring a privacy cost to the individuals upon becoming

revealed as a taxpayer, and that shame of delinquency status plays no a role. This provides an intuitive explanation for why the direct effects of recognizing compliers reduces compliance; that is, taxpayers on the margin act to avoid having their property ownership status publicly disclosed and do not value the social recognition of being believed compliant.

Fourth, we provide evidence on the mechanism through which public disclosure policies induce negative knock-on effects. In our model, the knock-on effects of public disclosure work through their influence on beliefs about the compliance rate. Estimates from our endline survey corroborate this specification. In particular, we find that dissemination of both tax compliers and tax delinquents *lowers* taxpayers' beliefs about compliance rates. These effects are consistent with the context in Kampala where compliance rates are very low and beliefs are substantially optimistic, and the idea that dissemination of tax behavior causes taxpayers to update their beliefs toward the truth.[7] Using an IV approach, we find that raising beliefs about the compliance rate of the wealthy by 1 percentage point increases the propensity to comply by about 0.4 percentage points. So while taxpayers are positively influenced by higher compliance rates of others, the effectiveness of public dissemination relies on actual compliance exceeding expectations.

Fifth, we find that enforcement messages (reminders of the different legal penalty measures that the government can take against delinquents) are a relatively effective means of raising compliance, while messages appealing to reciprocity (reminders of the services their tax payments contribute to), and relationship management (reminders of the contact number to call for any questions or concerns) have no statistically distinguishable effects.

These results together provide a policy-relevant basis for understanding the effects of public disclosure on tax payments in low-capacity, low-compliance settings. It is clear that the estimated net (direct plus knock-on) effect of public recognition is negative. At the same time, while the direct effect of public disclosure is positive, the negative knock-on effects are of similar magnitude, thereby substantially reducing the net effect to close to zero. We find that a simple reminder about the enforcement capacity of the government raises compliance by at least as much as the direct effect of threatening to disclose delinquency without even taking into consideration the negative knock-on consequences. Therefore, from a policy perspective, text messages that send information reminders are at least as effective, and do not suffer from the negative knock-on effects of public disclosure.

We make five main contributions to the literature. First, we provide evidence on tax compliance effects of public disclosure policies in low-income countries, an understudied

[7] Alternatively, we find no statistically detectable effect on beliefs about state capacity, that the state acts in the interest of its citizens, that tax behavior should be kept private, that not paying taxes is wrong and punishable, that tax behaviour will be published in the future, or the desire to have own compliance known.

and yet highly relevant context. One important early paper in this literature is Slemrod et al. (2022), who study the disclosure of all income tax payments in Pakistan by the national government. They leverage a novel research design which compares taxpayers with more or less common names, before and after the disclosure, and find that uncommonly named taxpayers are more responsive to disclosure than their commonly named peers. Our paper provides evidence on shaming and recognition policies in a context with substantially lower administrative capacity which is relevant for many local governments, especially in sub-Saharan Africa, struggling to raise compliance and potentially facing context-specific social norms around taxation. Further, using a controlled field experiment we are able to explore different mechanisms and unintended effects compared to Slemrod et al. (2022) as outlined next.

Second, we compare the effects of publicly reporting tax delinquents with the effects of publicly recognizing tax compliers through a controlled field experiment. Previous work has studied 'shaming' policies (Perez-Truglia & Troiano 2018, Dwenger & Treber 2022, Angaretis et al. 2022) or 'honouring' policies (Slemrod et al. 2022, Dwenger et al. 2016) in isolation, but to the best of our knowledge ours is the first study to compare the two directly. Importantly, our model demonstrates at least one straightforward case in which the effects of the two policies may diverge (i.e., when there are privacy costs or gains to public wealth signalling).

Third, we separately estimate the direct effects of public disclosure and the knock-on effects of publicly disseminating tax behavior. This extends previous work that either looks at direct effects alone (Angaretis et al. 2022, Dwenger & Treber 2022) or estimates a combination of the two effects (Bø et al. 2015, Hasegawa et al. 2013). While there have been some efforts to separately estimate knock-on effects, these have largely been through lab experiments (Blaufus et al. 2017, Wenzel 2005), quasi-experimental variation (Slemrod et al. 2022), or using a narrow and selected sample of information recipients (Perez-Truglia & Troiano 2018).[8] Our study is the first to use experimental variation to separately identify these knock-on effects for a large population of information recipients.

Fourth, we contribute to a literature on tax morale determinants of tax compliance (Luttmer & Singhal 2014). We provide evidence that raising beliefs about compliance of wealthy taxpayers positively affects the decision to comply. While this was a central question in Del Carpio (2022), the results were statistically indistinguishable from standard reminders. On the other hand, our model and results on direct effects suggest that the expected social sanctions to delinquency (or social rewards to compliance) are limited. We know of no comparable study that isolates this dimension of tax morale. By bench-

[8]Specifically, Slemrod et al. (2022) compare their control group (common named self-employed reporters) to a group of wage earners (reported by a third-party) and find a positive effect of public disclosure on control group compliance. Perez-Truglia & Troiano (2018) look at knock-on effects to a set of delinquents, which is a narrow and highly selected sample in the USA. We experimentally study the effects of disseminating tax behavior to tax-eligible properties generally.

marking our tax morale treatments against standard enforcement reminders, we add to a growing literature that finds a positive effect of enforcement reminders (Brockmeyer et al. 2023, Cohen 2020, De Neve et al. 2021, Dwenger et al. 2016, Castro & Scartascini 2015), though it is important to note that Mascagni & Nell (2022) find insignificant effects of similar 'deterrence' messages in Rwanda.

Fifth, we contribute to a broad literature on social image and economic behavior (Bursztyn & Jensen 2017) and the psychological factors affected by information disclosure (Loewenstein et al. 2014). We contribute to this literature by, again, studying a low-income country where baseline social norms are likely to differ from those in the developed world. Through our multi-armed field experiment and the lens of our model, we shed light on the relative importance of normative expectations (around property tax compliance) vs. status signalling (of property tax ownership).[9]

This paper is also related to a growing literature on property taxes in low-income countries (Del Carpio 2022, Knebelmann 2019, Best et al. 2019, Okunogbe 2019, Bergeron et al. 2019, Balan et al. 2022, Weigel 2020, Kapon et al. 2022, Brockmeyer et al. 2023). We are the first in this literature to study the effects of public disclosure policies on raising property tax compliance.

The remainder of this paper is organized as follows. In Section 1, we provide details of the data used for this project. In Section 2 we present background evidence on the property tax, compliance, and enforcement capacity in Kampala that informs our study. In Section 3 we outline key mechanisms through which public disclosure policies may affect compliance, and develop a model of tax evasion to help interpret our findings. In Section 4, we detail our experimental design, estimation strategy, and main outcomes of interest. Our empirical results and the insights they provide through our conceptual model, as well as our policy implications, are presented in Section 5. Section 6 concludes.

1 Data

1.1 Administrative data

The main source of data used in this paper is administrative data from the Kampala Capital City Authority (KCCA). We combine data extracts from the property owner registry, annual property bills, and property rate payment histories from the KCCA eCitie tax database. These data are used to create our primary outcomes of interest, baseline covariates, and general sampling frame.

The registry includes all property owners in Kampala and each is identified with an

[9]This paper is also related to a series of papers studying the social image effects on non-tax behavior, such as bureaucrat performance (Gauri et al. 2018), education investment (Bursztyn et al. 2019), and workplace safety (Johnson 2020).

ID called `COIN`. The registry records contact information for the property owner: first and last name, or name of legal entity (when relevant), phone number, and village of residence. The bills are created every year on July 1st for each taxable property identified with an ID called `propertyno`. The bills record the rateable value of the property, the annual liability (which is 6% of the rateable value) and the outstanding balance (debts+penalties-waivers-payments). The payment histories record the amount and date of each payment made towards a property. Both bills and and payments are extracted for the entire study period starting from the 2019/2020 financial year. We match owners from the registry to bills by `COIN`, and then bills to payments by `propertyno`. Further, each property has an ID called `CAMVID` that links to characteristics of the property collected for the Computer-Assisted Mass Valuation (CAMV) programme. These characteristics include the property's location (parish, village, and GPS coordinates), type (residential, commercial, institutional, etc.) and over 300 other features.

1.2 Survey data

1.2.1 Baseline Survey

In November and December 2020 we conducted a baseline survey with 1,172 property owners that were assigned to our control or public disclosure treatments (about 2% of the full sample) to collect information on their baseline characteristics, behaviors, and beliefs.[10] [11]This data is used to run descriptive statistics of the property tax context in Kampala (Section 2).

1.2.2 Endline Survey

In January and February 2022 we conducted an endline survey with 4,960 property owners (about 5% of the full sample) after both waves of treatments had been completed. This allowed collection of information on more proximate outcomes than are available in the administrative data.

To create a representative sample, we randomly sampled taxpayers from the KCCA

[10]To create a representative sample, we randomly sampled taxpayers from the KCCA registry. The randomization was stratified by total property value, past compliance, and year 1 treatment. We completed 1,172 surveys after having approached a total of 2,076 taxpayers.

[11]In order to understand how our baseline survey sample compares to the general population, Table A7 reports coefficients from simple regressions of characteristics on a dummy equal to one if a targeted respondent completed their survey, and equal to zero if they could not be reached, were the incorrect number, or refused to complete the questionnaire. From here we can see that, relative to attriters, our respondents are less likely to own commercial properties, and their neighbours (village or parish) have lower baseline compliance. There are no statistically significant differences in whether the property itself was compliant at baseline, the property liability, distance from the city centre, total liability of the owner's properties, whether the owner is a legal entity, or the village population density. Therefore, it is important to note that the descriptive baseline survey analysis is valid for a sub-sample of properties that are non-commercial, and are located in areas with relatively low compliance rates.

registry. The randomization was stratified by total property value, past compliance (a dummy if the property paid at least their annual liability in the 2019/2020 financial year), and year 1 and year 2 treatment groups.

We completed 4,960 surveys after having called a total of 8,525 taxpayers. Of the 3,853 that we were unable to survey, 63% never answered phone, 22% claimed not to be the correct person from the KCCA database, and 13% refused to be surveyed.

In order to understand how our survey sample compares to the general population, Table A8 reports coefficients from simple regressions of baseline characteristics on a dummy equal to one if a targeted respondent completed their survey, and equal to zero if they could not be reached, were the incorrect number, or refused to complete the questionnaire. From here we can see that, relative to attriters, our respondents have smaller liabilities, live in larger villages, and their neighbours (village or parish) have lower baseline compliance. There are no statistically significant differences in whether the property itself was compliant at baseline, the property type (commercial or other), distance from the city centre, total liability of the owner's properties, whether the owner is a legal entity, or the village population density. Therefore, in the survey analysis below it is important to note that our analysis is valid for a sub-sample of properties that are relatively low value, and are located in areas with relatively low compliance rates.

2 Property tax and the Kampala context

Property taxes have a variety of attractive features. Property, being immovable and easily observable, makes it's taxation more difficult to evade compared to income, sales, and other common tax bases. Further, there is an argument for fairness in taxing property since it, in part, captures rising land values which are driven by public investment and agglomeration externalities, and not by the efforts of the landlord. Taxes on the value of land and property can offer a significant source of funding to provide local services for city governments faced with limited municipal revenues and rapidly growing populations. These revenues can unlock a virtuous cycle of public investment and rising property values.

Property taxes play an important role in revenue collection in Kampala, contributing 47% of the Kampala Capital City Authority's (KCCA) own-source revenues in 2021/22. As shown in Figure 1, property tax revenues far exceed revenues from all other collection instruments at the KCCA's disposal. The property tax in Kampala is called 'Property Rates'. It taxes a share (6%) of a property's 'Rateable Value', which is a professional assessor's estimate of the typical rental income for such a property, minus a fixed proportion to account for vacancies.[12] Property Rates are applied to rented residential, commercial

[12]This tax is distinct from, and in addition to, rental income taxes applied by the Uganda Revenue Authority which levy a rate on taxpayer's reported annual income from rental properties.

and institutional properties across the five urban divisions of the city.[13] The property tax roll had been limited to relatively small set of properties since it was last updated in 2006. However, from 2016 to 2019, the KCCA successfully expanded the property tax net through mass enumeration, addressing and valuation of properties in the city. This process involved collecting ownership information, location details, GIS coordinates, and property attributes for over 300,000 properties in the city.

2.1 A compliance challenge

Despite its importance for KCCA own-source revenues, compliance rates with the property tax are very low. For each of the financial years 2019/20, 2020/21, and 2021/22 only around 10-11% of properties paid their annual liability.[14] While low, Kampala is not an outlier for cities in low-income countries. Figure 2 plots compliance rates for property taxes in a select set of cities around the world where studies are available. Kampala's compliance rate is similar to cities in other African countries, and while cities in South America tend to fare somewhat better, their compliance still sits around the 50% mark.

Since compliance is positively correlated with liability, the revenue collection figures look more encouraging. Over the years 2019-2022, the KCCA has collected 39% of the potential revenue from property taxes. In Figure A1, we plot actual and potential revenue collection by ventiles of property value. Here it is apparent that the highest value properties contribute the largest share of revenue. However, it is also worth noting that most uncollected potential revenue also comes from the highest value properties. Therefore, the largest potential revenue gains will continue to come from policies that can induce high value properties to comply.

2.2 Public disclosure as a 'social' enforcement alternative

While there are many different factors that may affect a taxpayers decision to pay taxes, the literature is in agreement that enforcement capacity is the dominant determinant of compliance (Luttmer & Singhal 2014). Cities in low-income countries like Kampala are often severely limited in their enforcement capacity, especially in the face of equilibrium compliance levels that require enforcing payments from a vast majority of taxpayers.

There are a number of enforcement measures that the KCCA can take once property tax payments are not received by the official deadline. First, the city has legal authority to take tax delinquents to court to recover the amount by warrant. Second, taxes can be demanded from tenants, allowing them to deduct those payments from rent. Third,

[13] There are some rare exceptional cases where property rates are not based on rental value, but rather business income. These special cases occur when the property owner runs a business from their property. The most common cases these are high end hotels, hospitals, and gas stations.

[14] Unless otherwise noted, statistics in this section come from authors calculations using the administrative and survey data outlined in Section 1.

properties in arrears can be locked up to prevent use until payments are made.[15] Finally, interest payments are charged to properties in arrears.

According to our baseline survey, less than half of taxpayers find the threat of fines, court action or lock ups to be "very important" in making tax payment decisions. This is in large part because taxpayers do not see these as credible threats. Of those who did not find these measures to be important, 49-62% either believed the KCCA is unlikely to take these actions, or that they do not have the legal authority to do so.

In contexts of low compliance and limited enforcement capacity, many cities, including Kampala, are exploring the potential for public disclosure as a low cost means to raise compliance.

3 Conceptual Framework

This section first describes the key mechanisms through which public disclosure policies affect tax compliance, drawing on the literature and examples from the Kampala context. Second, we introduce a model of tax compliance to help interpret our empirical findings.

3.1 Public Disclosure mechanisms

There are (at least) three possible mechanisms through which public disclosure can affect compliance: the shame of delinquency (or pride in compliance), the desire for privacy of ownership status, and the role of tax information dissemination in tax morale. Here we outline these in detail.

3.1.1 Delinquency status and shame

Perhaps the main mechanism through which public disclosure policies are thought to operate is the shame of delinquency, or the pride in compliance. When their tax delinquency is made public, property owners may feel that this heightens the expected social sanctions they will face from members of the public. Relatedly, when tax compliance is made public, property owners may feel that this heightens the expected social gains they receive from members of the public.

Shame and pride mechanisms have been shown to be important in raising compliance in high income countries (Perez-Truglia & Troiano 2018, Angaretis et al. 2022, Bø et al. 2015), with some growing evidence in low-income countries (Slemrod et al. 2022).

However, it is possible that norms are entirely different in certain contexts - particularly where compliance rates are very low and public service provision limited as they are in many African cities. In theory, it could even be that net social sanctions are actually

[15] Property lock-ups to enforce tax payments are typically reserved for commercial properties, and not used on residential rentals.

imposed on compliers rather than delinquents. These type of 'anti-compliance' norms may exist if for instance, people are ashamed to be seen as one of the few 'suckers' who pay their taxes in a population of delinquents.

At baseline we asked our respondents questions relating to social norms around compliance and delinquency. While less than 5% of respondents considered that not paying property rates is 'not wrong at all', the vast majority think that it is 'wrong but understandable' (Figure A2). This suggests that norms around tax compliance in the Kampala context may be relatively weak. When asked explicitly about social gains to compliance, the modal response is that taxpayers are 'very unlikely' to gain respect if they are known to pay, and the majority believe that it is 'very unlikely' for a known delinquent to face social costs (Figure 3). So while taxpayers believe that it is slightly more likely for compliers to gain social respect (blue bars) than delinquents to face social costs (red bars), overall social payoffs for either type of behavior are believed to be limited.

If norms around compliance and delinquency tend to be weak, then the 'shame' mechanism in the effects of public disclosure policies may be muted.

3.1.2 Property ownership status and privacy

At the same time, by disclosing property tax compliance or non-compliance, both public reporting and recognition policies also disclose the ownership status of taxpayers.

This may be a good or bad thing - as a signal of wealth and tenure, being known as a property owner may raise a taxpayer's perceived social status. Public knowledge may itself enhance security of ownership in a context where, according to our baseline survey, 45% of taxpayers do not have documented proof of property ownership such as land titles, property transfer certificates, tax receipts, rental contracts or utility bills.

However, it could also result in additional "taxation" in the form of pecuniary or in-kind demands from family or other social connections (Jakiela & Ozier 2015). This may be a pertinent issue where social networks are strong - over half of the respondents in our baseline survey noted that they typically interact with over 20 people outside of their household each day. More directly, property owners may want privacy from formal tax authorities. In contexts like Kampala, where property is subject to different local and national taxes, either reporting or recognition may reveal tax-eligible properties to other authorities. Finally, property owners may want to maintain privacy of their ownership status due to safety and security concerns.

3.1.3 Compliance beliefs and morale

The primary aim of public disclosure policies is to incentivise taxpayers to comply by threatening to publish their delinquency or publicly recognize their compliance. However, the publication of this behavior has the potential to induce a secondary "knock on" effect

by causing the recipients of this information to change their beliefs about compliance in their city. If taxpayers are motivated to pay when they know that others pay too, then public disclosure policies may also affect the compliance of recipients.

These knock-on effects may come about as the result of taxpayers being influenced by the behavior of particular groups of taxpayers - celebrities or politicians, for example - or simply by general compliance rates in the city. Both of these appear to be true in Kampala - 78% of taxpayers in our baseline survey completely or somewhat agreed that they would be more likely to pay their taxes if they knew that the majority of properties in Kampala were paying their share. Additionally, 70% completely or somewhat agreed that they would be more likely to pay their taxes if they knew that wealthier and more influential individuals were paying their taxes.

There are two broad ways in which public disclosure can affect recipient behavior: by providing additional information on compliance to taxpayers, or by putting a positive or negative "framing" to others' behavior that encourages or discourages compliance.

A key insight of Del Carpio (2022) is that the expected effect of disseminating true information about the compliance rate will depend on baseline beliefs of the compliance rate. If taxpayers already know the compliance rate, providing information on this is unlikely to affect their behavior. If they are pessimistic, information about the compliance rate may improve their tax morale, whilst if they are overly optimistic, information may actually demotivate them from paying their taxes. In Kampala, perceived compliance at baseline is low, but still substantially overestimates the actual compliance rate. Figure 4 plots histograms of beliefs about compliance from our baseline survey. The blue bars plot the share of respondents by their beliefs of the city-wide compliance rate. The average belief for city-wide property tax compliance is just under 50% compliance, much higher than the true 10% rate. Less than 10% of our respondents reported beliefs that either 0 or 1 out of 10 properties are compliant. The red bars plot the share of respondents by their beliefs of the city-wide compliance rate for wealthy (top 5% by property value) taxpayers. Respondents correctly believe that compliance is higher for wealthy taxpayers, but overall remain substantially optimistic relative to the true 22% rate. Since prior beliefs are highly optimistic and taxpayers report that they are positively influenced by the compliance of others, we may expect public disclosure policy to induce negative 'knock-on' effects. If taxpayers receive signals about the true compliance rate, they will update their beliefs downwards, and become demotivated to comply.

3.2 A parsimonious model of public disclosure

Drawing on the above, we develop a version of the Allingham & Sandmo (1972) model for tax compliance.[16] We tailor the model to help shed light on the theoretical determinants of our public disclosure effects.

An agent with income y faces a tax liability of t. They can decide whether to evade the tax or not $e \in \{0, 1\}$. In terms of payoffs, there is a material motive to pay m which can be interpreted as the expected penalties imposed by the state, and an intrinsic motive $i + v$ where the idiosyncratic component v has CDF $F(v)$ and $E[v] = 0$, which can be interpreted as the individual's preference for compliance.

Our analytical focus is on two further social dimensions of the motivation to pay. First, we examine a pair of payoffs related to expected social reactions. An agent faces a potential social reaction to property ownership x, which is incurred when the agent is believed by the public to be a property owner which happens with probability $p(e)$. An agent also faces a potential social reaction to property tax delinquency z, which is incurred when the agent is believed by the public to be a property tax delinquent (rather than a complier) which happens with probability $p(e)q(e)$ where $q(e)$ is the conditional probability that the agent is believed to be delinquent given that they are believed to be an owner.[17][18]

Second, the motivation to pay can depend on the agent's beliefs about the public in a few ways. The expected monetary penalties of evasion, average intrinsic motivation to pay and the expected social reactions to delinquency depend on the agent's belief of the population evasion rate $\hat{\lambda}$ (i.e. $m \equiv m(\hat{\lambda})$, $i \equiv i(\hat{\lambda})$, and $z \equiv z(\hat{\lambda})$).

All together, we express the utility of an agent as a linear function of these payoffs and their decision to evade

$$\begin{aligned} U(e) = (1-e)(y - t + v + i(\hat{\lambda}) - p(e)[x + q(e)z(\hat{\lambda})]) \\ + e(y - m(\hat{\lambda}) - p(e)[x + q(e)z(\hat{\lambda})]) \end{aligned} \tag{1}$$

To simplify notation we define $p(e) \equiv p_e$ and $q(e) \equiv q_e$. The agent's problem is then to maximise their payoffs by choosing to evade or comply. They will evade if: $v < t - m(\hat{\lambda}) - i(\hat{\lambda}) - [p_1 - p_0]x - [p_1 q_1 - p_0 q_0]z(\hat{\lambda})$ and otherwise they will choose to comply. Therefore, the share of evaders will be:

$$\lambda = F(t - m(\hat{\lambda}) - i(\hat{\lambda}) - [p_1 - p_0]x - [p_1 q_1 - p_0 q_0]z(\hat{\lambda})) \tag{2}$$

[16] Allingham & Sandmo (1972) is a seminal paper, and we also draw on many of the papers that followed to develop our model (Gordon 1987, Slemrod 2019, Besley et al. 2022, Benabou & Tirole 2011).

[17] For example, an agent may bare a shame cost ($z > 0$) if the public imposes pro-compliance social sanctions.

[18] While it is intuitive to think of privacy and shame costs, we do not impose signs on either x or z and leave these as empirical questions.

We return to this conceptual framework in Sections 5.1.1 and 5.2.1, to help us interpret empirical findings from our experiment. In particular, we will analyse the share of evaders under different public disclosure states denoted $\sigma \in \{C, S, H\}$, where C denotes 'control' or no public disclosure, S denotes public reporting, and H public recognition. These states represent our empirical treatments outlined in Section 4.

4 Experimental design and estimation strategy

In collaboration with the Kampala Capital City Authority, we designed an experiment to test for effects of public disclosure policies on tax compliance. In doing so, we have two broad aims. First, to separately identify the effects of publicly reporting delinquents from the effects of publicly recognizing compliers. To do so, we randomly assign property owners to sub-treatments that are either related to the positively framed disclosure of compliance, or the negatively framed disclosure of non-compliance. Second, to separately identify the effect of public disclosure on those warned their behavior will be made public (direct effects) from the effect of publicly disclosing this information on other's behavior (knock-on effects). To do so, we stagger our experiment across two waves and cross-randomize our treatments so that the direct effects are tested in the first wave (May-June 2021), and knock-on effects are tested in the second wave (Nov-Dec 2021). The full timeline of the study is given in Table A1.

For both phases of the experiment we used a sample of roughly 70,000 unique phone numbers associated with roughly 174,000 tax-owing properties. In both waves, treatments were administered via SMS message periodically over one and a half months.[19] We measure outcomes at the property level, so our observational unit is the property. Because multiple properties can be held under the same phone number, randomization was clustered at the phone number level so that all properties with the same phone number receive the same treatment.[20]

4.1 Direct Effects: randomising notice of public disclosure

The first intervention focuses on the direct effects of public disclosure by randomly varying whether individuals are notified they will be publicly reported for delinquency, publicly recognized for compliance, or neither. Individual property owners are assigned to one of three broad groups: Control, Public disclosure, and Benchmark nudges. The Public disclosure group is split into Reporting and Recognition, which are each split further by

[19] Messages were only sent to those taxpayers with some outstanding balance as of the previous working day. We also discarded a few properties that had recorded official objections to their valuations.

[20] We use machine randomization and treatments are administered digitally by staff at the KCCA Directorate of Revenue Collection. After randomization, we generated lists of properties for each treatment group. These were shared with staff at the KCCA, who then uploaded the lists to a mass text messaging system that sends standardized messages to each phone number according the the list it is on.

the mode of disclosure (SMS or an online list). The Benchmark nudge group is split into three subgroups; Enforcement, Reciprocity, and Relationship management, where messages provide information on enforcement, public services, and government contacts respectively. A diagram of these treatments and sub-treatments is given in Figure 5.

Six weeks in advance of the payment deadline, an initial standard message is sent out for each property (so multi-property owners receive multiple standard messages) in both English and Luganda. This message is identical regardless of the treatment group and notifies the recipient of the amount due on the property as well as the deadline for payments. The exact wording and translations of the standard message, and all other messages in the first wave, can be found in Table A2.

Experimental variation is introduced through a set of follow up messages. Following the standard message, each property receives a text message with content that depends on the group they were assigned to. The exact message content sent to each group is outlined in Table A2, and here we summarize this content. The Control group is simply sent a further reminder to pay their tax by the deadline. The Reporting group is notified that the KCCA will publicly report them as a defaulter if they do not pay their taxes on time.[21] On the other hand, the Recognition group is notified that the KCCA will publicly recognize their contribution if they pay their taxes on time.[22] Finally, the Benchmark Nudge groups are sent different types of information messages that attempt to nudge compliance: on enforcement measures the city can implement, on public services property rates contribute to, and on details of client relationship managers that taxpayers can contact with any issues.[23]

4.1.1 Direct Effects: Estimating equation

Our main empirical specification for estimating direct effects compares the reporting and recognition groups to the control group. Specifically, we estimate the equation below:

$$\begin{aligned} y_i = &\tau_S \text{ reporting}_{o(i)} + \tau_H \text{ recognition}_{o(i)} \\ &+ \gamma_1 \text{ enforcement}_{o(i)} + \gamma_2 \text{ reciprocity}_{o(i)} + \gamma_3 \text{ relationship management}_{o(i)} \\ &+ \eta_{s(o)} + \epsilon_i \end{aligned} \tag{3}$$

where y_i is one of the outcomes outlined in subsection 4.3 for property i, $\text{reporting}_{o(i)}$ is a dummy if owner $o(i)$ of property i was assigned to the reporting disclosure group

[21] This is further split by two modes: the Reporting-SMS group is told that delinquents will have their name and parish shared by SMS text message with other citizens and neighbours, and the Reporting-Web group is told that delinquents will have their name and parish shared via an online list.

[22] This is also split by two modes: the Recognition-SMS group is told that compliers will have their name and parish shared by SMS text message with other citizens and neighbours, and the Recognition-Web group is told that compliers will have their name and parish shared via an online list.

[23] The role of these treatment groups is to benchmark any public disclosure effects

in the first wave, recognition$_{o(i)}$ for assignment to the recognition group, enforcement$_{o(i)}$ for assignment to the benchmark enforcement group, reciprocity$_{o(i)}$ for assignment to the benchmark reciprocity group, and relationship management$_{o(i)}$ for assignment to the benchmark relationship group. Finally, $\eta_{s(o)}$ are fixed effects for each strata s that owner o falls into, and ϵ_i is an idiosyncratic error term for property i.[24] We always allow for the error term to be correlated within property owner, i.e. we always report standard errors clustering at the owner level.

Our main parameters of interest are τ_S and τ_H, i.e. the effects of reporting and recognition respectively - but we are alsointerested in γ_1, γ_2, and γ_3 to benchmark τ_S and τ_H against effects of standard nudges.[25] We also consider specifications that estimate heterogeneous effects and effects for subgroups.

4.1.2 Direct Effects: Balance tests

Here we run balance tests for effects of treatments in the first wave on baseline property characteristics, in order to check that the randomisation was balanced. The first wave treatment balance tests are reported in Table A5. Almost all coefficients are insignificant for all baseline outcomes we report. Inevitably, some characteristics are slightly unbalanced. In particular, properties with use type 'other' are marginally less likely to appear in our recognition treatment group, and village population is higher in the recognition group. For our main results, we always report robustness checks in the appendix where we control for all of these baseline characteristics. Our results are not sensitive to these controls.

4.2 Knock-on Effects: randomising public dissemination

The second intervention focuses on knock-on effects by randomly varying the type of information shared with taxpayers. This intervention assigns individual property owners to one of two broad groups: Control or Public dissemination. The Public dissemination group is further split into three: a Delinquents List group, a Compliers List group, and a Wealthy Compliers List group. The Delinquents List group is informed about tax delinquents from the previous year, whilst the Compliers List group is informed about compliers from the previous year. Both of these groups are further split into three subgroups which receive different list compositions (sampled from their neighbours, or from the city as a whole) and different modes of dissemination (lists provided directly by

[24] We employ a block-randomized design, stratifying the first wave treatments on ventiles of total property value at the phone number level and a dummy for whether tax was paid at baseline (2019/2020) for at least one of the properties associated with the phone number. Phone numbers within each of the 40 strata are randomly assigned to one of the first wave treatment or control groups.

[25] The control group is always the omitted category, so effects always represent the average effect relative to the control group for the relevant treatment.

SMS, or through a link to a webpage).[26] The Wealthy Compliers List group is specifically informed about compliers from the previous year who have paid at least 2mn UGX (which represents the top 5% city wide property wealth.)[27] A diagram of these treatments and sub-treatments is given in Figure 6.

As in the first wave, six weeks in advance of the (second wave) payment deadline, an initial standard message is sent out for each property (so multi-property owners receive multiple standard messages) in both English and Luganda. This message is identical regardless of the treatment group and notifies the recipient of the amount due on the property as well as the deadline for payments. The exact wording and translations of the standard message, and all other messages in the second wave, can be found in Table A3.

Experimental variation is introduced through a set of follow up messages. Following the standard message, each property receives a text message with content that depends on the group they were assigned to. The exact message content sent to each group is outlined in Table A3, and here we summarize this content. The Control group is simply sent a reminder to pay their tax by the deadline. The Delinquents List group is sent a message explaining that owners who did not pay in the previous year are being reported, and then given a list of taxpayers who did not pay their rates (delinquents) in the previous financial year along with their parish. On the other hand, the Compliers List group is sent a message explaining that owners who did pay in the previous year are being publicly recognized for their contribution, and then given a list of taxpayers who paid their rates (compliers) in the previous financial year along with their parish. Importantly, in all our dissemination treatments, we only report or recognize individuals who were assigned to the relevant treatment group in the first wave, and have therefore been warned about this public disclosure. For example, lists of tax compliers are selected from the set of individuals who were assigned to the public recognition treatment group and who paid their annual liability in the first wave.

4.2.1 Knock-on Effects: Estimating equation

Our main empirical specification for estimating knock-on effects compares the Delinquents List and Compliers List groups to the Control group. Specifically, we estimate the equation below:

$$y_i = \kappa_S \text{ delinquents}_{o(i)} + \kappa_H \text{ compliers}_{o(i)} + \eta_{s(o)} + \epsilon_i \tag{4}$$

where y_i is one of the outcomes outlined in subsection 4.3 for property i, $\text{delinquents}_{o(i)}$ is a dummy if owner $o(i)$ of property i was assigned to the Delinquents List group in

[26] Screenshots of the webpage used for the online treatments are given in Figure A4.

[27] We were unable to include the wealthy subgroup under the reporting treatment since the KCCA was concerned about a potential backlash of reporting specifically wealthy tax delinquents. We separate it out in order to keep the Delinquents and Compliers List treatment groups compositionally comparable.

the second wave, and recognition$_{o(i)}$ for assignment to the Compliers List group. Finally, $\eta_{s(o)}$ are fixed effects for each strata s that owner o falls into, and ϵ_i is an idiosyncratic error term for property i.[28] We always allow for the error term to be correlated within property owner, i.e. we always report standard errors clustering at the owner level.

Our main parameters of interest are κ_S and κ_H, i.e. the effects of receiving lists of delinquents and compliers respectively.[29] We also consider specifications that estimate heterogeneous effects and effects for subgroups.

4.2.2 Knock-on Effects: Balance tests

Here we run balance tests for effects of treatments in the second waves on baseline property characteristics, in order to check that the randomisation was balanced. The second wave treatment balance tests are reported in Table A6. Almost all coefficients are insignificant for all baseline outcomes we report. Inevitably, some characteristics are slightly unbalanced. In particular, properties with use type 'other' are marginally less likely to appear in our complier list and wealthy compliers treatment groups. For our main results, we always report robustness checks in the appendix where we control for all of these baseline characteristics. Our results are not sensitive to these controls.

4.3 Outcome measures

4.3.1 Outcomes from administrative data

Our main outcome in both waves is tax compliance, and we measure it in three different ways: (1) an indicator if total payments made in the treatment period at least covered the annual liability of the property, (2) an indicator if any payment was made towards the property during the treatment period, and (3) total payment amount made towards the property in the treatment period.[30] Because the distribution of liabilities is very skewed, we trim the top 1% of properties by annual liability when we look at total payment amount (outcome 3).

[28] We employ a block-randomized design, stratifying second wave treatments on ventiles of total property value at the phone number level, a dummy for whether tax was paid at baseline (2019/2020) for at least one of the properties associated with the phone number, and each of the 8 groups from the first wave. Phone numbers within each of the 320 strata are randomly assigned to one of the second wave treatment or control groups.

[29] The control group is always the omitted category, so effects always represent the average effect relative to the control group for the relevant treatment.

[30] Outcome (1) was pre-specified as our primary outcome of interest. Outcome (3) was pre-specified as the secondary outcome of interest. Outcome (2) was not pre-specified, but we include it here since properties may have made partial payments earlier in the same financial year, and so have less to pay than their annual liability in the treatment period.

4.3.2 Outcomes from survey data

In addition to the outcomes mentioned above, we use our endline survey to measure more proximate outcomes for a narrower sample of properties. Here we focus on five taxpayer sentiments: (1) belief about the compliance rate of properties in Kampala as a whole, (2) belief about the compliance rate of the most expensive properties in Kampala, (3) belief that the KCCA can detect who pays tax, (4) belief that the KCCA acts in the best interest of its citizens, and (5) belief that information about tax behavior is better kept private.[31] The full set of outcome variables that we used are described in Table A4.

5 Results

5.1 Direct Effects: estimates of public disclosure

In the first wave, we test for effects on tax compliance when it is known that tax behavior will be publicly disclosed. Our main results are reported in Table 1, showing estimates of effects on tax compliance for our three administrative data outcomes, following equation 3.[32]

There are three main takeaways from the results. First, the direct effect of reporting delinquency raises compliance. In column 1, we see that properties whose owners are warned that their tax delinquency will be publicly reported if they do not pay their dues are 0.58 percentage points more likely to have their liability paid for. While small in magnitude, this is a substantial increase of 17% on the control group mean of 3.4 percentage points.[33] The percentage change on the control mean is slightly higher (19%) if we measure tax compliance as any payment being made (col. 2). In column 3, we see qualitatively similar results for the total amount of payments made, though the estimate is noisy and we cannot reject that it is zero at conventional significance levels.

The second takeaway is that the direct effect of promising compliers recognition actually lowers compliance. In column 1, we see that properties whose owners are warned that their tax compliance will be publicly recognized if they pay their dues are 0.55 percentage points less likely to have their liability paid for, a 16% decline on the control group mean. The percentage decline on the control mean is slightly larger (19%) if we measure tax compliance as any payment being made (col. 2). In column 3, we see qualitatively similar

[31] Outcomes (1) and (2) are given by the respondent as an integer between 0 and 10, and we convert these to percentage points. Outcomes (3) - (5) are given by the respondent as a yes or no (or don't know) response, and we convert these to an indicator for 'yes' and set 'don't know' responses to missing.

[32] Table A10 runs a robustness check of Table 1 including all of the baseline characteristics from Table A5 as controls. As expected, our main results are very similar to those in Table 1 without controls.

[33] Note that mean compliance of the control group is lower than the average reported in Section 2 above, because we restrict to properties who are carrying a balance at the start of our experimental intervention. That is, we exclude some properties which have fully cleared their balance before our experiment.

results for the total amount of payments made, though the estimate is noisy and we can only reject that it is zero at 10% significance levels.[34]

The third takeaway is that the recognition and reporting effects are opposite signed, but of similar magnitude, i.e. $\hat{\tau}_S + \hat{\tau}_H = 0$. The p-value for the test that these coefficients are equal is 0.94 (reported at the bottom of Table 1). This finding has important implications linking to the conceptual framework in the section that follows.[35]

5.1.1 Linking to conceptual framework: what do direct effects tell us?

Focusing here on the direct effects of public disclosure, the government's choice of policy (σ) will affect both the probability of being discovered as a property owner ($p(e)$), and the probability of being discovered as a property tax delinquent ($q(e)$). The counterfactual exercise we consider here holds beliefs about the evasion rate ($\hat{\lambda}$) constant.

To be explicit, we introduce θ^* as the probability that an owner is discovered through a public disclosure program, i.e. the probability that a delinquent is discovered through a public reporting policy, or that a complier is discovered through a public recognition policy. We can think of "discovery" as discovery by one's relevant peer group - e.g. everyone who is delinquent will be reported through the public disclosure programme, which makes it more likely but not necessarily certain that they will be discovered as such by their relevant peers.

We define at baseline (i.e. in our control group) the probability of being believed to be a property owner $p_e \equiv \underline{p}$, and the conditional probability of being believed to be a tax delinquent as $q_e \equiv \underline{q}_e$ for evasion choice e.

Under the reporting state, an agent that chooses to evade will be discovered through the reporting program with probability θ^*. If they are discovered through the program, they are believed with certainty to be a delinquent owner, and otherwise they take the baseline probabilities for an evader (i.e. $p_1 = \theta^* + (1-\theta^*)\underline{p}$ and $p_1 q_1 = \theta^* + (1-\theta^*)\underline{pq_1}$). An agent that chooses to comply in the reporting state faces the baseline probabilities of perceived status.

Under the recognition state, an agent that chooses to comply will be discovered through the recognition program with probability θ^*. If they are discovered through the program, they are believed with certainty to be a compliant owner, and otherwise they take the baseline probabilities for a complier (i.e. $p_0 = \theta^* + (1-\theta^*)\underline{p}$ and

[34] Interestingly, results from our endline survey suggest that the majority (59%) of taxpayers would want others to know if they had paid their rates, with only 36% stating that they would not want this known. However, it may be that responses to the above are more aspirational - 'if I were able to pay my rates, I would want others to know, but I can't'. This is aligned with the fact that 63% of taxpayers who were delinquent at baseline (2019/2020) would like it to be known if they had paid, while only 48% of compliers share that belief.

[35] We also look at sub-effects by mode (SMS vs online disclosure) and find no evidence that the direct effects of public disclosure (recognition or reporting) vary by whether disclosure will be online rather than SMS - see Table A11.

$p_0 q_0 = (1 - \theta^*)\underline{pq_0}$). An agent that chooses to evade in the recognition state faces the baseline probabilities of perceived status.

These social detection probabilities under baseline, reporting, and recognition states are outlined in Appendix Table A9. Plugging back in to equation 2 and simplifying notation with $\underline{M} \equiv t - m - i - z\underline{p}[\underline{q}_1 - \underline{q}_0]$, the share of evaders under each regime (λ_σ) will be:

$$\begin{aligned}
\lambda_C =& F(\underline{M}) \\
\lambda_S =& F(\underline{M} - \theta^*[x(1 - \underline{p}) + z(1 - \underline{pq}_1)]) \\
\lambda_H =& F(\underline{M} - \theta^*[-x(1 - \underline{p}) + z\underline{pq}_0])
\end{aligned}$$

We assume that public disclosure policies induce a small enough change in payoffs that their effect on compliance can be approximated by a first order Taylor series, i.e. $F(v) \approx F(\underline{M}) + (v - \underline{M})F'(\underline{M})$. Therefore, the direct effect of public reporting τ_S and public recognition τ_H on compliance (note that compliance is $1 - \lambda$) can be expressed as:

$$\tau_S \equiv \lambda_C - \lambda_S = \theta^*[x(1 - \underline{p}) + z(1 - \underline{pq}_1)]F'(\underline{M}) \tag{5}$$

$$\tau_H \equiv \lambda_C - \lambda_H = \theta^*[x(\underline{p} - 1) + z\underline{pq}_0]F'(\underline{M}) \tag{6}$$

Intuitively, effects will be zero if there is no chance of being discovered through the public disclosure scheme (i.e. $\theta^* = 0$), or that there is no variation in the idiosyncratic motivation to comply near the baseline state (i.e. $F'(\underline{M}) = 0$).[36] We can discard these theoretical cases based on our empirical results below which find that both reporting and recognition effects are significantly different from zero. Otherwise, if privacy and shame costs are positive, then public reporting will unambiguously raise compliance. However the direct effect of public recognition will be ambiguous and depend on whether the change in expected payoffs from public knowledge of compliance $z\underline{pq}_0 > 0$ outweighs the change in expected payoffs from public knowledge of property ownership $x(\underline{p} - 1) < 0$.

It is important to note that examining the effects of either policy alone confounds privacy costs (x) and the shame costs (z). In our simple framework, we can separate the two by taking the sum of effects:

$$\tau_S + \tau_H = \theta^*[z(1 + \underline{p}(\underline{q_0} - \underline{q_1}))]F'(\underline{M}) \tag{7}$$

where privacy costs notably drop out, and we can use the sum of effects to sign the social cost of non-compliance (z). Following equation 7, when $\tau_S + \tau_H > 0$ then $z > 0$, suggesting

[36] There are also some extreme edge cases. There will be no effect of reporting when property owner status is believed with certainty at baseline ($\underline{p} = 1$) and the conditional probability of being believed as a delinquent under evasion is also believed with certainty ($\underline{q}_1 = 1$). Likewise, there will be no effect of recognition when ($\underline{p} = 1$) and ($\underline{q}_0 = 0$).

that the public imposes 'pro-compliance' social costs. Conversely, when $\tau_S + \tau_H < 0$ then $z < 0$, suggesting that the public imposes 'anti-compliance' social costs. Finally, $\tau_S + \tau_H = 0$ then $z = 0$ suggesting that there is no social cost of compliance.[37][38]

Our empirical estimates are significantly different from zero for both recognition and reporting, implying that $\theta^* > 0$ and $F'(\underline{M}) > 0$ and $\underline{p} \neq 1$ or $\underline{q}_1 \neq 1$ and $\underline{q}_0 \neq 1$. Our results also suggest that reporting and recognition effects are of equal magnitude but opposite sign (i.e. $\tau_S + \tau_H = 0$). From equation 7, this gives $z(1 + \underline{p}[\underline{q}_0 - \underline{q}_1]) = 0$ which holds only for $z = 0$. Our empirical results, in combination with our conceptual model, suggest that there is no shame cost to property tax delinquency in Kampala.

Finally, with $z = 0$, and returning to equations 5 and 6, our empirical estimates ($\widehat{\tau}_S > 0$ and $\widehat{\tau}_H < 0$) imply that the privacy cost to being known as an owner of rental properties is positive $x > 0$.

To summarise, our parsimonious model,together with our empirical estimates, suggests that concerns about privacy drive the direct effects of public disclosure policies, and that shame of delinquency status does not play a role. Understanding this mechanism is important to understand behavior of taxpayers, and why public disclosure policies can induce changes in compliance in low compliance settings.

5.1.2 Comparing Direct Effects with Benchmarks

Returning to Table 1, in the lower half we report estimates of the benchmark effects. While there is no effect of messages that appeal to reciprocity (reminding taxpayers of the services their tax payments contribute to) and relationship management (reminding taxpayers of the contact number to call for any questions or concerns), we do find large effects of enforcement reminder messages. Properties whose owners were reminded of legal action that the government can take against delinquents are 1.29 percentage points more likely to have their liability paid for, a 38% increase on the control group mean. These effects are roughly twice the magnitude of the direct effects of reporting. These results are consistent with a growing literature in many contexts that find positive effects of enforcement messages. The positive effects of enforcement messages on compliance are consistent with our model if messages raise beliefs on the size of m, and therefore reduce

[37] Again we take as given $\theta^* > 0$, $F'(\underline{M}) > 0$, and either $\underline{p} \neq 1$ or $\underline{q}_1 \neq 0$ and $\underline{q}_0 \neq 1$. This is consistent with our our empirical findings of non-zero effects.

[38] The model outlined above expressed the social cost of being perceived as a delinquent as z and implicitly the (relative) social cost of being perceived as a complier as zero. However, it may be that the social costs are non-linear in perceived status (Butera et al. 2022). For example, the shame cost of being known as a delinquent with certainty may be different from the gain from being known as a complier with certainty. Therefore, in Appendix Section D we extend the framework to include the possibility that a tax owner can have an ambiguously perceived status, and allow the payoffs of being perceived with certainty as a complier relative to an ambiguous status, to differ from the payoffs of being perceived with certainty as a delinquent relative to an ambiguous status. Again, our empirical results ($\widehat{\tau}_S + \widehat{\tau}_H = 0$) are consistent with there being no shame costs under the extended model with potentially ambiguous status.

the share of evaders.

5.1.3 Direct Effects: heterogeneity

We explore heterogeneity in public disclosure effects across baseline characteristics of properties, owners, and neighbourhoods.[39] Table A12 gives results estimating heterogeneity in effects, looking always at our primary outcome (property having paid at least its annual liability). We consider six different dimensions of heterogeneity;the annual liability of the property, the total value of properties owned by the owner, the baseline compliance rate in the property's parish, the baseline compliance rate in the properties village, the number of property owners in the parish, and the number of property owners in the village).[40][41]

The direct effect of recognition is significantly more negative for the highest value properties, and further, effects are insignificantly different from zero in the bottom two terciles (col. 1). On the other hand, the direct effect of reporting exhibits no heterogeneity across terciles of annuals property liability. The results are similar for heterogeneity by total value of properties (col. 2), but here the recognition effect in the third tercile has a relatively large standard error. Otherwise we do not find evidence of heterogeneous effects by baseline compliance (cols. 3 and 4) or number of properties in local area (cols. 5 and 6). We also do not find evidence of heterogeneity in direct effects of reporting by any of these measures.[42]

So while the effect of reporting is consistently positive across properties of different value, the average negative recognition effect comes from the most expensive (highest liability) properties. Through the lens of our conceptual framework, this suggests that the privacy costs relative to shame costs are highest for the highest value properties in the city.[43] We test this more directly at the bottom of the table showing, for each tercile, the p-value for the Wald test that the sum of the coefficients is zero. Looking across all terciles of property liability (col. 1), the p-value is always higher than 0.2, so we cannot reject the null that the shame effects are zero for each tercile. Across terciles of owner

[39]Here, we always restrict our sample to the properties in the public disclosure treatment groups (i.e. we discard the benchmark groups from the heterogeneity analysis).

[40]Appendix Figure A3 provides a map of property density, villages, and parishes in Kampala.

[41]For each characteristic we take terciles across the relevant units (e.g. wealth across owners, compliance across parishes or villages, etc.) and classify properties based on the tercile they fall into. The table reports interactions between tercile dummies and our main treatment groups, always treating the first tercile as the base.

[42]We hypothesise that this may reflect very low tax morale across the city as a whole, which means that shame motives do not come into effect even in more compliant areas.

[43]Responses from our endline survey shed some light on potential reasons for this. Some taxpayers appear to have been concerned that the actual tax amount would be disclosed and that preferential tax agreements would be made public. For example, some taxpayers stated that the reason they would not want their compliance to be made public was that "we might be paying different rates". Others were worried about "showing off" or encouraging jealousy from others. These types of concerns may be particularly pertinent among wealthier taxpayers.

property wealth (col. 2) the p-value in the first tercile is just 0.08, so there is some suggestive evidence that shame costs may exist the lowest tercile of owner wealth, but with low statistical significance. These findings have interesting implications for policies attempting to leverage public disclosure to target high value properties and wealthy owners in particular. It seems that the most effective way to raise compliance among those households is to warn taxpayers that their private information will be publicly disclosed if they do not. By contrast, appealing to public opinion of tax behavior may be more effective in raising compliance among lower value properties and less wealthy owners.

5.2 Knock-on Effects: estimates of public dissemination

In the second wave, we test for knock-on effects to tax compliance when property owners are informed of the tax behavior of their peers. Our main results are reported in Table 2, showing estimates of effects on tax compliance for our three administrative data outcomes, following equation 4.[44]

There are three main takeaways from the results. First, and perhaps unsurprisingly, the knock-on effect of reporting delinquency lowers compliance. Properties whose owners received lists of tax delinquents are 0.65 percentage points less likely to have their liability paid for, a 21% decline on the control group mean of 3.1 percentage points (col. 1). The percentage decline on the control mean is slightly smaller (-19%) if we measure tax compliance as any payment being made (col. 2). In column 3, we see qualitatively similar results for the total amount of payments made, though the estimate is noisy and we cannot reject that it is zero at conventional significance levels. In other words, while the threat of being reported induces compliance, disseminating these reports lowers others' compliance.[45]

Second, the knock-on effect of recognizing compliers also lowers compliance. Properties whose owners received lists of tax compliers are 0.78 percentage points less likely to have their liability paid for, a 24% decline on the control group mean of 3.1 percentage points (col. 1). The percentage decline on the control mean is smaller (-17%) if we measure tax compliance as any payment being made (col. 2). In column 3, we see qualitatively similar results for the total amount of payments made, though the estimate is noisy and we cannot reject that it is zero at conventional significance levels.

The third takeaway is that the Delinquent List and Complier List effects are equal, i.e. $\hat{\kappa}_S = \hat{\kappa}_H$. The p-value for the test that these coefficients are equal is at least 0.51

[44]Table A14 runs a robustness check of Table 2 including all of the baseline characteristics from Table A6 as controls. As expected, our main results are very similar to those in Table 2 without controls.

[45]While Perez-Truglia & Troiano (2018) find no effects on recipients of reporting tax delinquency of others, their peer group is very different from ours. In their setting, behavior is only reported to other delinquents (a small minority of the tax base in the US context), while in ours, behavior is disseminated to many potentially compliant taxpayers.

across specifications (reported at the bottom of Table 2). This finding has implications linking to the conceptual framework in the section that follows.

5.2.1 Linking to conceptual framework: what do estimates of knock-on effects tell us?

Focusing here on the knock-on effects of public dissemination, the government's choice of policy will affect beliefs about the evasion rate ($\hat{\lambda}$). The counterfactual exercises we consider holds both the probability of being discovered as a property owner, and the probability of being discovered as a property tax delinquent constant at baseline values ($p(e) \equiv \underline{p}$ and $q(e) \equiv \underline{q_e}$).

To be explicit we introduce $\theta^{\dagger}$ as the probability that recipient internalises the information in the message they receive. We model the average expected penalties for non-payment, intrinsic motivation to pay, and the expected social sanctions of delinquency as follows: $m \equiv m_0 - m_1\hat{\lambda}$, $i \equiv i_0 - i_1\hat{\lambda}$, and $z \equiv z_0 - z_1\hat{\lambda}$. Further, we assume that the expected monetary penalties of evasion, intrinsic motivation to pay, and expected social sanctions for delinquency are decreasing in evasion beliefs (i.e $m_1 > 0$, $i_1 > 0$, and $z_1 > 0$).

We define at baseline (i.e. in our control group) the belief of the evasion rate $\hat{\lambda} \equiv \underline{\hat{\lambda}}$. Under the reporting state agents are sent messages listing tax delinquents, which can lead to a change in beliefs $\Delta\hat{\lambda}_S$. Under the recognition state, agents are sent messages listing tax compliers, which can lead to a change in beliefs $\Delta\hat{\lambda}_H$. Conceptually beliefs can change from a pure signal of information (moving beliefs closer to the true evasion rate λ) or from framing effect (raising evasion beliefs under reporting, and lowering evasion beliefs under recognition).

Plugging back in to equation 2 and simplifying notation with $\underline{M} \equiv t - m(\underline{\hat{\lambda}}) - i(\underline{\hat{\lambda}}) - z(\underline{\hat{\lambda}})\underline{p}[\underline{q}_1 - \underline{q}_0]$ and $\tilde{z}_1 = (\underline{p}_1\underline{q}_1 - \underline{p}_0\underline{q}_0)z_1$, the share of evaders under each regime (λ_σ) will be:

$$\begin{aligned}
\lambda_C &= F(\underline{M}) \\
\lambda_S &= F(\underline{M} + \theta^{\dagger}(m_1 + i_1 + \tilde{z}_1)\Delta\hat{\lambda}_S) \\
\lambda_H &= F(\underline{M} + \theta^{\dagger}(m_1 + i_1 + \tilde{z}_1)\Delta\hat{\lambda}_H)
\end{aligned}$$

Next, we assume that public dissemination policies induce a small enough change in payoffs that their effect on compliance can be approximated by a first order taylor series. Therefore, the knock-on effect of public reporting κ_S and public recognition κ_H

on compliance can be written as:

$$\kappa_S \equiv \lambda_C - \lambda_S = -\theta^{\dagger}(m_1 + i_1 + \tilde{z}_1)\Delta\hat{\lambda}_S F'(\underline{M}) \tag{8}$$

$$\kappa_H \equiv \lambda_C - \lambda_H = -\theta^{\dagger}(m_1 + i_1 + \tilde{z}_1)\Delta\hat{\lambda}_H F'(\underline{M}) \tag{9}$$

Again, effects will be zero if text message lists are completely ignored (i.e. $\theta^{\dagger} = 0$), that there is no variation in the idiosyncratic motivation to comply near the baseline state (i.e. $F'(\underline{M}) = 0$), or if neither the expected penalties, idiosyncratic motivation to comply, nor the expected social sanctions depend on evasion beliefs (i.e. $m_1 = 0$, $i_1 = 0$ and $z_1 = 0$). We can discard these theoretical cases based on our empirical results below which find that both reporting and recognition effects are significantly different from zero.

Otherwise, if public dissemination raises evasion beliefs, compliance will fall, and if it lowers evasion beliefs, compliance will rise. Further, if $\kappa_S > \kappa_H$ it suggests that sending lists of delinquents raises evasion beliefs relative to sending lists of compliers. Which could be the case if there is a 'framing' effect of shame lists that causes people to raise their evasion beliefs relative to honor lists. If $\kappa_S = \kappa_H$ it suggests that sending lists of delinquents and sending lists of compliers raises evasion beliefs. Which could be the case if there is a constant 'information' effect of receiving either a list of delinquents or compliers and no additional framing effect.

Finally, we note that our framework does not allow us to distinguish the effects of what Bicchieri & Xiao (2009) call empirical (e.g. our i_1) and normative expectations (e.g. our z_1). That is, we cannot say if our effects reflect how evasion beliefs influence the intrinsic motivation to pay, or their expected sanctions from the public. Further, we are also unable to separately identify the effect of compliance beliefs through it's effect on the expectation of being penalised for evasion (e.g. m_1).

Our empirical estimates are significantly different from zero for both delinquents and compliers lists. In particular, we find evidence that $\kappa_S = \kappa_H < 0$. In the framework of the model this suggests $\Delta\hat{\lambda}_H = \Delta\hat{\lambda}_S > 0$, or in words, the Delinquents and Compliers Lists lead to similar adjustments in evasion beliefs upwards (they cause recipients to lower their beliefs of the compliance rate).

These results are consistent with Delinquents and Compliers Lists providing an equally informative signal about the true compliance rate in the city. Recall from Figure 4 that taxpayers appear overly optimistic with beliefs about compliance at baseline substantially higher than the actual compliance rate. Together with baseline evidence that the evasion belief is significantly over-optimistic, we interpret our findings as both shame and honor lists providing an informative signal of the true compliance rate which, in this context, shifts beliefs about the evasion rate upwards. For example, it seems that when taxpayers receive a list of compliers in the city, they interpret this to mean that fewer people are complying than they otherwise thought - perhaps because they do not recognize anyone on

the list. In the next section, we show direct evidence that the Compliers and Delinquents Lists caused taxpayers to lower their beliefs of the compliance rate.

We may have expected that sending Compliers lists would cause beliefs to shift upwards, at least relative to sending lists of delinquents, due to a 'framing' effect. It could be that our Compliers Lists have a larger (more negative) information effect that counter balances a latent framing effect. While we cannot rule out this scenario directly, it is clear that framing effects do not play a large enough role to cause Compliers List recipients to be relatively more optimistic about the compliance rate than Delinquents List recipients.

5.2.2 Knock-on Effects: survey outcomes

In Table 3 columns 1-3 we estimate knock-on effects on our primary outcome (property having paid at least their liability, col. 1) and more proximate outcomes from our endline survey: beliefs about the compliance rate in Kampala (col. 2) and beliefs about the compliance rate of the most expensive properties (col. 3). The estimates of our primary outcome (col. 1) show that our main results from the full sample hold for the survey respondents.

The first set of survey outcomes we consider are measures of beliefs about compliance around Kampala (cols. 2-3). We see that property owners who receive a list of tax delinquents have lower beliefs on the city wide compliance rate (-2.3 percentage points) and lower beliefs on the compliance rate of the most expensive properties (-4.3 percentage points). Similarly, receiving a list of tax compliers lowers beliefs on the city wide compliance rate (-1 percentage point), though the estimate is noisy, and lower beliefs on the compliance rate of the most expensive properties (by a precisely estimated -3.3 percentage points). These results are consistent with our interpretation of mechanisms above, that both Delinquent and Complier Lists provide an informative signal of the true compliance rate causing taxpayers to lower their beliefs downwards towards the truth.[46] This reduction in beliefs lowers tax morale, ultimately lowering the propensity to comply. Interestingly, the effects are strongest on beliefs about the compliance behavior of the wealthiest property owners. It could be that taxpayers recognize these wealthy owner names and so knowledge of their non-compliance is more salient. At the same time, those that receive lists of compliers may believe that since they do not see wealthy taxpayers on their list, these taxpayers did not pay. This is particularly important for tax compliance - in our baseline survey, 69% of taxpayers agreed that they would be more motivated to pay their rates if they knew the rich and influential were paying their tax share.

We can go one step further, and estimate the compliance response to a change in beliefs

[46] Again, it is also worth noting the over optimism of taxpayers. In Table 3 the control group mean belief of the compliance rate (52% from col. 2) in Kampala is substantially higher than the actual rate (10%), and similarly for the belief of the compliance rate of the top 5% of property owners (62% from col. 3 vs. the actual rate of 22%).

about the compliance rate using a instrumental variables approach. In Table 3 columns 4 and 5 we estimate the effect of beliefs on compliance using our randomized treatment groups as instrumental variables. Column 4 estimates the effect of beliefs about the compliance rate in Kampala on compliance. This estimate is statistically insignificant, but the point estimate is positive as we would expect. Column 5 estimates the effect of beliefs about the compliance rate of the highest value properties in Kampala, and is significant at the 5% level. Here we find that raising the belief that the highest value of properties pay their tax by 1pp raises the actual compliance by about 0.4pp. In general, given the small magnitude of effects in our first stage, the F-statistics are low in columns 4 and 5.

As potential alternative mechanisms we consider other measures of taxpayer beliefs in Table 4. Here we estimate knock-on effects to other outcomes from our endline survey, where each column is an indicator: believes the KCCA can detect who pays tax (col. 1), believes the KCCA acts in the best interest of it's citizens (col. 2), believes that tax behaviour is better kept private (col. 3), believes that tax delinquency is wrong and punishable (col. 4), believes that tax behaviour will be publicly disclosed in the future (col. 5), and believes they would want it known that they pay taxes if they comply (col. 6). We find no significant knock-on effects of Delinquent or Complier Lists on any of these alternative beliefs. Together these findings help identify a likely mechanism, that knock-on effects lower prior beliefs about the compliance rate in the city and especially for the wealthiest properties.

5.2.3 Knock-on Effects: heterogeneity and sub-treatments

As with direct effects, we explore heterogeneity in effects across baseline characteristics of properties, owners, and neighbourhoods. Table A16 gives results estimating heterogeneity in effects looking always at our primary outcome (property having paid at least it's annual liability). Again, we consider six different dimensions of heterogeneity; the annual liability of the property, the total value of properties owned by the owner, the baseline compliance rate in the property's parish, the baseline compliance rate in the properties village, the number of property owners in the parish, and the number of property owners in the village.[47]

We find no statistical evidence of heterogeneous knock-on effects across any of the dimensions we consider. Further, we can never reject the null that effects of Delinquent Lists are equal to effects of Complier Lists within terciles (top rows in bottom panel of Table A16).

[47]For each characteristic, we take terciles across the relevant units (e.g. wealth across owners, compliance across parishes or villages, etc.) and classify properties based on the tercile they fall into. The table reports interactions between tercile dummies and our main treatment groups always treating the first tercile as the base.

For Complier lists, we also consider the subtreatment that sampled compliers only from the wealthiest (top 5%) of property owners in the city. Our estimate is noisy and we cannot reject that is zero, nor that it is different from the effect of receiving a list of random taxpayers. We hypothesise that there may be two countervailing effects that limit the negative impact of these types of Complier lists. First, it may be that Complier lists cause taxpayers to update their beliefs downward in part because they do not recognize the names of anyone on the lists they see, and therefore assume that those they know are not paying their rates. In the case of Wealthy Complier lists, taxpayers may be more likely to recognize the names of those on the lists they receive or understand why others they know do not qualify for this list, and therefore do not make the same assumption. Through the lens of our conceptual framework, $\hat{\lambda}_W$ (the proportion of those believed to be compliers under the Wealthy Complier treatment) $= \hat{\underline{\lambda}}$. Second, being made aware of the fact that wealthy taxpayers are paying their rates may have a separate positive effect on intrinsic motives to pay, due to taxpayers feeling that the tax system is being fairly implemented and wealthy are paying their share. In the context of our conceptual framework, $i \equiv i(\hat{\lambda}, \hat{\lambda}_W)$, where the compliance of wealthy taxpayers is particularly influential on one's motivation to pay taxes.

5.3 Tax morale determinants of tax compliance

In this paper we provide insights into a variety of tax morale determinants of compliance. As we have discussed thoroughly, our direct effects are consistent with the model where there are no social penalties to delinquency nor social gains to compliance, instead they suggest there is a cost to being know as tax eligible in our setting.

In addition, our results on benchmark effects (Section 5.1.2) shed light on the relative importance of different tax morale mechanisms. We compare messages appealing to reciprocity (reminders of the services their tax payments contribute to) and relationship management (reminders of the contact number to call for any questions or concerns), and enforcement practices (reminders of the different legal penalty measures that the government can take against delinquents). The enforcement messages clearly generate a positive compliance response, while the others have no statistically detectable effect.

Further, our knock-on effects (Section 5.2.2) shed light on the role of compliance beliefs. Using our instrumental variables approach, we show that an increase in compliance beliefs (induced by randomized variation in our treatments) cause an increase in the likelihood that an individual pays their taxes. Our estimates are imprecise for the belief of the city-wide compliance rate, but we find that increasing the perceived compliance rate of the richest property owners by one percentage point raises actual compliance by about 0.4 percentage points. How can these results help explain why compliance is so low in many developing country cities? The positive relationship between compliance beliefs

and actual compliance highlights the potential of a "compliance trap": taxpayers are less inclined to comply when they believe other citizens – and in particular wealthy taxpayers – are not paying.

An important question that deserves further attention but goes beyond the scope of our study is *why* it is that shame costs do not appear to play a role in this context. It may be that, in a context of low compliance and low service provision, payment of taxes does not gain sufficient social esteem for public disclosure to have a counterbalancing positive effect on tax morale. There may not necessarily be a stronger preference for privacy among taxpayers in Kampala, but instead, the positive effects of public disclosure may rely on a certain level of trust in government, moral obligation to pay taxes, and perceived level of compliance amongst others.

5.4 Policy Implications

Our study has important implications for policy makers in low-compliance, low-income urban settings like Kampala. There are three main takeaways for policymakers. First, is that public disclosure policies in such a context seems to have, at best, limited effects on compliance, and at worst may even reduce tax compliance. As we have shown, there are positive direct effects of public reporting on compliance, but these are counterbalanced by negative knock-on effects of this disclosure: when individuals are notified of fellow citizens' delinquent behavior, they become less likely to comply themselves. If public disclosure policies were put into practice, the total effect on taxpayer behavior would be a combination of these two effects. Treating the full effect of a full public disclosure program as the sum of direct and knock-on average effects, this gives a very small but negative (-0.075 percentage point) effect on tax compliance. At the same time, recognition of compliant taxpayers also backfires in this context, with both direct and knock-on effects of recognition reducing tax compliance. Putting together these effects, we would expect a relatively large negative effect on compliance (-1.33 percentage points) from rolling this policy out. It appears that policies that preserve taxpayer privacy are more effective at raising compliance in these settings.[48]

Second, we show that standard reminders about the legal enforcement ability of the government can raise compliance by about 1.3 percentage points. This is over twice the size of the estimated direct effect of reporting, even before accounting for the negative knock-on consequences of reporting. These simple enforcement messages represent a clearly more effective policy tool than public disclosure. Although our outcome measures for amount paid are noisy, they suggest that sending an enforcement message (rather than a simple reminder) raises anywhere between 200 - 8,400 UGX per property at no

[48]Of course, these estimates of total effects are only a back of the envelope calculation and the effects of disclosure may change over time, for better or worse, as taxpayers get used to this.

additional cost (95% CI).[49]

Third, our study suggests that taxpayers are more likely to pay their taxes if they believe others are complying - in particular, wealthy taxpayers. Currently, taxpayer expectations exceed actual compliance, and so efforts to publicise actual compliance rates would not be effective. A concerted effort to, for instance, raise compliance rates of the wealthy to a high level and make this salient to all taxpayers, could push the city out of the current bad equilibrium and into a high compliance equilibrium.

Finally, our study suggests that SMS can be an effective tool for policy communication. We called 8,525 taxpayers for our endline survey. Of the 6,303 taxpayers that we were able to reach by phone, 87% had correct phone numbers according to the KCCA database. The majority (88%) of our endline survey respondents recall receiving a message from KCCA regarding property rate compliance at some point in the study year. However, sending messages with links to online information may be less effective - only 15% of those claiming to have received a link to a website report opening it. This makes sense when taking into account people's access to devices - while 78% of those surveyed in our baseline noted that someone in their household had access to a smartphone, only 26% said the same of a computer. Accessing and internalising information from online lists may not be realistic in this context.

6 Conclusion

Our study investigates the impacts of public disclosure policies on tax compliance in a low-income, low-compliance setting. Through a field experiment with the Kampala Capital City Authority, we separately study the effects of public reporting of delinquents from public recognition of compliers. Further, we separately identify the direct effects of warning taxpayers about public disclosure from the knock-on effects induced through information dissemination.

On the effects of publicly reporting delinquent, we find evidence of positive direct effects whereby those warned are more likely to pay their taxes, but negative knock-on effects on those who are informed. On publicly recognizing tax compliers, however, we find both negative direct effects and negative knock-on effects. We investigate these effects further to better understand the underlying mechanisms. We find that the positive direct effects of public reporting are similar in magnitude but of opposite sign than the direct effects of public recognition. Using a parsimonious model, we show that this can be explained by a zero shame cost to tax delinquency alongside a positive privacy cost to being revealed as a property owner. Further, we provide evidence that public disclosure

[49]By contrast, other types of information messages that appeal to reciprocity or that provide contact details of government relationship managers have no effect on compliance - we explore this in more detail in (Ahabwe et al. 2023).

polices induce negative knock-on effects by updating taxpayers' beliefs about general compliance downward. Through the lens of our model, this could work through lowering intrinsic motivation, expected state punishment, or expected social sanctions.

Why is it that shame and privacy costs act in this way, and therefore public disclosure has such limited effects on tax compliance in this context? It may be that, in a context of low compliance and low service provision, payment of taxes does not gain social esteem, resulting in limited shame of delinquency. There may also be cultural norms related to sharing of wealth that result in a high value of privacy in property ownership. At the same time, perhaps public disclosure would be more effective in a context of high compliance, where disseminating information raises taxpayers beliefs about compliance. Further work is needed to understand whether and how the effects of public disclosure policies systematically vary across different contexts.

The findings of our field experiment shed light on options for future policy. It seems that public disclosure policies are not effective at raising compliance in this context; public recognition uniformly reduces compliance, and while the threat of public disclosure of delinquents does raise compliance with property taxes, this effect is counterbalanced almost exactly by a negative knock-on effect when lists are shared. Instead, a simple message communicating potential enforcement measures is a cost-effective way of raising compliance by at least as much as the threat of public reporting and without its negative knock-on effects.

With the above in mind, it is important to note that the cost effectiveness of nudge policies comes largely from the fact that they are so cheap, and not because they are particularly effective. In the context of property tax collection in sub-Saharan Africa where compliance rates are extremely low, more ambitious policies are needed in order to reach high levels of tax compliance. This remains an important avenue for future research.

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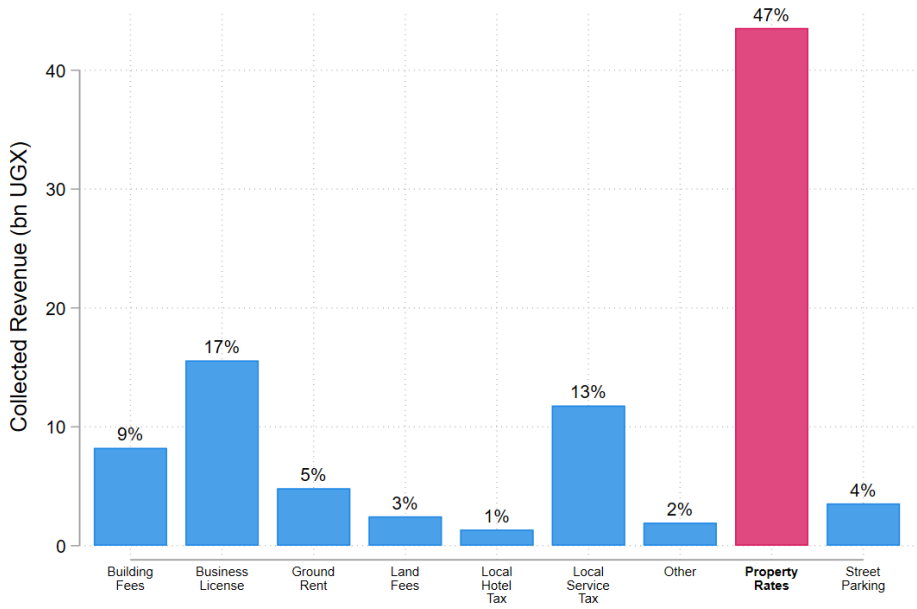
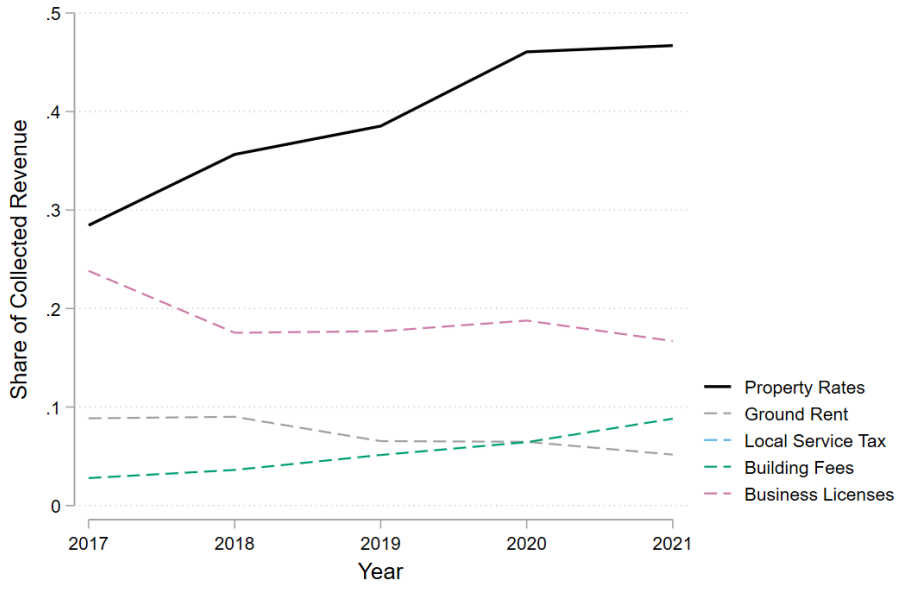
Figures

Figure 1: KCCA Own-Source Revenues by Collection Instrument

(a) Collected Revenues in 2021

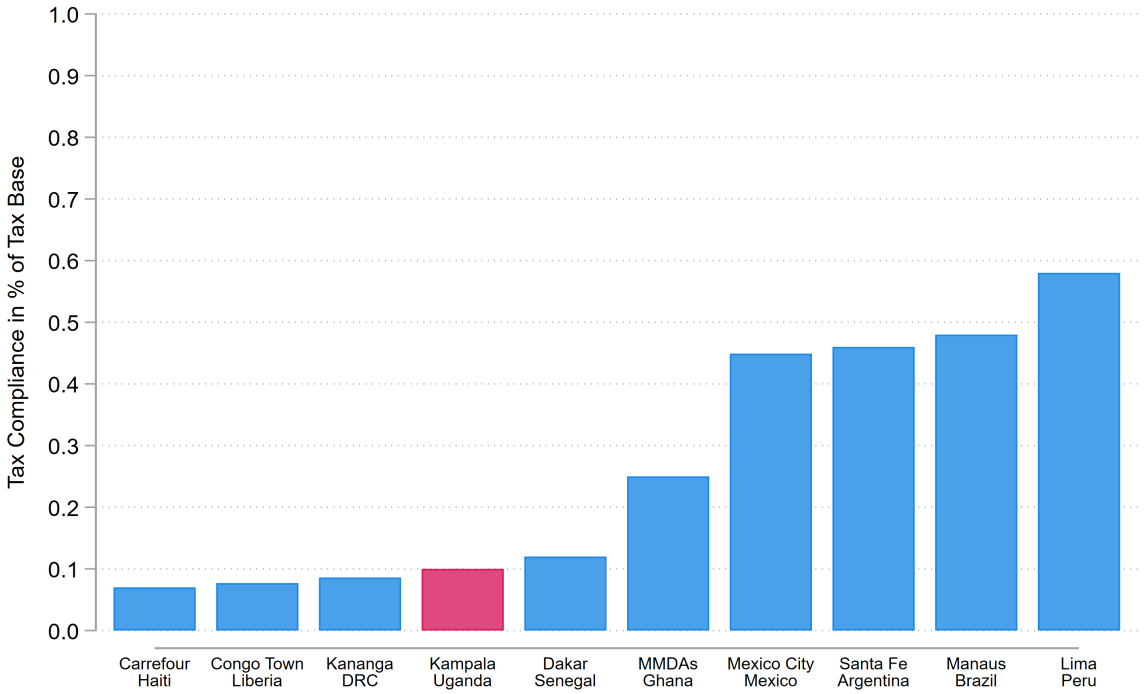


(b) Revenue Share by Instrument 2017-2021



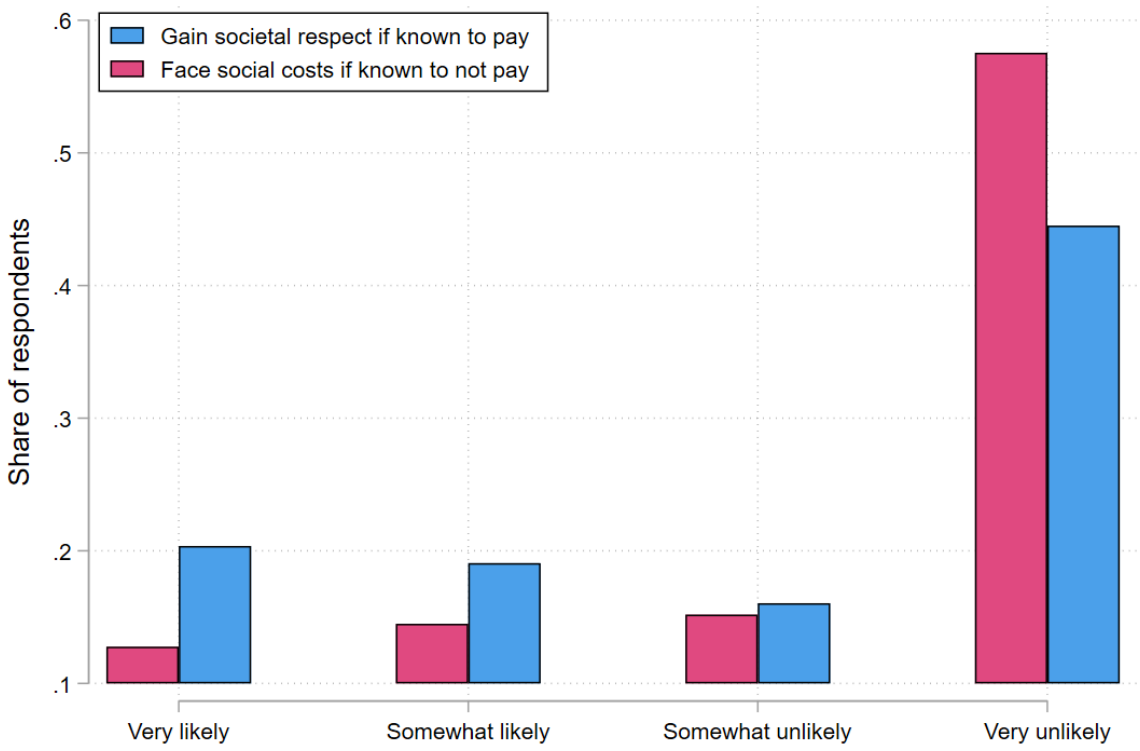
Notes: This figure plots distributions of own-source revenue collection by the Kampala Capital City authority. Panel (a) plots collected revenue in the financial year 2021/22 by collection instrument. Panel (b) plots revenue share from a select set of instruments over the period 2017/18-2021/22.

Figure 2: Property Tax Compliance Rates Across Countries



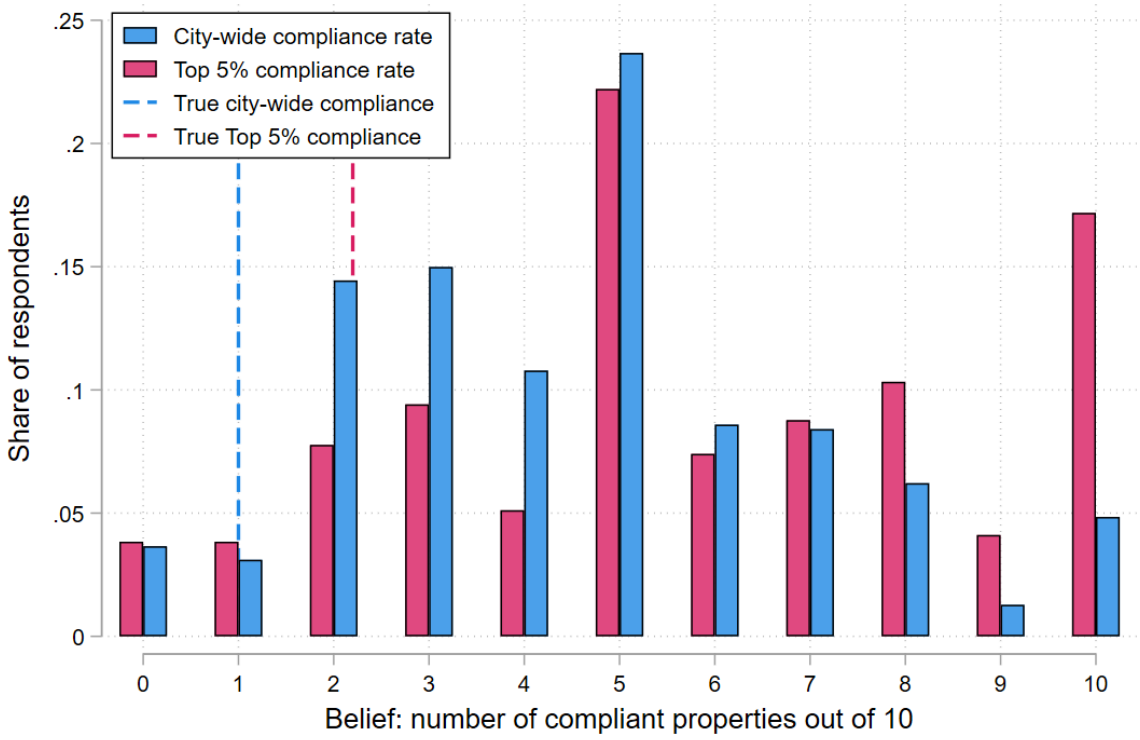
Notes: This figure plots property tax compliance rates from cities around the world. Estimates are taken from studies of property taxes in low- and middle- income countries where available. The cities or regions include: Carrefour, Haiti (Krause 2020); Cong Town, Liberia (Okunogbe 2019); Kananga, DRC (Bergeron et al. 2019); Kampala, Uganda (this paper); Dakar, Senegal (Cogneau et al. 2020); MMDAs in Ghana (Dzansi et al. 2020); Mexico City, Mexico (Brockmeyer et al. 2023); Santa Fe, Argentina (Castro & Scartascini 2015); Manaus, Brazil (Best et al. 2019); and Lima, Peru (Del Carpio 2022).

Figure 3: Baseline Compliance and Delinquency Norms



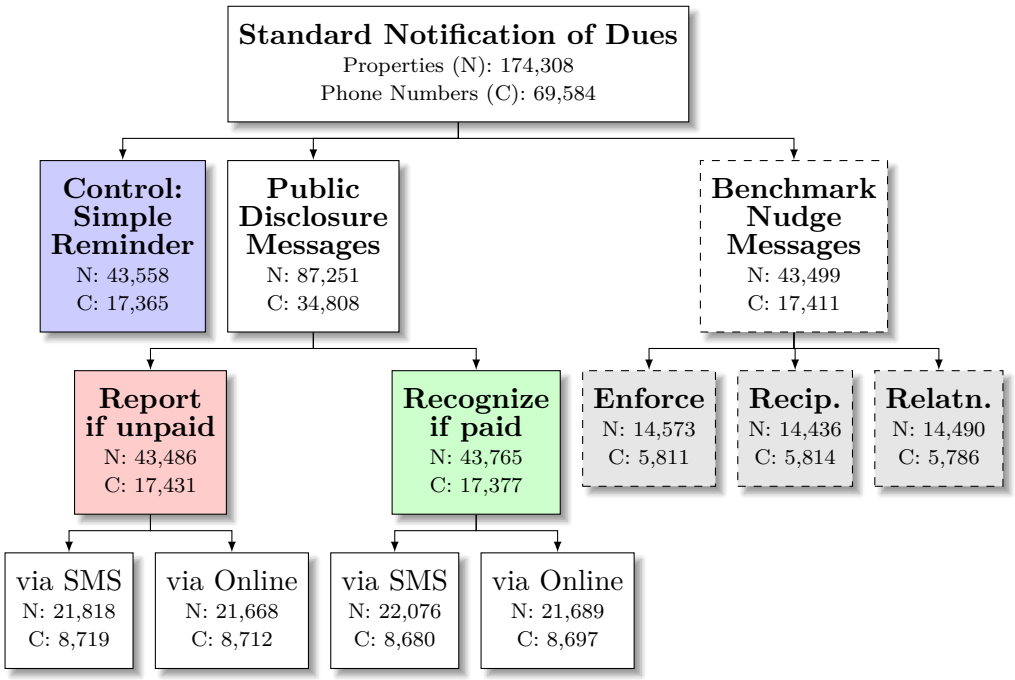
Notes: This figure plots histograms of stated norms of compliance and delinquency. The blue bars plot the share of respondents for different opinions about social gains to compliance, and the red bars plot the share of respondents for different opinions about social costs to delinquency. The data is from the baseline survey of taxpayers.

Figure 4: Baseline Compliance Beliefs



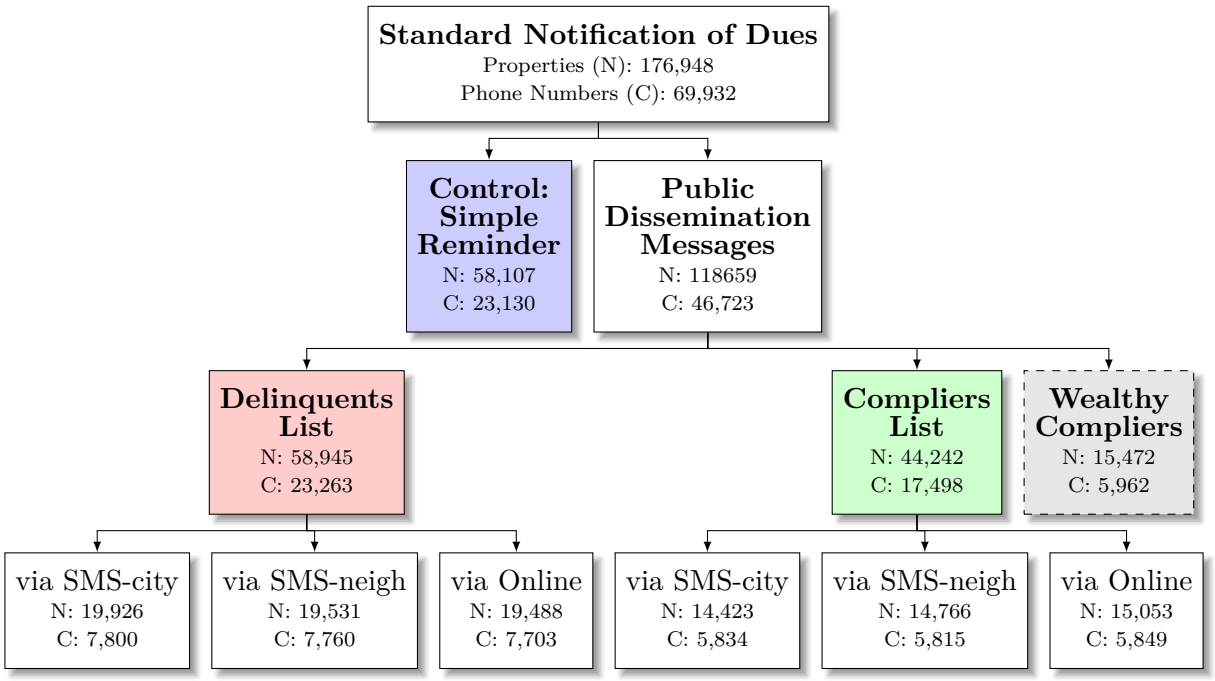
Notes: This figure plots histograms for beliefs of compliance across all properties city-wide (blue bars) and across all wealthy (top 5% by value) property owners (red bars). The data is from the baseline survey of taxpayers. The dashed blue line gives the true compliance rate in the city, the dashed red line gives the true compliance rate for the wealthy property owners.

Figure 5: Treatment groups in first wave (direct effects)



Notes: This Figure depicts the assignment of properties and property owners to the control and treatment groups from the first wave. The number of properties in each group is denoted N and the number of phone number clusters, the level of randomization, is denoted C.

Figure 6: Treatment groups in second wave (knock-on effects)



Notes: This Figure depicts the assignment of properties and property owners to the control and treatment groups from the second wave. The number of properties in each group is denoted N and the number of phone number clusters, the level of randomization, is denoted C.

Tables

Table 1: Direct (wave 1) effects

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Public Disclosure Effects			
Reporting (τ_S)	0.579	0.816	2293.3
	(0.218)	(0.251)	(1522.8)
Recognition (τ_H)	-0.551	-0.786	-2118.1
	(0.193)	(0.213)	(1237.7)
Benchmark Effects			
Enforcement	1.290	1.509	4316.2
	(0.316)	(0.353)	(2101.4)
Reciprocity	-0.0674	-0.0455	279.2
	(0.311)	(0.343)	(1770.9)
Relationship	-0.503	-0.520	-75.47
	(0.274)	(0.317)	(3064.2)
p-value ($\tau_S + \tau_H = 0$)	0.94	0.94	0.94
Control Mean	3.4pp	4.2pp	14.2k
N	174304	174308	172520
N clusters	69584	69584	69033

Note: This table presents treatment effects from the first endline. Each outcome is a measure of payments towards a property in the first endline (may-june 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level.

Table 2: Knock-on (wave 2) effects

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Delinquent List (κ_S)	-0.654	-0.877	-2277.1
	(0.176)	(0.220)	(1373.2)
Complier List (κ_H)	-0.782	-0.811	-2169.4
	(0.185)	(0.258)	(1571.0)
p-value ($\kappa_S = \kappa_H$)	0.44	0.78	0.94
Control Mean	3.1pp	4.6pp	18.1k
N	161709	161713	160027
N clusters	64004	64004	63549

Note: This table presents treatment effects from the second endline. Each outcome is a measure of payments towards a property in the second endline (nov-dec 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level.

Table 3: Knock-on (wave 2) effects to compliance beliefs

	OLS			2SLS	
	(1) Paid Liability (pp)	(2) Kampala Comply (pp)	(3) Rich Comply (pp)	(4) Paid Liability (pp)	(5) Paid Liability (pp)
Delinquent List	-1.65	-2.54	-4.82		
	(0.63)	(1.13)	(1.50)		
Complier List	-1.69	-1.37	-3.53		
	(0.72)	(1.24)	(1.52)		
Belief Kampala Comply				0.66	
				(0.36)	
Belief Rich Comply					0.37
					(0.17)
F stat (K&P)				2.58	5.67
Control Mean	4.3pp	52.5pp	62.2pp	4.3pp	4.3pp
N	7603	7603	7603	7603	7603
N clusters	3614	3614	3614	3614	3614

Note: This table presents treatment effects from the second endline in the survey sample on survey outcomes. In columns 1-3 we present OLS treatment effects for three outcomes: in col. 1 an indicator for whether at least the annual liability was paid at the second endline, col. 2 the stated belief about the compliance rate in Kampala (in percentage points), and col. 3 stated belief about the compliance rate of the most expensive (top 5%) properties in Kampala (in percentage points). In columns 4 and 5 we present IV/2SLS results for the effect of each of the two beliefs on compliance, and the outcome is always an indicator for whether at least the annual liability was paid at the second endline. The Kleibergen-Paap F-statistic is reported at the bottom of the table. Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level.

Table 4: Knock-on (wave 2) effects to alternative beliefs

	(1) KCCA Detection Capacity	(2) KCCA Acts in Best Interest	(3) Compliance Better Kept Private	(4) Unpaid Wrong and Punish.	(5) Tax Behav. Publish. in Future	(6) Want others to know complied
Delinquent List	-0.014	0.0037	0.031	0.010	-0.0021	-0.040
	(0.014)	(0.022)	(0.022)	(0.015)	(0.016)	(0.022)
Complier List	0.0075	0.024	-0.00081	-0.0016	0.013	0.018
	(0.014)	(0.024)	(0.023)	(0.017)	(0.017)	(0.023)
Control Mean	0.91	0.47	0.35	0.91	0.47	0.35
N	8875	8645	8905	9427	8728	9034
N clusters	4236	4109	4247	4454	4136	4267

Note: This table presents treatment effects from the second endline in the survey sample on survey outcomes. Each column is an indicator for a particular stated belief: believes the KCCA can detect who pays tax (col. 1), believes the KCCA acts in the best interest of it's citizens (col. 2), believes that tax behaviour is better kept private (col. 3), believes that tax delinquency is wrong and punishable (col. 4), believes that tax behaviour will be publicly disclosed in the future (col. 5), and believes they would want it known that they pay taxes if they comply. Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level.

For Online Publication Only

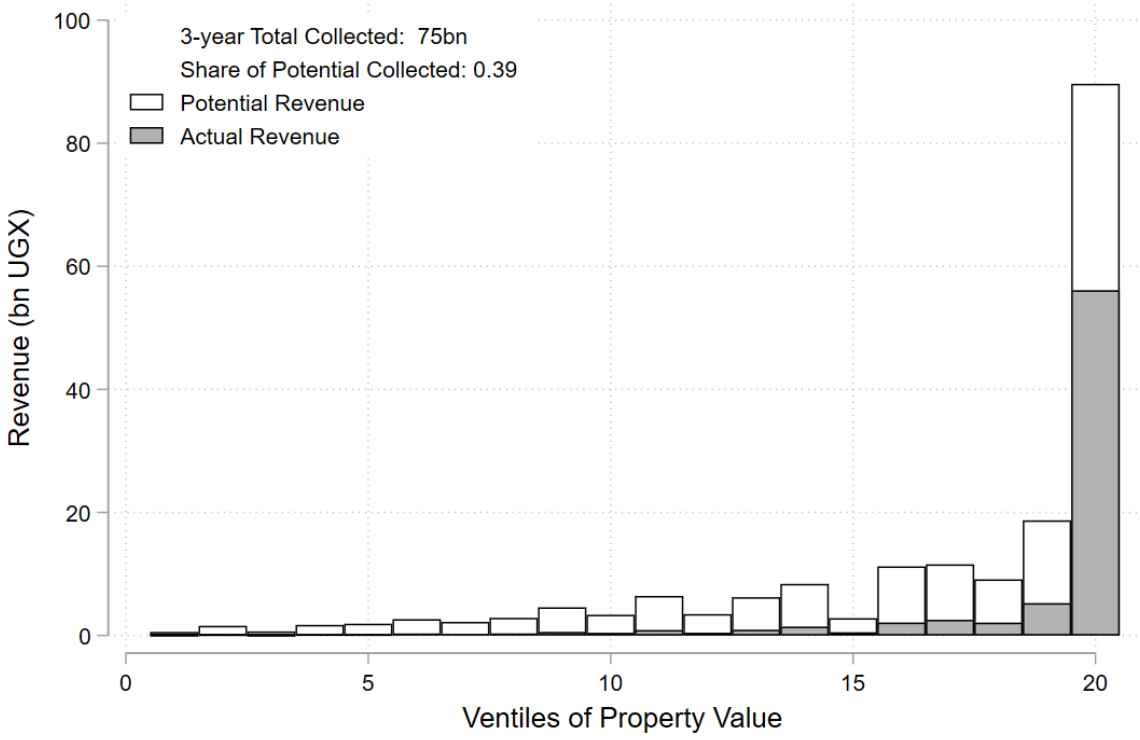
Appendices for Public Disclosure and Tax Compliance: Evidence from Uganda

Priya Manwaring & Tanner Regan

August 8, 2023

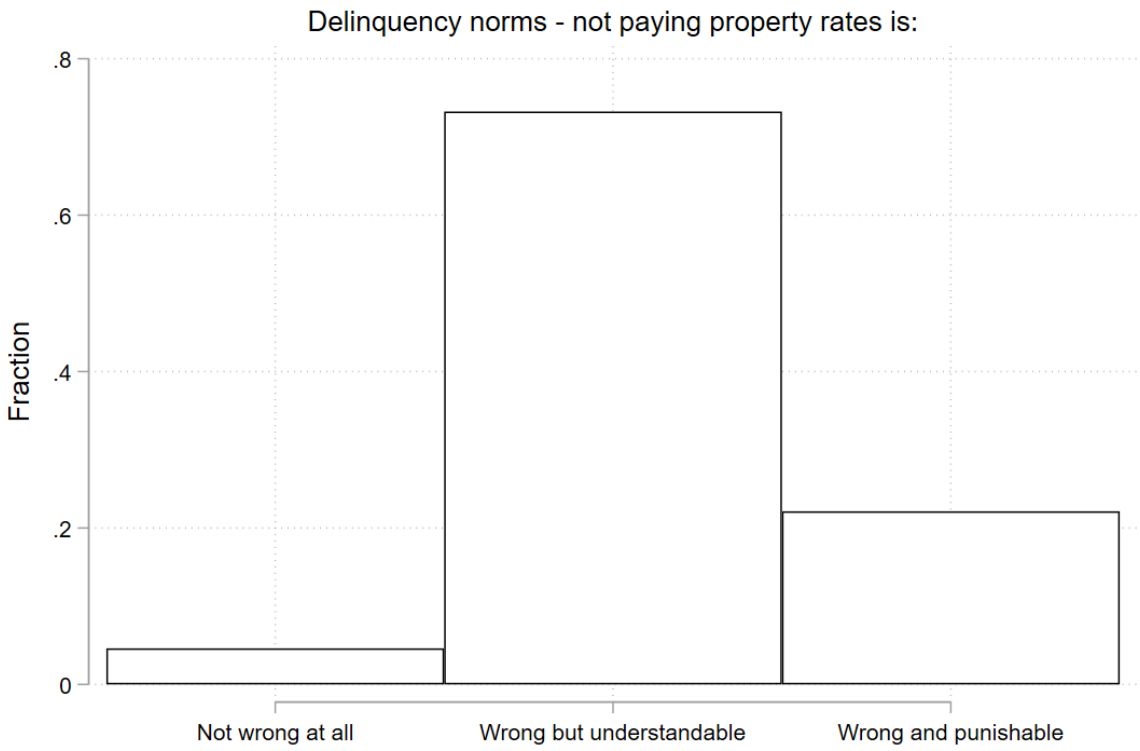
A Appendix Figures

Figure A1: Actual and Potential Revenue Collection vs. Property Value



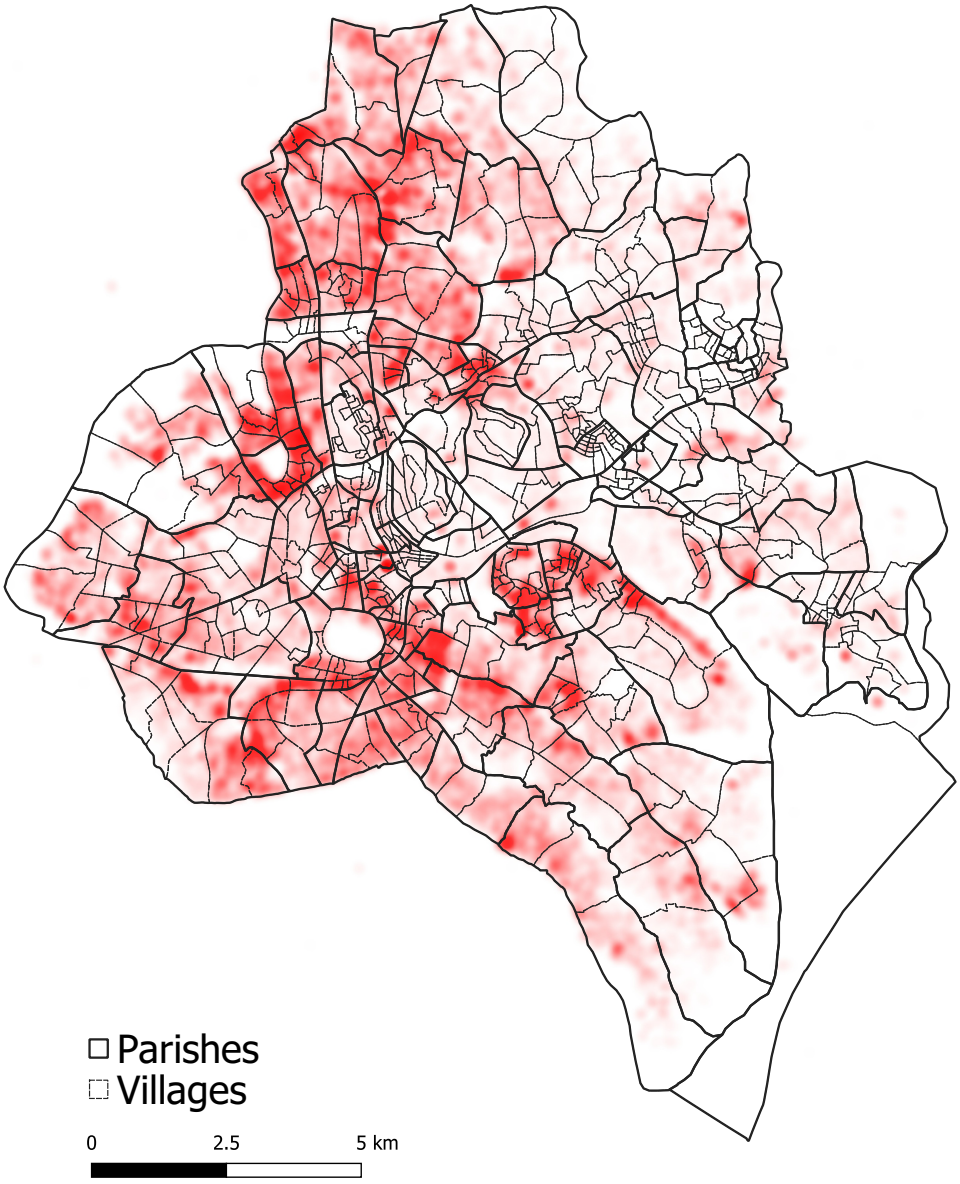
Notes: This Figure plots actual and potential revenue collection by ventiles of property value. Revenue is calculated over three financial years: 2019/20, 2020/2021, and 2021/22. Actual revenue includes all payments: liabilities, interest, and penalties. Potential revenue is calculated as three years of annual liability.

Figure A2: Baseline Delinquency Norms (Is it wrong to not pay?)



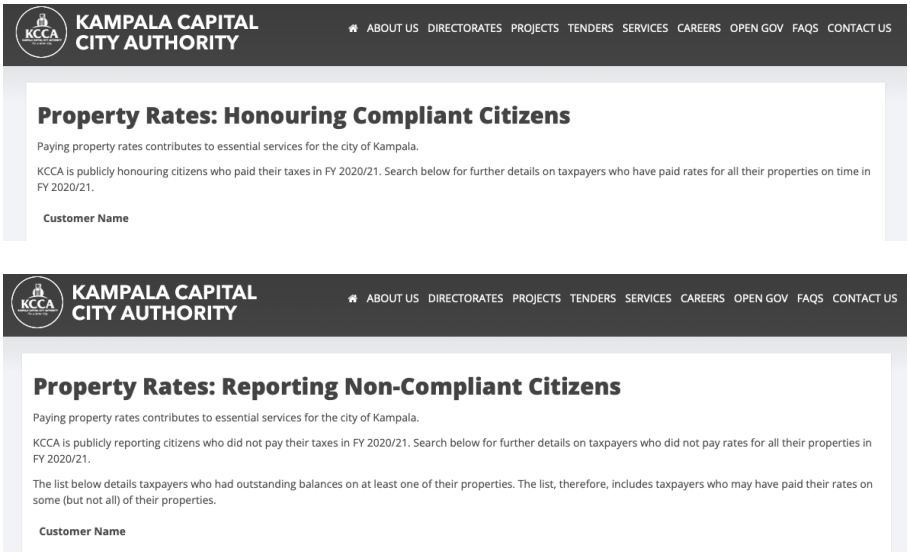
Notes: This Figure plots the histograms of survey respondents opinion that whether paying property rates is wrong. The data is from the baseline survey of taxpayers.

Figure A3: Taxable Properties (density) and village and parish boundaries in Kampala



Notes: This Figure plots the density of taxable properties and village and parish boundaries in Kampala. Darker areas represent higher taxable property density.

Figure A4: Screenshots of the online KCCA 'honoring' and 'reporting' webpages'



Notes: This Figure shows screenshots from the official KCCA webpage. The top image is the page that is sent to the Online Recognition treatmentment group in public dissemination (knock-on effcts in wave 2), and the bottom image is the page sent to the Online Reporting treatment group in public dissemination (knock-on effcts in wave 2).

B Appendix Tables

Table A1: Timeline of Study

Date	**Activity**	**Messages sent (per language)**
Baseline Survey, 2020		
Nov-Dec	In-person baseline Survey conducted by a private surveying company with 1,172 property owners	N/A
Events During First Wave of Experiment, 2021		
14 May	Colleagues at KCCA generated list of all properties and balances as of this date	N/A
17 May	Standard messages sent - one for each property that had an outstanding liability	174,617
18 May	Control and treatment messages sent - one for each phone number against which there was an outstanding liability (i.e. among any of the properties listed as owned by someone with this phone number)	70,381
21 May	Colleagues at KCCA generated list of all properties and balances as of this date	N/A
24 May	Control and treatment messages sent - one for each phone number against which there was an outstanding liability (i.e. among any of the properties listed as owned by someone with this phone number)	69,238
11 June	Colleagues at KCCA generated list of all properties and balances as of this date	N/A
14 June	Standard messages sent - one for each property that had an outstanding liability	170,942
15 June	Control and treatment messages sent - one for each phone number against which there was an outstanding liability (i.e. among any of the properties listed as owned by someone with this phone number)	76,902
Events During Second Wave of Experiment, 2021		
17 Nov	Colleagues at KCCA generated list of all properties and balances as of this date	N/A
18-19 Nov	Standard messages sent - one for each property that had an outstanding liability	176,126
19-22 Nov	Control and treatment messages sent - one for each phone number against which there was an outstanding liability (i.e. among any of the properties listed as owned by someone with this phone number)	69,186
2 Dec	Colleagues at KCCA generated list of all properties and balances as of this date	N/A
3 Dec	Control and treatment messages sent - one for each phone number against which there was an outstanding liability (i.e. among any of the properties listed as owned by someone with this phone number)	69,054
13 Dec	Colleagues at KCCA generated list of all properties and balances as of this date	N/A
14 Dec	Standard messages sent - one for each property that had an outstanding liability	173,566
15 Dec	Control and treatment messages sent - one for each phone number against which there was an outstanding liability (i.e. among any of the properties listed as owned by someone with this phone number)	68,520
Endline Survey, 2022		
Jan -Feb	Endline Survey conducted by a private surveying company over the phone with 4,960 property owners.	N/A

Table A2: Content of 'Direct Effects' messages sent in wave 1

Message	English Text	Luganda Text
Standard	Please pay UGX [liability] as overdue property rates balance for [propertyno] in the next 6 weeks. For details visit [Division] Division or call [KCCA phone contact]. Thank you KCCA	Nyabo/Sebo, osabibwa okusasula [liability] sente zobusulu bw'amayumba ezirudde enyo okusasulwa nga za [propertyno] mu sande 6 ezijja. Ebisingawo genda ku [Division] oba kuba essimu [KCCA phone contact]. Webale nyo. KCCA
Control	Dear [customername], Please remember to pay your overdue property rates in the next 6/5/2 weeks. Thank you KCCA	[customername], jjukira okusasula sente zobusuulu bw'amayumba ezirudde enyo mu sande 6/5/2 ezijja. Webale nyo. KCCA
Benchmark Enforcement	Dear [customername], If you do not pay your property rates, KCCA will implement enforcement measures (including fines and legal action) to recover this amount. Thank you KCCA	[customername], bw'otosasule sente z'obusuulu bw'enyumba yo, KCCA ejja kwongeramu amaanyi ng'ekozesa (omutango n'embuga z'amateeka) okkusobozesa okuzisasula. Webale nyo. KCCA
Benchmark Reciprocity	Dear [customername], Paying property rates makes it possible for KCCA to fund roads, drainage, street lighting and other essential services. Pay your taxes for a better city. Thank you KCCA	[customername], okusasula obusuulu bw'amayumba kisobozesa KCCA okukola enguudo, emyaala, amataala g'okunguudo n'obuwereza obw'enkizo obulala. Sasula emisolo gyo kulw'ekibuga ekisingako. Webale nyo. KCCA
Benchmark Relationship	Dear [customername], KCCA assigns a client relationship manager (CRM) for every taxpayer to address any query. Please contact your CRM with any issues: [arm_name] [arm_contact]. Thank you KCCA	[customername], KCCA yatongoza Maneja akola ku buli kwemulugunya kw'omusasuzi w'omusolo. Okubirizibwa okumwebuzaako singa oba n'ensonga yonna: [arm_name] [arm_contact]. Webale nyo. KCCA
Reporting SMS	Dear [customername], If you do not pay your property rates within 6/5/2 weeks KCCA will REPORT you as a DEFAULTER and share your name and parish in an SMS to fellow citizens and neighbours. Thank you KCCA	[customername], bw'otasasule sente z'abusuulu bw'anyumbayo mu sande 6/5/2, KCCA ejjakkutwala nga alemeddwa okusasula era ejjakusasaanya erinya lyo n'omuluka gwo eri batuuze banno nebaliranwa bo. Webale nyo. KCCA
Reporting Web	Dear [customername], If you do not pay your property rates within 6/5/2 weeks KCCA will REPORT you as a DEFAULTER and share your name and parish on kcca.go.ug/reporting-citizens. Thank you KCCA	[customername], bw'otasasule sente z'obusuulu bw'enyumbayo mu sande 6/5/2, KCCA ejjakkutwala nga alemeddwa okusasula era esasaanye erinya lyo n'omuluka gwo ku mukutu kcca.go.ug/reporting-citizens. Webale nyo. KCCA
Recognition SMS	Dear [customername], If you pay your property rates within 6/5/2 weeks KCCA will RECOGNISE your CONTRIBUTION by sharing your name and parish in an SMS to fellow citizens and neighbours. Thank you KCCA	[customername], bw'onosasula sente z'obusuulu bw'enyumbayo mu sande 6/5/2, KCCA ejjakwenyumiriza mu busasuzi bwo nga esasaanya erinya lyo n'omuluka gwo nga ekozesa obubaka obufunze eri batuuze banno nebaliranwa bo. Webale nyo. KCCA
Recognition Web	Dear [customername], If you pay your property rates within 6/5/2 weeks KCCA will RECOGNISE your CONTRIBUTION by posting your name and parish publicly on kcca.go.ug/honouring-citizens. Thank you KCCA	[customername], bw'onosasula sente z'obusuulu bw'enyumbayo mu sande 6/5/2, KCCA ejjakwenyumiriza mu busasuzi bwo ng'eteeka erinnya lyo n'omuluka gwo mu lujjudde ku mukutu kcca.go.ug/honouring-citizens. Webale nyo. KCCA
Recognition SMS (alternate)	Dear [customername], Thank you for paying your property rates. KCCA will RECOGNISE your CONTRIBUTION by sharing your name and parish in an SMS to fellow citizens and neighbours. Thank you KCCA	[customername], webale nyo okusasula sente z'obusuulu bw'amayumba. KCCA ejjakwenyumiriza mu busasuzi bwo nga esasaanya erinya lyo n'omuluka gwo nga ekozesa obubaka obufunze eri batuuze banno ne baliranwabo. Webale nyo. KCCA

Table A3: Content of 'Knock-on Effects' messages sent in wave 2

Message	English Text	Luganda Text
Standard	Dear [customer name], Please pay UGX [balance] as property rates balance for [property no] by Dec 31. For details visit [division] Division or call your account manager: [arm_contact]. Thank you, KCCA	[Customer name], Osabibwa okusasula UGX [balance] z'ekizimbe kyo namba [property no] ezaasigalayo obutasukka 31 Dec. Bwewabaawo ekyebuuzibwa, tuukirira ekitebe kyaffe e [division] Division oba kubira staff waffe ku ssimu [arm_contact] Weebale nyo. KCCA.
Control	Dear [customer name], Please remember to pay your property rates by Dec 31. Thank you KCCA	[customer name], Jukira okusasula Obusuulu bw'ennyumba yo obutasukka nga 31 December. Weebale nnyo. KCCA.
Delinquents List SMS-city	By paying property rates, you contribute to essential services for the city. KCCA is publicly reporting on property owners who DID NOT pay their balance last year. Here are some of these non-compliers: [100 characters of five names]. Thank you, KCCA	Bw'osasula obusuulu gw'amayumba, oba okoze nnyo ku byetaago ebikulu eby'ekibuga. KCCA egenda kwanika bannanyini bizimbe abataasasula sente zonna ze babanjibwa. Bano beebamu kubataamalayo: [100 characters of five names] Webale nyo. KCCA
Delinquents List SMS-neighbour	By paying property rates, you contribute to essential services for the city. KCCA is publicly reporting on your neighbours who DID NOT pay their balance last year. Here are some of these non-compliers: [100 characters of five names]. Thank you, KCCA	Bw'osasula obusuulu bw'amayumba, oba okoze nnyo ku byetaago ebikulu eby'ekibuga. KCCA eyatuukiriza baliranwaabo abataasasula sente ezaasigalayo omwaka oguwedde. Bano bebatamalayo: [100 characters of five names] Webale nyo. KCCA
Delinquents List online-city	By paying property rates, you contribute to essential services for the city. KCCA is publicly reporting on your neighbours who DID NOT pay their balance last year. Visit this link for a list of these non-compliers: kcca.go.ug/reporting-citizens. Thank you, KCCA	Bw'osasula obusuulu bw'amayumba, oba okoze nnyo ku byetaago ebikulu eby'ekibuga. KCCA eyatuukiriza baliranwaabo ababanjibwa. Tuukirira omukutu guno okuli olukalala lw'abatamalayo: kcca.go.ug/reporting-citizens. Weebale nyo. KCCA
Compliers List SMS-city	By paying property rates, you contribute to essential services for the city. KCCA is publicly recognising the contribution made by property owners who PAID their balance last year. Here are some of these compliers: [100 characters of five names]. Thank you, KCCA	Bw'osasula obusuulu bw'amayumba, oba okoze nnyo ku byetaago ebikulu eby'ekibuga. KCCA eyozaayoza mu lwatu bannanyini bizimbe abaasasula sente ze baali babanjibwa zonna. Bano bebamalayo: [100 characters of five names]. Weebale nnyo. KCCA
Compliers List SMS-neighbour	By paying property rates, you contribute to essential services for the city. KCCA is publicly recognising the contribution made by your neighbours who PAID their balance last year. Here are some of these compliers: [100 characters of five names]. Thank you, KCCA	Bw'osasula obusuulu bw'amayumba, oba okoze nnyo ku byetaago ebikulu eby'ekibuga. KCCA eyozaayoza mu lwatu baliranwaabo abaasasula sente ezasigalayo omwaka oguwedde. Bano bebaamalayo: [100 characters of five names]. Weebale nnyo. KCCA
Compliers List SMS-wealthy	By paying property rates, you contribute to essential services for the city. KCCA is publicly recognising property owners who PAID their balance of over UGX 2m last year. Here are some of these compliers: [100 characters of five names]. Thank you, KCCA	Bw'osasula obusuulu bw'amayumba, oba okoze nnyo ku byetaago ebikulu eby'ekibuga. KCCA eyozaayoza mu lwatu bannanyini bizimbe abaasasula UGX 2m, oba okusingawo, omwaka oguwedde. Bano bebaamalayo: [100 characters of five names]. Weebale nnyo. KCCA
Compliers List online-city	By paying property rates, you contribute to essential services for the city. KCCA is publicly recognising the contribution made by property owners who PAID their balance last year. Visit this link for a list of these citizens: kcca.go.ug/honouring-citizens. Thank you, KCCA	Bw'osasula obusuulu bw'amayumba, oba okoze nnyo ku byetaago ebikulu eby'ekibuga. KCCA eyozaayoza mu lwatu nga esiima bannanyini bizimbe abaasasula sente ezaali zibabanjibwa omwaka oguwedde. Tuukirira omukutu guno okuli abatuuze: kcca.go.ug/honouring-citizens. Weebale nnyo. KCCA

Table A4: Outcome and Balance variables

Outcome variable	**Description**
KCCA administrative data	
Paid Liability	Indicator variable which = 1 if total payments made in the treatment period (i.e. after the first message was sent) at least covered the annual liability of the property.
Any Payment	Indicator variable which = 1 if any payment was made towards the property in the treatment period.
Amount Paid	The total payment amount made towards the property in the treatment period. Because the distribution of liabilities is very skewed, we trim the top 1% of properties by annual liability when we look at total payment amount.
Paid 2020	Indicator variable which = 1 if total payments in the baseline year (2019/2020) covered at leas the annual liability of the property.
Liability - Property	The annual tax amount owed for the property, calculated as 6% of the rateable value.
Total Value - Owner	The total value of all properties owned by the owner
Baseline Compliance Rate - Parish	The mean of 'Paid 2020' across properties in the parish.
Baseline Compliance Rate - Village	The mean of 'Paid 2020' across properties in the village.
Number of Property Owners - Parish	The number of unique property owners who own property in the parish where the property is located.
Number of Property Owners - Village	The number of unique property owners who own property in the village where the property is located.
Property Type	The type, or use, of the property. Coded as either Residential, Commercial, or Other.
Legal Entity	Indicator if the owner is a legal entity rather than a private individual.
km to CBD	Distance in kilometers of the property to Kampala City Hall.
Owner Liability	The annual tax amount owed for all properties of the owner.
Population	Population in property's village
Population Density	Population density in property's village
Endline survey data	
Kampala Comply	A percentage that represents the average belief about the compliance rate of properties in Kampala as a whole. Respondents were asked "Out of every 10 property owners in Kampala that are supposed to pay property rates, how many do you think actually pay?" and we convert these integer responses into percentage points.
Rich Comply	A percentage that represents the average belief about the compliance rate of the most expensive properties in Kampala. Respondents were asked "Out of every 10 property owners with the most expensive property in Kampala that are supposed to pay property rates, how many do you think actually pay?" and we convert these integer responses into percentage points.
Detection Capacity	An indicator variable which = 1 if the respondent responded "yes" to the question "Do you think KCCA has the ability to detect who does and does not pay taxes?" and =0 for those that responded "no". Those who responded "I don't know" were coded as missing.
KCCA Acts Best	An indicator variable which = 1 if the respondent responded "yes" to the question "Do you think that KCCA generally acts in the best interests of citizens?" and =0 for those that responded "no". Those who responded "I don't know" were coded as missing.
Better Kept Private	An indicator variable which = 1 if the respondent responded "yes" to the question "Do you think that information on whether individuals pay taxes or not should be kept private?" and =0 for those that responded "no". Those who responded "I don't know" were coded as missing.
Picked-up	An indicator variable which = 1 for those survey respondents who answered the call for the endline phone survey
Picked-up & Correct	An indicator variable which = 1 for those survey respondents who picked up their phones and were either the individual listed in the KCCA registry, their heir, or their representative on tax matters.

Table A5: Direct (wave 1) Balance Tests

			Information			Public Disclosure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	N	Control Mean	Enforce	Reciprocity	Relationship	Reporting	Recognition
Paid 2020	174308	0.080	0.0055	0.0032	0.0015	-0.0025	0.0012
			(0.0034)	(0.0036)	(0.0034)	(0.0026)	(0.0025)
Lability	174304	0.29	0.024	0.017	0.034	0.010	-0.0050
			(0.021)	(0.018)	(0.022)	(0.011)	(0.012)
Type=Cmrcl.	174308	0.17	-0.0011	0.0073	0.00097	0.0075	0.0051
			(0.0064)	(0.0066)	(0.0062)	(0.0049)	(0.0053)
Type=Other	174308	0.064	-0.0023	-0.011**	-0.0050	-0.0041	-0.0077*
			(0.0061)	(0.0051)	(0.0058)	(0.0046)	(0.0047)
km to CBD	171494	4.91	0.012	0.054	0.0033	0.013	0.015
			(0.042)	(0.042)	(0.040)	(0.033)	(0.033)
Owner Liability	174308	21.1	-2.87	-1.66	1.09	6.12	0.74
			(2.15)	(2.60)	(4.54)	(6.10)	(2.72)
Legal Entity	174308	0.046	-0.0028	-0.017***	-0.0099	0.0040	-0.0015
			(0.0066)	(0.0056)	(0.0061)	(0.0061)	(0.0061)
Pop. Dense.	170345	30.6	3.29	0.29	-0.95	-0.028	-0.15
			(3.63)	(1.54)	(1.50)	(1.13)	(1.12)
Village Pop.	170345	5.60	-0.13	-0.20**	0.0076	0.019	0.13**
			(0.087)	(0.086)	(0.089)	(0.067)	(0.067)
Shr. Vlg. Paid 2020	174308	0.11	0.0027	-0.00010	0.000042	0.0024	-0.0019
			(0.0023)	(0.0017)	(0.0018)	(0.0015)	(0.0014)
Shr. Par. Paid 2020	174308	0.11	-0.00040	-0.0020	0.00070	0.0021	-0.00067
			(0.0019)	(0.0014)	(0.0014)	(0.0014)	(0.0011)

Note: This table presents coefficients from regressions of baseline characteristics on treatment groups from the first endline. Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: Knock-on (wave 2) Balance Tests

	(1) N	(2) Control Mean	(3) Delinquent List	(4) Complier List	(5) Wealthy Compliers
Paid 2020	176766	0.11	-0.0023	-0.0014	0.00070
			(0.0025)	(0.0025)	(0.0039)
Liability	176762	0.33	-0.016	-0.013	-0.026
			(0.014)	(0.015)	(0.023)
Type=Cmrcl.	176766	0.18	0.0042	-0.00035	0.0055
			(0.0041)	(0.0044)	(0.0063)
Type=Other	176766	0.065	-0.0024	-0.0074*	-0.0088*
			(0.0037)	(0.0039)	(0.0051)
km to CBD	174040	4.92	-0.040	-0.032	0.049
			(0.027)	(0.028)	(0.043)
Owner Liability	176766	27.5	-5.15	-3.56	-6.09
			(3.67)	(3.87)	(5.82)
Legal Entity	176766	0.049	-0.0059	-0.0042	-0.0027
			(0.0043)	(0.0049)	(0.0070)
Pop. Dense.	172887	30.0	1.91	0.56	-0.92
			(1.27)	(1.11)	(1.31)
Village Pop.	172887	5.52	-0.0064	-0.0057	-0.073
			(0.057)	(0.060)	(0.088)
Shr. Vlg. Paid 2020	176766	0.11	-0.00093	0.00031	0.00036
			(0.0013)	(0.0014)	(0.0022)
Shr. Par. Paid 2020	176766	0.11	-0.00050	-0.00055	-0.00067
			(0.0012)	(0.0013)	(0.0020)

Note: This table presents coefficients from regressions of baseline characteristics on treatment groups from the second endline. Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: Baseline Survey Attriters Balance Tests

	(1) N	(2) Attriter Mean	(3) Survey Completed
Paid 2020	1883	0.097	0.0072
			(0.023)
Liability	1883	0.30	-0.012
			(0.041)
Type=Cmrcl.	1883	0.22	-0.057**
			(0.027)
Type=Other	1883	0.061	-0.020
			(0.019)
km to CBD	1852	4.83	0.22
			(0.15)
Owner Liability	1883	13.7	2.38
			(2.65)
Legal Entity	1883	0.038	-0.010
			(0.016)
Pop. Dense.	1835	33.7	-5.42
			(6.89)
Village Pop.	1835	5.49	0.82**
			(0.37)
Shr. Vlg. Paid 2020	1883	0.12	-0.022***
			(0.0072)
Shr. Par. Paid 2020	1883	0.12	-0.016***
			(0.0061)

Note: This table presents coefficients from regressions of baseline characteristics on a dummy for whether the property owner completed our baseline survey. Each observation is a property and only property owners targeted for the endline survey are included. Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Endline Survey Attriters Balance Tests

	(1) N	(2) Attriter Mean	(3) Survey Completed
Paid 2020	17549	0.096	0.017
			(0.011)
Liability	17549	0.42	-0.11***
			(0.032)
Type=Cmrcl.	17549	0.19	-0.0057
			(0.012)
Type=Other	17549	0.050	0.0073
			(0.0065)
km to CBD	17267	4.83	0.045
			(0.075)
Owner Liability	17549	37.5	-3.39
			(20.2)
Legal Entity	17549	0.053	-0.0035
			(0.014)
Pop. Dense.	17140	30.1	-1.58
			(1.42)
Village Pop.	17140	5.24	0.35**
			(0.14)
Shr. Vlg. Paid 2020	17549	0.12	-0.012**
			(0.0051)
Shr. Par. Paid 2020	17549	0.12	-0.010***
			(0.0035)

Note: This table presents coefficients from regressions of baseline characteristics on a dummy for whether the property owner completed our endline survey. Each observation is a property and only property owners targeted for the endline survey are included. Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: Unconditional probabilities of perceived status

Perceived Status	Evasion Decision	Control (σ=C)	Reporting (σ=S)	Recognition (σ=H)
owner	evade	$\underline{p}$	$\theta^* \cdot 1 + (1-\theta^*)\underline{p}$	$\underline{p}$
owner	comply	$\underline{p}$	$\underline{p}$	$\theta^* \cdot 1 + (1-\theta^*)\underline{p}$
delinquent	evade	$\underline{pq}_1$	$\theta^* \cdot 1 + (1-\theta^*)\underline{pq}_1$	$\underline{pq}_1$
delinquent	comply	$\underline{pq}_0$	$\underline{pq}_0$	$\theta^* \cdot 0 + (1-\theta^*)\underline{pq}_0$

Note: This table outlines the different unconditional probabilities of an agent's perceived status from the conceptual framework in Section 3. The first column denotes the relevant social status: owner of rental property, or property tax delinquent. The second column denotes the evasion choice of the agent: evade, or comply. The last three columns denote the unconditional probability that the agent is thought to be of a status, given the agent's evasion choice. The third column gives these probabilities under the 'Control' state, the fourth under the 'Reporting' state, and the fifth under the 'Recognition' state.

Table A10: Direct (wave 1) Treatment Effects with baseline controls

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Public Disclosure Effects			
Reporting (τ_S)	0.619	0.821	2308.8
	(0.214)	(0.241)	(1351.9)
Recognition (τ_H)	-0.545	-0.774	-1548.8
	(0.190)	(0.207)	(1204.1)
Benchmark Effects			
Enforcement	1.271	1.463	4498.0
	(0.315)	(0.346)	(1972.5)
Reciprocity	-0.112	-0.119	410.1
	(0.309)	(0.339)	(1709.2)
Relationship	-0.545	-0.605	-502.1
	(0.276)	(0.315)	(2983.7)
p-value ($\tau_S + \tau_H = 0$)	0.83	0.90	0.73
Control Mean	3.4pp	4.3pp	14.3k
N	170341	170341	168605
N clusters	68362	68362	67816

Note: This table presents treatment effects from the first endline. Each outcome is a measure of payments towards a property in the first endline (may-june 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects and baseline controls: (dummy if property paid liability in previous year (2019/20), property liability, a dummy for commercial properties, a dummy for other non-residential properties, distance to the city centre, total liability of property owner, a dummy if the property owner is a legal entity (not an individual), population and population density of the property's village, and the share of properties paying their liability in the previous year at both the village and parish level.). Standard errors in parentheses are clustered at the property owner level.

Table A11: Direct (wave 1) Treatment Effects with sub-treatments

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Public Disclosure Effects			
Reporting	0.587**	0.612**	1447.7
	(0.285)	(0.312)	(1521.8)
Reporting × Online	-0.0160	0.409	1696.1
	(0.324)	(0.389)	(2540.8)
Recognition	-0.630***	-0.820***	-996.1
	(0.224)	(0.252)	(1694.4)
Recognition × Online	0.159	0.0697	-2266.2
	(0.254)	(0.283)	(1735.0)
Benchmark Effects			
Enforcement	1.290***	1.509***	4316.4**
	(0.316)	(0.353)	(2101.4)
Reciprocity	-0.0674	-0.0454	278.9
	(0.311)	(0.343)	(1771.0)
Relationship	-0.503*	-0.520	-75.54
	(0.274)	(0.317)	(3064.1)
N	174304	174308	172520
N clusters	69584	69584	69033
Control Mean	3.4pp	4.2pp	14.2k

Note: This table presents treatment effects from the first endline. Treatments are interacted with sub-group treaments: the base sub-treatment is always public disclosure by text message. Each outcome is a measure of payments towards a property in the first endline (may-june 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects and baseline controls: (dummy if property paid liability in previous year (2019/20), property liability, a dummy for commercial properties, a dummy for other non-residential properties, distance to the city centre, total liability of property owner, a dummy if the property owner is a legal entity (not an individual), population and population density of the property's village, and the share of properties paying their liability in the previous year at both the village and parish level.). Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A12: Direct (wave 1) Heterogeneous Treatment Effects

	dependent variable: Paid Liability (pp)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Annual Liability	Total Value	Baseline Compliance Rate		Number of Property Owners	
	Property	Owner	Parish	Village	Parish	Village
Reporting	0.65**	0.92***	0.40	0.50*	0.82	4.63*
	(0.31)	(0.29)	(0.29)	(0.27)	(0.89)	(2.51)
Reporting × T2	0.13	-0.68	0.53	0.19	-0.11	-4.08
	(0.36)	(0.42)	(0.40)	(0.39)	(0.93)	(2.59)
Reporting × T3	-0.33	-0.28	-0.55	-0.11	-0.32	-4.13
	(0.48)	(0.47)	(0.77)	(1.07)	(0.93)	(2.52)
Recognition	-0.24	-0.076	-0.31	-0.36	-1.16*	0.75
	(0.26)	(0.27)	(0.27)	(0.25)	(0.70)	(1.18)
Recognition × T2	-0.091	-0.36	-0.057	-0.19	1.08	-0.81
	(0.30)	(0.40)	(0.37)	(0.36)	(0.75)	(1.30)
Recognition × T3	-0.85**	-0.73*	-1.12*	-0.89	0.44	-1.40
	(0.40)	(0.42)	(0.66)	(0.93)	(0.73)	(1.20)
T2	0.54**	1.15***	0.14	0.29	-0.12	2.09***
	(0.23)	(0.35)	(0.28)	(0.27)	(0.58)	(0.79)
T3	1.69***	2.30***	1.31**	1.59**	0.25	1.75***
	(0.30)	(0.38)	(0.51)	(0.68)	(0.58)	(0.68)
T1 p-value ($\tau_S + \tau_H = 0$)	0.39	0.08	0.86	0.76	0.80	0.06
T2 p-value ($\tau_S + \tau_H = 0$)	0.31	0.71	0.22	0.77	0.31	0.66
T3 p-value ($\tau_S + \tau_H = 0$)	0.24	0.79	0.17	0.60	0.60	0.67
N	130805	130802	130805	130805	130532	130508
N clusters	52173	52172	52173	52173	52116	52106
Control Mean	3.4pp	3.4pp	3.4pp	3.4pp	3.4pp	4.2pp
First Tercile	97k	2893k	6pp	8pp	45	15
Second Tercile	211k	7331k	15pp	20pp	363	76

Note: This table presents heterogeneous treatment effects from the first endline. The sample is restricted to the control and public disclosure treatment groups (benchmark treatments are discarded). The outcome is always a dummy if at least the annual liability was paid in the first endline (may-june 2021). Each column considers a different dimension of heterogeneity broken into terciles: annual liability of the property (col. 1), total property value owned by owner (col. 2), baseline compliance rate in the parish (col. 3), baseline compliance rate in the village (col. 4), number of property owners in the parish (col. 5), and number of property owners in the village (col. 6). Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A13: Direct (wave 1) Treatment Effects by Correct Phone Number

	dependent variable: Paid Liability (pp)		
	(1) Average Effect	(2) Picked-up	(3) Picked-up & Correct
Public Disclosure Effects			
Reporting	-0.0473	0.149	0.319
	(0.632)	(0.912)	(0.689)
Reporting × Responsive		-0.291	-0.586
		(1.191)	(1.106)
Recognition	-0.0508	0.371	0.541
	(0.596)	(1.016)	(0.804)
Recognition × Responsive		-0.540	-0.884
		(1.225)	(1.109)
Benchmark Effects			
Enforcement	2.297*	0.719	0.521
	(1.189)	(2.482)	(1.716)
Enforcement × Responsive		2.003	2.663
		(2.874)	(2.357)
Reciprocity	1.019	1.669	1.990
	(0.984)	(1.810)	(1.529)
Reciprocity × Responsive		-0.861	-1.462
		(2.145)	(1.975)
Relationship	-0.287	-1.171	0.606
	(0.936)	(0.854)	(1.107)
Relationship × Responsive		1.161	-1.327
		(1.504)	(1.725)
N	16529	16529	16529
N clusters	7886	7886	7886
Control Mean	3.2pp	4.2pp	4.2pp
Share Responsive	.	0.75	0.67

Note: This table presents treatment effects from the first endline in the survey sample. Heterogeneous treatment effects are estimated for responsive vs. unresponsive taxpayers in two ways: responsive taxpayers are those who were reachable by phone (col. 2) and responsive taxpayers are those who were reachable by phone and confirmed that they were the individual listed in the registry (col. 3). The outcome is always a dummy if at least the annual liability was paid in the first endline (may-june 2021). Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A14: Knock-on (wave 2) Treatment Effects with baseline controls

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Delinquent List (κ_S)	-0.639	-0.832	-1560.0
	(0.175)	(0.218)	(1357.4)
Complier List (κ_H)	-0.773	-0.788	-1684.5
	(0.185)	(0.257)	(1542.6)
p-value ($\kappa_S = \kappa_H$)	0.41	0.85	0.93
Control Mean	3.2pp	4.6pp	18.3k
N	158151	158151	156503
N clusters	62911	62911	62451

Note: This table presents treatment effects from the second endline. Each outcome is a measure of payments towards a property in the second endline (nov-dec 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects and baseline controls: (dummy if property paid liability in previous year (2019/20), property liability, a dummy for commercial properties, a dummy for other non-residential properties, distance to the city centre, total liability of property owner, a dummy if the property owner is a legal entity (not an individual), population and population density of the property's village, and the share of properties paying their liability in the previous year at both the village and parish level.). Standard errors in parentheses are clustered at the property owner level.

Table A15: Knock-on (wave 2) Treatment Effects with sub-treatments

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Delinquent List	-0.868***	-1.130***	-3784.8*
	(0.234)	(0.300)	(1947.7)
Delinquent List × Online	0.339	0.367	2403.7
	(0.264)	(0.342)	(2266.4)
Delinquent List × Neighbours	0.334	0.432	2302.4
	(0.275)	(0.353)	(2225.9)
Complier List	-0.751***	-0.700**	-1338.6
	(0.239)	(0.308)	(1941.5)
Complier List × Online	0.0465	0.0218	1516.4
	(0.301)	(0.487)	(2884.2)
Complier List × Neighbours	-0.0573	-0.268	-3395.8
	(0.285)	(0.382)	(2268.5)
Wealthy Complier List	-0.258	-0.352	-225.2
	(0.408)	(0.507)	(3717.1)
N	176762	176766	174936
N clusters	69853	69853	69360
Control Mean	3.1pp	4.6pp	18.1k

Note: This table presents treatment effects from the second endline. Treatments are interacted with sub-group treaments: the base sub-treatment is always an SMS list of compliers (for recognition) or delinquents (for reporting) drawn randomly from the city. Each outcome is a measure of payments towards a property in the second endline (nov-dec 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects and baseline controls: (dummy if property paid liability in previous year (2019/20), property liability, a dummy for commercial properties, a dummy for other non-residential properties, distance to the city centre, total liability of property owner, a dummy if the property owner is a legal entity (not an individual), population and population density of the property's village, and the share of properties paying their liability in the previous year at both the village and parish level.). Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A16: Knock-on (wave 2) Heterogeneous Treatment Effects

	dependent variable: Paid Liability (pp)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Annual Liability	Total Value	Baseline Compliance Rate		Number of Property Owners	
	Property	Owner	Parish	Village	Parish	Village
Delinquent List	-0.81***	-0.54***	-0.71***	-0.92***	-0.85	1.71
	(0.17)	(0.19)	(0.23)	(0.22)	(0.98)	(1.48)
Delinquent List × T2	0.21	-0.31	-0.091	0.45	0.58	-2.70*
	(0.22)	(0.28)	(0.32)	(0.31)	(0.99)	(1.59)
Delinquent List × T3	0.31	-0.064	0.52	0.47	0.0064	-2.36
	(0.38)	(0.36)	(0.62)	(0.81)	(1.00)	(1.49)
Complier List	-0.67***	-0.70***	-1.11***	-1.10***	-0.43	0.0057
	(0.19)	(0.19)	(0.23)	(0.21)	(1.11)	(1.83)
Complier List × T2	-0.13	-0.010	0.33	0.50*	-0.29	-0.90
	(0.24)	(0.30)	(0.33)	(0.30)	(1.11)	(1.96)
Complier List × T3	-0.15	-0.17	0.80	0.66	-0.45	-0.78
	(0.41)	(0.38)	(0.64)	(0.92)	(1.13)	(1.83)
T2	0.51***	0.91***	0.19	0.066	-0.41	1.97*
	(0.17)	(0.24)	(0.26)	(0.23)	(0.89)	(1.07)
T3	2.17***	2.06***	0.98**	1.45**	-0.55	0.89
	(0.28)	(0.29)	(0.46)	(0.63)	(0.89)	(0.97)
T1 p-value ($\kappa_S = \kappa_H$)	0.42	0.38	0.04	0.32	0.58	0.37
T2 p-value ($\kappa_S = \kappa_H$)	0.30	0.50	0.89	0.57	0.16	0.86
T3 p-value ($\kappa_S = \kappa_H$)	0.36	0.37	0.84	0.99	0.83	0.43
N	161709	161706	161709	161709	161355	161326
N clusters	64004	64003	64004	64004	63933	63903
Control Mean	3.1pp	3.1pp	3.1pp	3.1pp	3.1pp	3.1pp
First Tercile	97k	2892k	6pp	8pp	45	15
Second Tercile	211k	7331k	15pp	20pp	363	76

Note: This table presents heterogeneous treatment effects from the second endline. The sample is restricted to the control and public disclosure treatment groups (the recognition of wealthy treatment group is discarded). The outcome is always a dummy if the property paid their annual liability in the second endline (nov-dec 2021). Each column considers a different dimension of heterogeneity broken into terciles: annual liability of the property (col. 1), total property value owned by owner (col. 2), baseline compliance rate in the parish (col. 3), baseline compliance rate in the village (col. 4), number of property owners in the parish (col. 5), and number of property owners in the village (col. 6). Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A17: Knock-on (wave 2) Treatment Effects by Correct Phone Number

	dependent variable: Paid Liability (pp)		
	(1) Average Effect	(2) Picked-up	(3) Picked-up & Correct
Delinquent List	-0.714	-0.738	-0.342
	(0.516)	(0.969)	(0.750)
Delinquent List × Responsive		0.0392	-0.568
		(1.146)	(1.001)
Complier List	-1.126**	-0.652	-0.261
	(0.486)	(0.862)	(0.703)
Complier List × Responsive		-0.621	-1.304
		(1.018)	(0.916)
Wealthy Complier List	-1.150	1.373	0.751
	(0.746)	(1.800)	(1.386)
Wealthy Complier List × Responsive		-3.420*	-2.911*
		(1.996)	(1.664)
N	17178	17178	17178
N clusters	8152	8152	8152
Control Mean	3.5pp	3.5pp	3.5pp
Share Responsive	.	0.75	0.67

Note: This table presents treatment effects from the second endline in the survey sample. Heterogeneous treatment effects are estimated for responsive vs. unresponsive taxpayers in two ways: responsive taxpayers are those who were reachable by phone (col. 2) and responsive taxpayers are those who were reachable by phone and confirmed that they were the individual listed in the registry (col. 3). The outcome is always a dummy if at least the annual liability was paid in the second endline (nov-dec 2021). Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A18: Direct (wave 1) treatment effects on wave 2 outcomes, without controls

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Enforcement	0.0377	0.0235	-3137.3
	(0.233)	(0.264)	(2230.7)
Reciprocity	0.00975	-0.161	-784.9
	(0.204)	(0.233)	(2874.1)
Relationship	0.360*	0.398	9408.7*
	(0.219)	(0.258)	(4903.9)
Reporting	0.0201	0.0737	3557.3
	(0.151)	(0.182)	(2369.2)
Recognition	0.302*	0.267	-406.0
	(0.169)	(0.197)	(2241.7)
Control Mean	1.9pp	2.7pp	16.9k
N	174304	174308	174308
N clusters	69584	69584	69584

Note: This table presents treatment effects from the first year treatments on outcomes from the second endline. Each outcome is a measure of payments towards a property in the second endline (nov-dec 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level.

Table A19: Direct (wave 1) treatment effects on wave 2 outcomes, with controls

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Enforcement	0.0257	-0.00358	-5049.4**
	(0.238)	(0.270)	(2573.7)
Reciprocity	-0.0388	-0.242	-2073.5
	(0.205)	(0.232)	(2511.8)
Relationship	0.369*	0.394	6499.2*
	(0.220)	(0.256)	(3776.3)
Reporting	0.0201	0.0909	3169.6
	(0.151)	(0.179)	(2224.8)
Recognition	0.316*	0.309	282.3
	(0.169)	(0.195)	(2146.7)
Control Mean	1.9pp	2.7pp	16.9k
N	170341	170341	170341
N clusters	68362	68362	68362

Note: This table presents treatment effects from the first year treatments on outcomes from the second endline. Each outcome is a measure of payments towards a property in the second endline (nov-dec 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects and baseline controls: (dummy if property paid liability in previous year (2019/20), property liability, a dummy for commercial properties, a dummy for other non-residential properties, distance to the city centre, total liability of property owner, a dummy if the property owner is a legal entity (not an individual), population and population density of the property's village, and the share of properties paying their liability in the previous year at both the village and parish level.). Standard errors in parentheses are clustered at the property owner level.

Table A20: Direct (wave 1) treatment*knock-on (year 2) effects on wave 2 outcomes, without controls

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Reporting	-0.431	-0.480	1007.3
	(0.290)	(0.341)	(4023.6)
Recognition	0.459	0.404	1132.4
	(0.385)	(0.427)	(3661.7)
Delinquent List	-0.804***	-1.060***	-839.3
	(0.285)	(0.333)	(3824.8)
Complier List	-0.811**	-1.173***	-696.3
	(0.316)	(0.362)	(4272.7)
Reporting × Delinquent List	0.802**	1.002**	3999.0
	(0.399)	(0.470)	(5529.8)
Reporting × Complier List	0.477	0.817	8117.2
	(0.419)	(0.504)	(7098.8)
Recognition × Delinquent List	-0.284	-0.295	173.4
	(0.461)	(0.529)	(6044.1)
Recognition × Complier List	-0.243	0.000467	-1885.3
	(0.492)	(0.562)	(5395.9)
Control Mean	1.9pp	2.8pp	17172.5k
N	167600	167604	167604
N clusters	67557	67557	67557

Note: This table presents treatment effects from the first year treatments interacted with second year treatments on outcomes from the second endline. Each outcome is a measure of payments towards a property in the second endline (nov-dec 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects. Standard errors in parentheses are clustered at the property owner level.

Table A21: Direct (wave 1) treatment*knock-on (year 2) effects on wave 2 outcomes, with controls

	(1) Paid Liability (pp)	(2) Any Payment (pp)	(3) Amount Paid (UGX)
Reporting	-0.465	-0.488	-61.14
	(0.292)	(0.339)	(3847.7)
Recognition	0.471	0.436	556.1
	(0.388)	(0.428)	(3987.9)
Delinquent List	-0.835***	-1.083***	-414.1
	(0.284)	(0.325)	(3638.3)
Complier List	-0.773**	-1.107***	198.1
	(0.317)	(0.359)	(4119.7)
Reporting × Delinquent List	0.837**	1.002**	3258.5
	(0.401)	(0.466)	(5409.7)
Reporting × Complier List	0.472	0.763	8469.1
	(0.421)	(0.504)	(6720.3)
Recognition × Delinquent List	-0.253	-0.247	1513.2
	(0.464)	(0.528)	(6006.5)
Recognition × Complier List	-0.314	-0.0931	-1360.9
	(0.496)	(0.563)	(5405.2)
Control Mean	1.9pp	2.7pp	17126.3k
N	163813	163813	163813
N clusters	66354	66354	66354

Note: This table presents treatment effects from the first year treatments interacted with second year treatments on outcomes from the second endline. Each outcome is a measure of payments towards a property in the second endline (nov-dec 2021): a dummy if at least the annual liability was paid (col. 1), a dummy if some payment was made (col. 2), and the total amount paid (col. 3). Notably, in column 3 we also trim our sample by removing the top 1% of properties by annual liability. Each observation is a property and each model controls for randomisation strata fixed effects and baseline controls: (dummy if property paid liability in previous year (2019/20), property liability, a dummy for commercial properties, a dummy for other non-residential properties, distance to the city centre, total liability of property owner, a dummy if the property owner is a legal entity (not an individual), population and population density of the property's village, and the share of properties paying their liability in the previous year at both the village and parish level.). Standard errors in parentheses are clustered at the property owner level.

C Sub-treatment direct and knock-on effects by mode

In Table A11 we break down direct public disclosure effects by mode. We find no evidence that the direct effects of public disclosure (recognition or reporting) vary by whether disclosure will be online rather than SMS.

In Table A15 we breakdown knock-on public dissemination effects by mode and sample. Again, we find no evidence that the knock-on effects of public dissemination (Complier or Delinquent Lists) vary by whether the taxpayers disclosed are sample from the city as a whole, the local neighbourhood, or posted to a searchable online list.

Why is there no heterogeneity in terms of public disclosure mode, i.e. SMS or online lists - particularly for knock-on effects? We explore this further in our endline survey, where we ask taxpayers about messages they received over the course of the year. In general, there does not appear to be accurate recall regarding receiving a link to an online list - out of those taxpayers who received a message, 11% recall the message containing a link, but this 11% holds even when we focus on only those that were in a treatment group with a link. Further, only 15% of those claiming to have received a link report opening it. This suggests that the added effort and/or phone capacity needed to open online links limits the effectiveness of sending information in this way. Therefore, given that we find effects of similar magnitude for SMS and online lists, it could be that the treatment on the treated effects for online lists are much larger, or simply that many respondents pay similarly limited attention to the SMS lists.

D Conceptual framework extension with ambiguity

In the main conceptual framework we express the social cost of being perceived as a delinquent as z and implicitly the social cost of being perceived as a complier as zero. Here we extend this to allow for there to be a non-linear cost to perceived status. Specifically we assume that an agent can be perceived ambiguously as neither a complier nor a delinquent. We introduce new notation: z_d is the cost of being perceived as a delinquent occurring with the conditional probability q_{de}, z_c is the cost of being perceived as a complier occurring with the conditional probability q_{ce}, and a cost of 0 is incurred if the status is ambiguously perceived occurring with conditional probability $1 - q_{de} - q_{ce}$.

Treatment effects from equations 5 and 6 can be rewritten as:

$$\tau_S = \theta^*[x(1-\underline{p}) + z_d(1-\underline{pq_{d1}}) - z_c\underline{pq_{c1}}]F'(\underline{M}) \tag{10}$$

$$\tau_H = \theta^*[x(\underline{p}-1) + z_d\underline{pq_{d0}} - z_c(1-\underline{pq_{c0}})]F'(\underline{M}) \tag{11}$$

Note that if $1 = q_{de}+q_{ce}$, then these collapse back to equations 5 and 6. Again, we can use our empirical finding that $\tau_S+\tau_H = 0$ that gives $z_d(1-\underline{pq_{d1}}+\underline{pq_{d0}})-z_c(1-\underline{pq_{c0}}+\underline{pq_{c1}}) = 0$.Again, we rule out the edge case where $\underline{p} = 1$, $\underline{q}_{d1} = \underline{q}_{c0} = 1$, and $\underline{q}_{d0} = \underline{q}_{c1} = 0$ on the empirical grounds that our treatment effects are significantly different from zero. Otherwise the solution depends on the signs of the social costs.

Our preferred interpretation is that the empirical finding can be explained by $z_d = z_c = 0$, i.e. that there is no substantive shame cost to delinquency, nor pride gains from compliance. The empirical result that $\tau_S + \tau_H = 0$ rules out two intuitive cases of non-linearity in shame costs and pride gains. First, the case where the public imposes pro-compliance social costs (i.e., there is a positive shame cost to delinquency and a pride gain to compliance $z_d \geq 0$ and $z_c \leq 0$) only holds for our preferred interpretation with $z_d = z_c = 0$. Likewise, the second case where the public imposes anti-compliance social costs (i.e., where there is a social 'sucker' cost to being believed as a complier and a social gain to being known as a 'savvy' delinquent $z_c \geq 0$ and $z_d \leq 0$) also only holds when $z_d = z_c = 0$.

It is important to note that there are there are two cases of non-linearity that could alternatively explain our empirical results that, while we cannot rule out, seem less plausible. These cases exist when being believed as a complier or a delinquent are each better or each worse than having an ambiguous status (i.e. $z_d > 0$ and $z_c > 0$ or $z_d < 0$ and $z_c < 0$). This requires internal inconsistency on the part of the public, e.g. they impose social costs on both delinquents and compliers. While it may be inconsistent for a representative agent to impose social costs both on delinquents and on compliers (relative to those for which they have ambiguous beliefs about compliance), the public could be a mix of people who impose 'anti-compliance' social costs and others who impose

'pro-compliance' social costs. With this 'polarized public', an agent may face expected social costs that are non-monotonic in perceived status (e.g. complier and delinquent status both bear costs relative to an ambiguous status). While we cannot rule out this 'polarized public' scenario directly, we can turn to descriptive evidence from our endline survey. When asked whether they think that someone not paying the property rates they owe is wrong, the vast majority (79%) responded that it is 'wrong but understandable'. On the other hand, only 8% said it was 'not wrong at all' and only 14 % said it was wrong and punishable. Therefore, there does not seem to be a strong polarization of the public where some people hold pro-compliance beliefs and others anti-compliance beliefs. So while we cannot fully rule out this alternate scenario, our preferred (and simplest) interpretation is that there are no social costs to being perceived as a non-complier.

E Robustness checks using correct phone numbers

Of the individuals that we were able to reach by phone in our endline survey (6,303 out of 8,525 called), 87% had correct phone numbers according to the KCCA database - the person who had the phone was either the owner as listed in the database (77%), their heir (3%), or their property manager (7%). However, this signals a data quality issue whereby in 13% of cases, KCCA is not able to reach the relevant person responsible for paying taxes by their phone - and among these 13%, only 24% even knew of the owner. This may go some way to explain the limited effects we see of some of our treatments.

To investigate this we compare treatment effects in the sample with the correct phone number to the sample with incorrect phone numbers for both endline one (Table A13) and endline two (Table A17). In both tables we consider two different definitions for incorrect phone numbers: cols 1-2 use the sample of all properties that were called as part of the endline survey (so incorrect phone numbers include both numbers that did not pick up and respondents claiming not to be the correct number), and cols 3-4 use the sample of properties that responded to the phone call (so incorrect phone numbers are only those where respondents claimed to be the incorrect number). In columns 1 and 3 in both tables we re-estimate treatment effects on making any payment inside the sample where we have data on correct phone numbers. Starting with Table A13 in columns 1 and 3 we see that only the enforcement effects are significant in this sample. Turning to columns 2 and 4 we see that most of the positive enforcement effect comes from the sample of properties with the correct phone number, though only column 4 is significant at the 10% level. Moving to Table A17 in columns 1 and 3 we see that only the recognition effects are significant in this sample. Turning to columns 2 and 4 we see that most of the negative recognition effect comes from the sample of properties with the correct phone number, both significant at the 10% level. We can also see that most of the (imprecise) reporting effects can be attributed to the sample with the correct phone number. In summary,

incorrect phone numbers diminish the average intention to treat effects and correcting these numbers in the KCCA database could make messaging campaigns more effective.

of treatment effects, and determine the empirical relevance of different mechanisms. Finally, we conduct surveys at baseline and endline to provide direct evidence on taxpayer sentiments and beliefs and how they relate to these mechanisms.

Our experiment was designed in partnership with the Kampala Capital City Authority (KCCA) with two broad research aims in mind. First, to separately identify the effect of warning taxpayers that their behavior will be publicly disclosed ('direct effects') from the effect of publicly disseminating this information on the tax behavior of recipients ('knock-on effects'). To do so, we implement our experiment across two 'waves' (June 2020 and December 2020) and cross-randomize our treatments, so that the direct effects are tested in the first wave, and knock-on effects are tested in the second wave. Our second broad aim is to estimate the effects of publicly reporting delinquent taxpayers separately from the effects of publicly recognizing compliant taxpayers. To do so, we compare randomly assigned public disclosure sub-treatments that can they be either positive or negatively framed. In the first wave, taxpayers can be informed that their delinquency will be reported or their compliance recognized. In addition, we compare these treatments with different types of text message reminders from the literature (e.g., Brockmeyer et al. 2023, Cohen 2020, Mascagni & Nell 2022, De Neve et al. 2021, Collin et al. 2021, Okunogbe 2019). In the second wave, taxpayers can receive public reports of delinquents or recognition of compliers. In all cases treatments are administered by text message: in the first wave, treatment messages inform taxpayers whether they will be disclosed for delinquency, or for compliance; and in the second wave, treatment messages contain information about either past delinquents, or past compliers.

We outline a conceptual framework for understanding the effects of public disclosure policies on tax compliance that is motivated by three key potential mechanisms. First, the shame or pride in delinquency or compliance status. For instance, the threat of disclosing delinquents could have a positive impact on payment if individuals are ashamed to appear as reneging on their social obligations (Benabou & Tirole 2011).[4] On the other hand, norms may be such that they impose social costs on compliers.[5] Second, an incidental channel works through the publicity of tax eligibility status as a signal of wealth. If a cost is incurred by being revealed as tax-eligible (e.g., through informal or 'kin' taxes (Jakiela & Ozier 2015) or direct concerns for personal privacy or security.), then disclosing delinquents raises the cost of delinquency, while recognizing compliers raises the cost of compliance.[6] Third, public disclosure may also influence the taxpayers to whom the information is disclosed. Depending on prior beliefs, information about

[4]Similarly, recognizing compliers could have a positive effect on payment if there is esteem or social merit obtained from being known by others as having paid taxes.

[5]For instance, tax compliers may not want to appear as naive 'suckers' in a population of tax delinquents, or tax payments may be interpreted as support of an unpopular government.

[6]Opposing effects could exist if taxpayers derive value from signalling their wealth via their tax eligibility (Glazer & Konrad 1996).

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