Using reminders and nudges to increase property tax compliance in Dar es Salaam

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Abstract

Following a decade of centralization, decentralization and re-centralization of the responsibility to collect property tax revenue, the Tanzania Revenue Authority has struggled with high levels of non-compliance. In this paper, we experiment with a text-message campaign aimed at promoting compliance amongst a group of landowners in Dar es Salaam who had not paid any property tax two months prior to the annual deadline. We randomly treat taxpayers with one of three treatments: a text message reminding them to pay their tax, a message strengthening the connection between taxes and public services, and a social pressure message. Initial results indicate that the messages has a small, but significant impact on compliance: recipients of any message were one percentage point more likely to pay any property tax by the end of the study period. Recipients of the social pressure message were significantly less likely to make a payment, relative to that of a simple-reminder. Despite this compliance effect, overall levels of revenue sent in were similar across treatment arms, with the exception of the reciprocity treatment. While taxpayers in this treatment were just as likely to make a payment, they ultimately made larger payments to the TRA.

Keywords: Tax Compliance, Households, Communication Experiment

JEL classification: H26, H13 O17

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1 Introduction

Governments need enough revenue to sufficiently fund public goods and services. It is becoming clearer that the source of that revenue also matters. The states ability to raise tax revenue is thought to invite public scrutiny and strengthen the ‘social contract between citizen and state, leading to positive effects on institutional development and economic development (Besley and Persson 2013; Ali, Fjeldstad, and Katera 2017; Dincecco and Katz 2014). Despite the purported benefits of higher tax capacity, developing countries largely struggle to raise the same levels of tax revenue as their more developed peers. As of 2014, the tax-to-GDP ratio of the median low or lower-middle income country was 15.8%, compared to 20.5 for more developed countries. Evidence suggests that this may condemn some of these countries to lower levels of economic growth in the long term (Gaspar, Jaramillo, and Wingender 2016).

Aware of this, governments in developing countries are making efforts to improve their ability to collect more revenue, broaden their tax base and utilize a wider number of tax instruments. One of these is the taxation of immovable property, which presents an attractive source of revenue as it is in theory - both easier to target and tax in a non-distortionary way. But developing countries perform even worse in the collection of property tax than they do overall: where OECD countries bring in approximately 2.12% of GDP of revenue from property taxes, developing countries bring in only bring 0.6% (Norrregaard 2013). Some of this is due to a lack of clear information on ownership, but even in contexts where the governments have made progress in mapping out existing properties, overall tax compliance remains a problem.

The context of our study is Tanzania, which has historically struggled with low rates of tax revenue, both overall (11.78% of GDP) and for property tax (0.12% of GDP). Specifically, this project is focused Dar es Salaam, where officials have struggled with property tax collection for some time. Furthermore, the government has gone through a disruptive process of oscillating between a regime of decentralized revenue collection (where the local authorities are responsible for collecting property tax) and that of centralized collection (where the Tanzania Revenue Authority (TRA) is responsible) (Fjeldstad, Ali, and Katera 2017). This process has led to unstable and unpredictable levels of property tax revenues over the past decade. Furthermore, the Tanzanian government has struggled with overall low levels of compliance. This has been in part due to a lack of perceived reciprocity by taxpayers: property owners do not understand how the government will use their money (PO-RALG, 2013). It is also the result of a small tax base: while legally every property owner is obligated to pay tax, local authorities have previously prioritized those with larger properties living in the most affluent areas of the city (PO-RALG, 2013). It is within this context that the TRA is examining new ways to improve property tax compliance in the cities it is now responsible for.

In this context of rapidly-changing regimes and weak capacity to enforce, this paper
investigate the impact of ‘nudges’ on property tax collection in the city. Working with the Tanzania Revenue Authority (TRA) we randomly allocate a group of more than 200,000 individuals who are liable to pay property tax - but as of one month prior to the annual deadline had not paid any - into three main text message treatments: a simple reminder, a ‘reciprocity’ treatment emphasizing the link between tax revenue and publicly-provided goods, and a ‘social pressure’ treatment intended to highlight the non-cooperative nature of tax evasion. We find that all three treatment had a positive impact of both the propensity of taxpayers to make payments to the TRA and the total amount paid. We find some evidence that while those receiving the reciprocity treatment were no more likely to make a tax payment, the treatment resulted in higher amounts of tax paid overall. We also find some evidence that the social pressure message backfired, performing worse than a simple reminder. We show a significant amount of heterogeneity in the treatment effects across the geography of the city, with areas that a higher rate of control group compliance showing lower treatment effects, suggested that nudges may be more successful in low-compliance areas.

2 Background

Until recently, economists have viewed tax compliance largely through the lens of enforcement (Allingham and Sandmo 1972) where taxpayers increase their compliance when the perceived probability of detection goes up.

There is evidence that letters and electronic forms of communication have the potential to do this: research in many advanced economies suggests that letters containing an implicit or explicit threat of audit increases tax payments (Coleman 1996; Blumenthal et al. 2001; Hasseldine et al. 2007; Kleven et al. 2011; Fellner et al. 2013; Castro and Scartascini 2015; Hallsworth et al. 2017; Pomeranz 2015; Meiselman 2018; Hernandez et al. 2017) although the effect sizes vary across contexts and are sometimes insignificant (Ariel 2012). Work in developing countries has largely revealed similar results (Ortega and Scartascini 2015; Brockmeyer, Hernandez, Kettle, and Smith 2016; Kettle, Hernandez, Ruda, and Sanders 2016), although recent evidence from Rwanda suggests that less aggressive messages (such as reciprocity or reminder-framed messages) work slightly better than those aimed at deterrence (Mascagni, Nell, and Monkam 2017).

In recent years, economists have extended the Allingham and Sandmo model to include the concept of “tax morale,” a bundle of mechanisms which explain voluntary tax compliance. Recent experiments have attempted to make these components of tax morale more salient through careful messaging, with mixed results. The only such natural field experiment to have taken place in a developing country, Kettle et al. (2016), finds that both letters with national pride and those with social norm messages do improve compliance in Guatemala, but not discernably more than a letter invoking a heightened probability of audit. In richer countries, randomized studies of letter/e-mail campaigns typically find
that attempts to emphasize the social contract or civic duty either have little impact or
are marginally effective (Coleman 1996; Blumenthal et al. 2001; Torgler 2004; Ariel 2012;
Fellner et al. 2013; Castro and Scartascini 2015; Meiselman 2018) with some exceptions
(Hallsworth et al. 2017).

3 Experiment and data collection

3.1 Baseline data and randomization

Our frame for the experiment is a list of 241,200 properties for which, as of June 1st, 2018,
no property tax had been paid to the TRA. The deadline for property tax payments to be
completed was June 30th. After June 30th had passed, the TRA extended the deadline
for another two weeks.

As some taxpayers own multiple parcels, we collapse this data to the taxpayer level
(237,699 taxpayers), as indicated by the taxpayer ID associated with the property. We
use two sources of information in the randomization: the location of the property and
whether or not the property had been served a ‘demand notice’ at the time the data was
collected. The location of the property is the lowest level of administration in Dar es
Salaam, the sub-ward of ‘mtaa’ level. We assign taxpayers the same location as their
property. When taxpayers have multiple properties that span more than one subward,
we pick the modal subward. Where there is no modal subward, we randomly choose one
of those subwards to assign to the taxpayer.

Demand notices are bills issued by the TRA to landowners. Approximately 19% of
the sample had been issued a bill at the time of the data collection. We stratify on these
two characteristics: location and whether the property had been issued a demand notice.

3.2 Treatments

We randomized each property owner into one of five groups.\footnote{The randomization was conducted using the Stata command \texttt{randtreat}, with misfits being dealt with using the \texttt{strata} method, which randomly allocates misfits across all strata (Carril et al. 2017).}

The treatments are summarized below in Table 1. The first group (control) was not to
receive any message from the TRA. Group T1 received a simple message reminding them
to pay their property tax, indicating the due date (June 30th) and providing information
the taxpayers could use to contact the TRA in case they had any questions. All other
treatments included this reminder message.

Treatment 2 was an enforcement message which included the simple statement “Pay
your rent early to avoid penalties.” Treatment 3 was a ‘reciprocity’ treatment, where
taxpayers were reminded that taxes fund social services and infrastructure and finished
with the TRA’s slogan “Together we build our Nation.” The final treatment was a
‘social pressure’ treatment in which taxpayers were reminded, in a negative fashion, that
### Table 1: Treatment arms and treatment assignments

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Type</th>
<th>Message</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control</td>
<td>No message</td>
<td>“Dear brother, TRA reminds you to pay your rent tax. Pay before 30th June. For more information: dial * 152 * 00 #, visit your nearest TRA office or call 0800780078. Thank you.”</td>
<td>47,555</td>
</tr>
<tr>
<td>T1</td>
<td>Reminder</td>
<td>“Dear brother, TRA reminds you to pay your rent tax. Pay before 30th June. For more information: dial * 152 * 00 #, visit your nearest TRA office or call 0800780078. Thank you.”</td>
<td>46,985</td>
</tr>
<tr>
<td>T2</td>
<td>Enforcement*</td>
<td>[Reminder] + “Pay your rent early to avoid penalties”</td>
<td>47,049</td>
</tr>
<tr>
<td>T3</td>
<td>Reciprocity</td>
<td>[Reminder] + “Your tax facilitates access to social services and infrastructure. Together we build our Nation”</td>
<td>47,064</td>
</tr>
<tr>
<td>T4</td>
<td>Social Pressure</td>
<td>[Reminder] + “Non-taxpayers are not contributing to national development and thus restoring the development of their communities. Please pay us our own.”</td>
<td>47,076</td>
</tr>
</tbody>
</table>

*Note: Subjects randomly allocated to T2 were accidentally sent T1. See sub-section 3.3 for details.

Non-compliers were not contributing to the development of the country or their own communities.

### 3.3 Implementation and challenges

Following the randomization, a list of taxpayers, including phone numbers (included in the original TRA dataset were) were provided to the TRA. The majority of messages were sent out after June 20, fewer than ten days before the initial deadline to pay property tax. While there was overlap in the delivery of different treatments, completion of each treatment arm proceeded sequentially, with reminder messages being sent first, reciprocity messages second, etc. Figure 1 displays the timing of messages sent by the TRA during this period to all taxpayers in Dar es Salaam (including, but not limited to our sample). It is possible that the delayed release of the Social Pressure message may have led to a decline in its effectiveness, but evidence we will present later in the paper suggests this is not the case.

There were two errors in the message delivery. First, the firm in charge of sending the messages sent Treatment 1 messages to taxpayers who had been randomly allocated to Treatment 2 (Enforcement), essentially doubling the size of the first treatment arm. Second, instead of using a list of cleaned and prepared phone numbers they were provided with, the firm chose an unformatted list which contained the same numbers, but in some cases were not usable due to missing pre-fixes or county codes that were included. These were instead allocated to Treatment 1 (Reminder). Thus between 22-33% of each treatment arm was not sent the intended message. Using data from the text message delivery, we can account for which taxpayers were or were not sent a message, and use this information to account for which taxpayers were never sent a message.

Finally, the randomization was conducted at the taxpayer identification level. But
Figure 1: Timeline of message delivery

Note: Graph shows the cumulative proportion of messages sent (out of messages sent to all taxpayers, not just those in our experimental sample) over time.

Table 2: Frequencies of actual treatment, by treatment group

<table>
<thead>
<tr>
<th>Treatment Arm</th>
<th>Received No Message</th>
<th>Received T1</th>
<th>Received T3</th>
<th>Received T4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control</td>
<td>90.32%</td>
<td>5.06%</td>
<td>2.53%</td>
<td>2.24%</td>
</tr>
<tr>
<td>T1 + T2</td>
<td>9.92%</td>
<td>81.71%</td>
<td>4.61%</td>
<td>3.76%</td>
</tr>
<tr>
<td>T3</td>
<td>4.45%</td>
<td>11.53%</td>
<td>77.65%</td>
<td>6.37%</td>
</tr>
<tr>
<td>T4</td>
<td>13.14%</td>
<td>12.15%</td>
<td>7.49%</td>
<td>67.22%</td>
</tr>
</tbody>
</table>

a small subset of taxpayer IDs shared identical phone numbers. This is likely because some taxpayers were issued multiple taxpayer IDs, or households sharing a single number contained multiple taxpayers. This lead to spillovers in actual treatment between the various treatment arms and the control group. The actual frequency of treatment across these groups are summarized below in Table 2.

3.4 Outcome data

Out data on outcomes was retrieved from the TRA at the beginning of August, 2018. We merge the complete record of all property tax payments made for a given taxpayer ID between June and the beginning of August to our original experimental sample. For each taxpayer ID we record, for each date during this period, whether any payments associated with that ID had been made up to that date as well as the cumulative amount of payments made so far.
4 Results

For most of the main results, we display the results from one of two specifications:

\[ P_i = \alpha + \beta \times treated_i + \epsilon \] (1)

and

\[ P_i = \alpha + \beta_1 T1_i + \beta_3 T3_i + \beta_4 T4_i + \epsilon \] (2)

Where \( P_i \) is alternatively whether the taxpayer has paid anything to the TRA or the amount the taxpayer has paid. \( treated_i \) is a dummy variable equal to one if the taxpayer was randomized into any of the three treatment groups. \( T1_i, T3_i \) and \( T4_i \) are dummy variables equal to one if the taxpayer was randomized into the reminder, reciprocity or social pressure treatments, respectively. Unless otherwise specified, we run both specifications (1) and (2) with strata fixed effects.

Figure 2 shows the intent-to-treat (ITT) coefficient estimates from specification (1) when the outcome is whether the taxpayer has paid anything, measured at different points of time during the experiment. As can be seen, prior to the introduction of the text messages, treated taxpayers had the same propensity to have made a payment to the TRA as an untreated taxpayer. Only following the introduction of the text messages do we see a difference open up. By the end of the period we have administrative data for, those randomized into a message treatment were approximately 1.1 percentage points more likely to have made a payment to the TRA, over a baseline of approximately 10%.

Figure 3 shows the average payment rates for the control group and each treatment group over the course of the study. Table 3 shows the results of running specification (2) when the outcome is whether the taxpayer has paid anything, measured at different points of time during the experiment. Column (1) shows the results when we consider payments made at the start of the study period (the beginning of June). Column (2) considers payments made up to the point where the very first text messages were sent out. Columns (3), (4) and (5) consider payments made by the June 30th deadline, the two week extension, and the end of the study period (the beginning of August).

As can be seen in the table, the ITT effects of the text message campaign only manifest after messages had been sent. The differences emerge by the time of the first tax deadline (column (3)) with both reminder and reciprocity treatments increasing payment rates by approximately 1 percentage points and the social pressure treatment increasing payment rates by roughly half a percentage point. At the bottom of each column we present test of equality of these coefficients: that both the reminder and reciprocity treatments have a larger effect than the social pressure treatment. These differences continue as we move to the end of the study period, at which point the reminder, reciprocity and social pressure ITT effects are 1.1, 1.3 and 7.9 percentage points, respectively.
Figure 2: Timeline of ITT effect of all messages on payment rates

Note: Graph shows the pooled effect of being randomized into one of the treatment groups over time. Outcome is the proportion of taxpayers who have made any payments to the TRA.
Table 3: Impact of message assignment on payment rates

<table>
<thead>
<tr>
<th></th>
<th>(1) Start</th>
<th>(2) First message</th>
<th>(3) First tax deadline</th>
<th>(4) Second tax deadline</th>
<th>(5) One month after first deadline</th>
</tr>
</thead>
<tbody>
<tr>
<td>T1: Reminder</td>
<td>-0.000228+</td>
<td>-0.000348</td>
<td>0.00930***</td>
<td>0.0117***</td>
<td>0.0114***</td>
</tr>
<tr>
<td></td>
<td>(0.000123)</td>
<td>(0.000442)</td>
<td>(0.00143)</td>
<td>(0.00161)</td>
<td>(0.00171)</td>
</tr>
<tr>
<td>T2: Reciprocity</td>
<td>0.0000409</td>
<td>-0.0000904</td>
<td>0.00977***</td>
<td>0.0126***</td>
<td>0.0130***</td>
</tr>
<tr>
<td></td>
<td>(0.000157)</td>
<td>(0.000514)</td>
<td>(0.00167)</td>
<td>(0.00188)</td>
<td>(0.00200)</td>
</tr>
<tr>
<td>T3: Social pressure</td>
<td>-0.000210</td>
<td>0.0000723</td>
<td>0.00513**</td>
<td>0.00824***</td>
<td>0.00792***</td>
</tr>
<tr>
<td></td>
<td>(0.000139)</td>
<td>(0.000517)</td>
<td>(0.00164)</td>
<td>(0.00186)</td>
<td>(0.00198)</td>
</tr>
</tbody>
</table>

R² 0.011 0.010 0.037 0.042 0.044
Obs 237262 237262 237262 237262 237262
Test: T1 = T3 0.035 0.558 0.754 0.592 0.383
Test: T1 = T4 0.868 0.343 0.004 0.036 0.045
Test: T3 = T4 0.078 0.752 0.006 0.023 0.013

Standard errors in parentheses
Dependent variable is a dummy = 1 if the taxpayer has made any payment to the TRA by the given date

+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001
4.1 Message delivery and payment rates

As described above, due to implementation problems, not everyone assigned to a treatment group received a message. In column (1) of Table 4, the ITT results from the last column of Table 3. In column (2), we interact treatment assignment with a dummy variable equal to one if we have a record of that taxpayer actually being sent a message reflecting that treatment. We find that the effect of treatment assignment only survives when this is the case - that taxpayers who were assigned to a treatment, but for whom there is no record of a message being sent, are no more likely to have made a tax payment during the study period. We then estimate the LATE effects of the treatments by instrumenting message receipt with randomization into each treatment. Column (3) reports these results - effect sizes are larger for each treatment, commensurate with the relative probability of treatment. The reminder, reciprocity and social pressure LATE effects are 1.4, 1.5 and 9.5 percentage points, respectively.

4.2 ITT effects and payment amounts

In Table 5, we present the results of specification 2, using the total amount of money paid to the TRA as an outcome, using a poisson regression as our outcome is largely censored at zero. Here we find that when we consider the intensive margin of the amount of tsh that are paid to the TRA, the reciprocity treatment slightly outperforms the other
Table 4: Message receipt and payment rates

<table>
<thead>
<tr>
<th></th>
<th>(1) ITT</th>
<th>(2) ITT interacted with message receipt</th>
<th>(3) IV</th>
</tr>
</thead>
<tbody>
<tr>
<td>T1: Reminder</td>
<td>0.0114***</td>
<td>-0.000458</td>
<td>(0.00171)</td>
</tr>
<tr>
<td>T2: Reciprocity</td>
<td>0.0130***</td>
<td>-0.000428</td>
<td>(0.00200)</td>
</tr>
<tr>
<td>T3: Social pressure</td>
<td>0.00792***</td>
<td>-0.00139</td>
<td>(0.00198)</td>
</tr>
<tr>
<td>T1 X T1 message sent</td>
<td>0.0145***</td>
<td>0.01453</td>
<td>(0.00251)</td>
</tr>
<tr>
<td>T3 X T3 message sent</td>
<td>0.0172***</td>
<td>0.01723</td>
<td>(0.00328)</td>
</tr>
<tr>
<td>T4 X T4 message sent</td>
<td>0.0139***</td>
<td>0.01393</td>
<td>(0.00294)</td>
</tr>
<tr>
<td>T1 message sent</td>
<td>0.0143***</td>
<td>0.01433</td>
<td>(0.00218)</td>
</tr>
<tr>
<td>T3 message sent</td>
<td>0.0155***</td>
<td>0.01553</td>
<td>(0.00251)</td>
</tr>
<tr>
<td>T4 message sent</td>
<td>0.00945***</td>
<td>0.00945</td>
<td>(0.00285)</td>
</tr>
</tbody>
</table>

R²                         0.044  0.044  0.044
Obs                        237262 237262 237262
Test: T1 = T3              0.383  0.622
Test: T1 = T4              0.045  0.070
Test: T3 = T4              0.013  0.052
Test: (T1 X T1 sent) = (T3 X T3 sent) 0.178
Test: (T1 X T1 sent) = (T4 X T4 sent) 0.446
Test: (T3 X T3 sent) = (T4 X T4 sent) 0.076

Standard errors in parentheses
Dependent variable is a dummy = 1 if the taxpayer has made any payment to the TRA by the given date

+ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001
treatments by the end of the study period.

4.3 External validity and the geographic spread of effects

As described above, the randomization was conducted within 1,211 different geographic strata, based on the reported location of the property within the TRA. This allows us to compare effect sizes across these different strata, to see if any systematic relationships appear.

For this analysis we drop strata with fewer than 100 observations (roughly 8% of the total experimental sample), leaving us with 413 strata. For each strata, we run a regression of the above form (equation X), where the outcome is an indicator equal to one if the taxpayer made any payment to the TRA during the study period. We then recover both the coefficient $\beta$ and the constant $\alpha$ from each regression. The top half of Figure 4 graphs (i) the distribution of those effect sizes across the 413 strata, (ii) the distribution of the constants (what we refer to as control group compliance). As we can see here, there is a substantial amount of variation in effect sizes across strata. To give a sense of the implications for comparing small-scale experiments to city-wide ones: if the experiment had been conducted in a randomly-chosen strata, the chance the resulting effect size would have been within .01 percentage points of the ‘true’ effect estimate is less than 17%.

The bottom half of Figure 4 compares the estimated distributions of control group compliance ($\alpha$) and treatment group compliance ($\alpha + \beta$) across strata. As can be seen in the bottom left graph, the distribution of treatment group compliance is shifted up and to the right of control group compliance. As can be seen in the scatterplot in the bottom right, the effect sizes is inversely proportional to control level compliance: the treatment seems to be more effective in areas that have a higher ‘baseline’ rate of non-compliance.

References


Table 5: Poisson regressions: impact of message assignment on payment amounts

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Start</td>
<td>First message</td>
<td>First tax deadline</td>
<td>Second tax deadline</td>
<td>One month after first deadline</td>
</tr>
<tr>
<td><strong>T1: Reminder</strong></td>
<td>-0.621</td>
<td>0.0801</td>
<td>0.899</td>
<td>1.125</td>
<td>1.008</td>
</tr>
<tr>
<td></td>
<td>(0.471)</td>
<td>(0.263)</td>
<td>(0.667)</td>
<td>(0.721)</td>
<td>(0.695)</td>
</tr>
<tr>
<td><strong>T2: Reciprocity</strong></td>
<td>0.0172</td>
<td>0.626</td>
<td>1.275</td>
<td>1.564⁺</td>
<td>1.399⁺</td>
</tr>
<tr>
<td></td>
<td>(0.383)</td>
<td>(0.529)</td>
<td>(0.803)</td>
<td>(0.843)</td>
<td>(0.829)</td>
</tr>
<tr>
<td><strong>T3: Social pressure</strong></td>
<td>0.0397</td>
<td>0.480</td>
<td>0.915</td>
<td>1.218⁺</td>
<td>1.013</td>
</tr>
<tr>
<td></td>
<td>(0.446)</td>
<td>(0.303)</td>
<td>(0.673)</td>
<td>(0.729)</td>
<td>(0.699)</td>
</tr>
</tbody>
</table>

R²

<p>| | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Obs</td>
<td>237370</td>
<td>237370</td>
<td>237370</td>
<td>237370</td>
<td>237370</td>
</tr>
<tr>
<td>Test: T1 = T3</td>
<td>0.087</td>
<td>0.290</td>
<td>0.106</td>
<td>0.024</td>
<td>0.065</td>
</tr>
<tr>
<td>Test: T1 = T4</td>
<td>0.236</td>
<td>0.158</td>
<td>0.898</td>
<td>0.447</td>
<td>0.969</td>
</tr>
<tr>
<td>Test: T3 = T4</td>
<td>0.965</td>
<td>0.787</td>
<td>0.119</td>
<td>0.084</td>
<td>0.065</td>
</tr>
</tbody>
</table>

Standard errors in parentheses
Dependent variable is a dummy = 1 if the taxpayer has made any payment to the TRA by the given date
⁺ p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001
Figure 4: Distribution of effects and compliance rates across strata

Note: Each observation is either the coefficient or the constant from a strata-specific regression of whether a household has made any payments to the TRA during the study period on a dummy = 1 if the household was randomized into a message treatment. Sample includes all strata with at least 100 observations.


Fellner, G., R. Sausgruber, and C. Traxler (2013). Testing enforcement strategies in


