

Can an Information Campaign about Taxation and Service Delivery Increase Tax Compliance?

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Abstract

Can information about the linkage between tax payment and public service delivery inspire citizens to pay taxes and improve relations between citizens and the government? To answer this question, we conducted an information campaign in Zomba, Malawi, a city that suffers from chronic underpayment of property taxes. In the context of a shock to public service delivery (i.e., increased waste collection), we told homeowners that continued service delivery depended on payment of their property taxes. Although homeowners exposed to the information campaign gained knowledge about property taxes and city service delivery, we see no evidence of increased tax payments in administrative data or of increased intent to make tax payments in survey data. We diagnose these null results by looking at the implementation of the intervention and spillover from treatment to control units, finding some evidence that heterogeneous effects and spillover may have contributed to the overall null findings.

Keywords: taxation, tax compliance, service delivery, field experiment, Malawi

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1 Introduction

Many political jurisdictions experience a vicious cycle in which too few revenues are collected and too few services are provided: when citizens do not pay taxes, governments cannot provide services; when governments do not provide services, citizens may perceive little reason to pay taxes (Schmelzle and Stollenwerk 2018). For example, in the city of Zomba, Malawi, demand for city services is high. The city, however, can only provide limited services due to persistent non-payment of property taxes, the main form of municipal revenue in Zomba. This lack of service delivery motivates many citizens to ignore their tax obligation, further decreasing city resources and the city's capacity to provide services.

We worked with the city of Zomba to simultaneously provide a shock to city service delivery (i.e., an increase in waste collection) and an information campaign linking tax payments with continued service delivery. The door-to-door information campaign, which we assigned to randomly selected households, also reduced barriers to tax payment by teaching citizens how to make payments and highlighting the existence of an amnesty program for delinquent taxpayers. Zomba City Council deployed the new waste collection services across underserved city wards.¹ In this paper, we examine survey responses and administrative data to learn if the information campaign increased citizens' desires to formalize their relationship with the city government and become taxpayers in good standing.

We find little evidence that the information campaign had the desired results. Although citizens exposed to the information campaign become more aware of their obligation to pay property taxes, they are no more likely to say that they intend to pay them, to actually have paid them, or to say that they expect to pay them in the future. We also find the information campaign does not improve satisfaction with the government; nor does it increase perceptions of government legitimacy or capacity.

To diagnose these null results, we identify heterogeneity in the way that the information campaign was implemented and divide the information campaign treatment into three different types. We find indicative evidence of more positive effects where the information campaign was delivered following the provided script and negative effects when the campaign was delivered without following the provided script. We also find preliminary evidence of spillover under both types of treatment implementation.

Although our study is one of the few in the literature that examines a tax that funds local service delivery, our overall results confirm existing findings that simple information campaigns linking taxes to direct benefits are not, by themselves, effective at increasing tax compliance.

2 What Encourages Tax Compliance?

The Allingham and Sandmo (1972) model of tax compliance proposes that individuals consider non-compliance with taxation as a risky lottery with payoffs determined by their income, the tax rate, the probability of an audit, and the size of a fine in the event that non-compliance is detected. Citizens can gamble between getting away with paying less in taxes or else ending up sanctioned. In line with this model, a large number of field experiments have shown that an increase in audit probabilities leads to an increase in tax compliance (see the review in Hallsworth 2014). Nonetheless, for empirically realistic levels of auditing and typical fines, the model predicts lower levels of compliance than commonly observed. This unexpectedly high compliance has led to the literature on taxation reframing the puzzle of taxation from "Why would anyone pay?" to "Why do so many people pay?"

One line of research argues that high levels of compliance might arise from a tendency to overestimate the probability of being caught (Alm, McClelland, and Schulze 1992). Other scholars have incorporated additional characteristics of the environment into the citizen's decision making calculus, proposing that levels of trust in the taxing authorities (Levi 1988; Smith 1992; Kirchler 2007; see also Tyler 2006; Levi, Sacks, and Tyler 2009), social norms surrounding compliance (Traxler 2010), and subjective evaluations of the fairness of the tax system (Smith 1992; Steenbergen, McGraw, and Scholz 1992) might also play a role in willingness

¹This aspect of the intervention was not randomized.

to obey tax laws.

Another line of research asks if citizens' tax compliance is influenced by what the government does with tax revenue (Cowell and Gordon 1988). Timmons (2005), for instance, argues that states have an incentive to trade services for revenue, given the costly nature of extracting taxes through force.² Our study aims to learn more about whether citizens think in terms of such a reciprocal exchange and if framing taxation as a trade for services can improve tax compliance.

The existing literature is inconclusive about whether framing taxation as a trade for services improves tax compliance or if citizens even think in terms of such an exchange. Blumenthal et al. (2001) sent a letter to taxpayers in Minnesota, describing the allocation of tax revenue across state services and advising citizens, "So when taxpayers do not pay what they owe, the entire community suffers." They do not find substantively meaningful differences in tax payments between those who received the letters and a control group. Torgler (2004) studies the effects of a similar letter in Switzerland, where citizens were told that their community "would suffer greatly" without taxpayers contributing their fair share and that taxes "help keep [the city of] Trimbach attractive for its inhabitants." The letter did not change tax compliance. Torgler (2013) replicates this result. In a property tax context similar to the one we study in Malawi, Castro and Scartascini (2015) provide information to Argentine citizens about how local property taxes have been used in the past year. This information does not change tax payment behavior, whereas, in line with experiments that manipulate people's perceptions of the costs of tax evasion, information about the schedule of fees for non-compliance increases tax payments.

On the other hand, in a laboratory experiment where the experimenters provide a multiplier to tax revenues (meant to represent the public goods that can be funded by tax revenues), Alm, McClelland, and Schulze (1992) show that increasing payoffs from tax payments leads to increasing compliance. Looking at observational data from urban Nigeria about perceived service delivery and tax morale, Bodea and LeBas (2016) find that individuals with more positive experiences of state service delivery express stronger beliefs in citizen obligations to pay tax. Those authors also find that these pro-compliance norms diminish where non-state actors substitute for state service delivery. In a context that parallels the one we study, Gonzalez-Navarro and Quintana-Domeque (2015) provide evidence that new asphalt paving in residential neighborhoods in a Mexican city (i.e., new city service delivery) increased property tax compliance rates.

Most of the literature cited above studies taxpayers in wealthy and middle-income countries. Our study contributes to this literature by working in a low-income country context, where we expect that — in line with the findings in Gonzalez-Navarro and Quintana-Domeque (2015) — a visible shock to service delivery in conjunction with a message reinforcing the link between tax payments and service delivery might increase citizens' willingness to pay taxes.

3 Tax Compliance in Zomba, Malawi

Zomba, Malawi, is a city of around 100,000 people, located in the southern part of the country. The fourth largest city in the country, it was the capital city of Malawi from independence in 1964 until 1974 and the seat of parliament until 1994. It remains the administrative capital of Malawi's Eastern region. Like many other cities in the developing world, Zomba struggles with service delivery and tax collection. Few people pay taxes, and the local government can provide few services. Functioning essentially as a "budget-to-budget" government, Zomba is locked into a vicious cycle where services are not provided because taxes are not paid, plausibly because services are not provided.

Zomba relies primarily on locally-raised revenue. To this end, Zomba requires that every homeowner pay a property tax (known locally as a "city rate").³ In planned areas of the city, households are required to pay 0.25% of their property value twice per year; in unplanned areas, households owe a flat rate of between

²A related literature discusses the linkages between taxation and representation (e.g., Bates and Lien 1985; Ross 2004).

³The Zomba City Council also generates revenue through market fees and license fees and through specialized service provision like septic tank emptying. Property taxes are the primary revenue source.

3,000 and 10,000 MWK (US\$4 to \$14) twice per year, depending on the size of the house. These tax revenues finance waste collection, street lighting, road maintenance, and other city services. The city concentrates its provision of public services in the areas of the city where people tend to pay property taxes. Despite this connection and despite valuing these services, many households in the city do not pay their property taxes, leaving certain city wards underserved or unserved. The lack of services has, in turn, effected the public health and quality of life for the thousands of people living in those wards.⁴

Zomba City Council generally succeeds in collecting property taxes in the wealthier wards of the city, where it can more easily identify (and therefore punish) property owners. Other wards contain densely packed informal housing, which makes it difficult for the government to tax the citizens: it is a challenge to find houses built without formal property rights, to assess property values in such places, or to focus on individual homeowners for punishment when entire neighborhoods are evading payment. In our survey area, the median monthly income is 45,000 MWK (about US\$60). A tax payment of 10,000 MWK is a significant expense for such a household.

As a result of the challenges of collecting taxes in the more informal settlements in the city, the government has prioritized expanding the tax base by incorporating homeowners in new parts of the city where wealthy and middle-class individuals build houses that are legible to the tax authorities. Similarly, the city's enforcement efforts have typically focused on extracting owed taxes from a few easily identifiable taxpayers rather than campaigning to obtain new revenues from harder-to-identify taxpayers in informal areas. The city's strategy for collecting revenues from delinquent taxpayers involves the locking people out of their houses until remaining debts are paid off.

We began conversations with representatives from Zomba City Council in 2016 about the possibility of combining increased service delivery with an information campaign about the linkages between tax payment and continued service delivery. Zomba City Council identified improved waste management (i.e., regular trash pick-up) as a city service that citizens wanted and from which they would benefit.⁵

In 2017, we partnered with the Invest in Knowledge Initiative (IKI), a Zomba-based research institute, to conduct background research. IKI conducted exploratory research in the neighborhoods that Zomba City Council identified as lacking waste management services. The research team documented the existence of trash piles in these neighborhoods and used interviews to confirm that local residents perceived waste management as an important problem for the city to solve. In our baseline survey (N=1,806), conducted in November – December 2018, only around 2.4 percent of respondents reported that they had access to government trash collection, and a majority of respondents without access to trash collection said that they were (completely) unsatisfied with their current method of trash disposal.

We visited Zomba in June 2018 and through continued conversations with Zomba City Council and local citizens, we identified a suite of four reasons why some residents do not pay property taxes, each of which could be ameliorated by our intervention. We describe each of these reasons in turn. Figure 1 reports descriptive data from the baseline survey that validates our initial impressions.

Lack of information. Many residents, particularly in unplanned areas, were simply not aware that they owed property taxes at all, nor did they know how much they needed to pay. In our baseline survey, only 40 percent of respondents said that they thought the law required them to pay property taxes.⁶ Citizens did not draw a connection between the payment of property taxes and the provision of public services like waste collection. Simply providing basic information to people through an information campaign, therefore, might plausibly increase tax compliance.

Barriers to payment. Some residents who were aware of the property taxes identified that they

⁴The average ward in Zomba has roughly 10,000 inhabitants.

⁵Our baseline survey, described below, corroborated this perception of demand for waste services.

⁶This proportion varied across areas of the city in a way that correlates with average neighborhood wealth. In the poorest wards in our study, only around one-third of respondents thought that they were required to pay property taxes, whereas in the wealthiest ward in our study, almost 60 percent of respondents said that they were required to do so.

needed to go to the city center to pay property taxes and suggested that this often involved waiting a long time in line in order to make the payment. As they were able to make easier payments for other goods and services, they viewed this process as onerous. In our baseline survey, over 70 percent of respondents said that paying property taxes was (very) difficult. Providing information about alternative means for making tax payment might increase tax compliance.

Overdue payments. Many residents who are on the tax rolls have overdue payments for previously assessed property taxes. In our baseline survey, a full 80 percent of respondents said that they had not paid property taxes the last time they were due.⁷ If citizens fear penalties and fines and lack the resources to pay their overdue assessments, then past lack of payment increases the probability that they will avoid payment in the future. Although Zomba City Council reports willingness to work with delinquent tax payers to structure their payments in a way that helps them come into compliance, over 90 percent of our respondents in the baseline survey said that they were unaware of the city's tax forgiveness plan. Providing information about opportunities for paying overdue taxes may help bring citizens into a regular, formal relationship with the city.

Lack of service provision. Some residents were skeptical that Zomba City Council would provide public services even if every citizen paid property taxes. Over 40 percent of baseline respondents not currently paying their property taxes thought that it was (very) unlikely that either they personally or their neighborhood would start benefiting from city services if they were to begin paying their taxes. This lack of trust in the city government is not conducive to creating a stable compliance relationship with the city. Information about the ability of the city to provide services might increase trust that services could, in fact, be provided if citizens paid property taxes.

⁷Once again, there was variation across the city. In one ward, only six out of 395 respondents reported having paid (1.5 percent), whereas in the wealthiest ward in our study, 45 percent of respondents reported having paid.

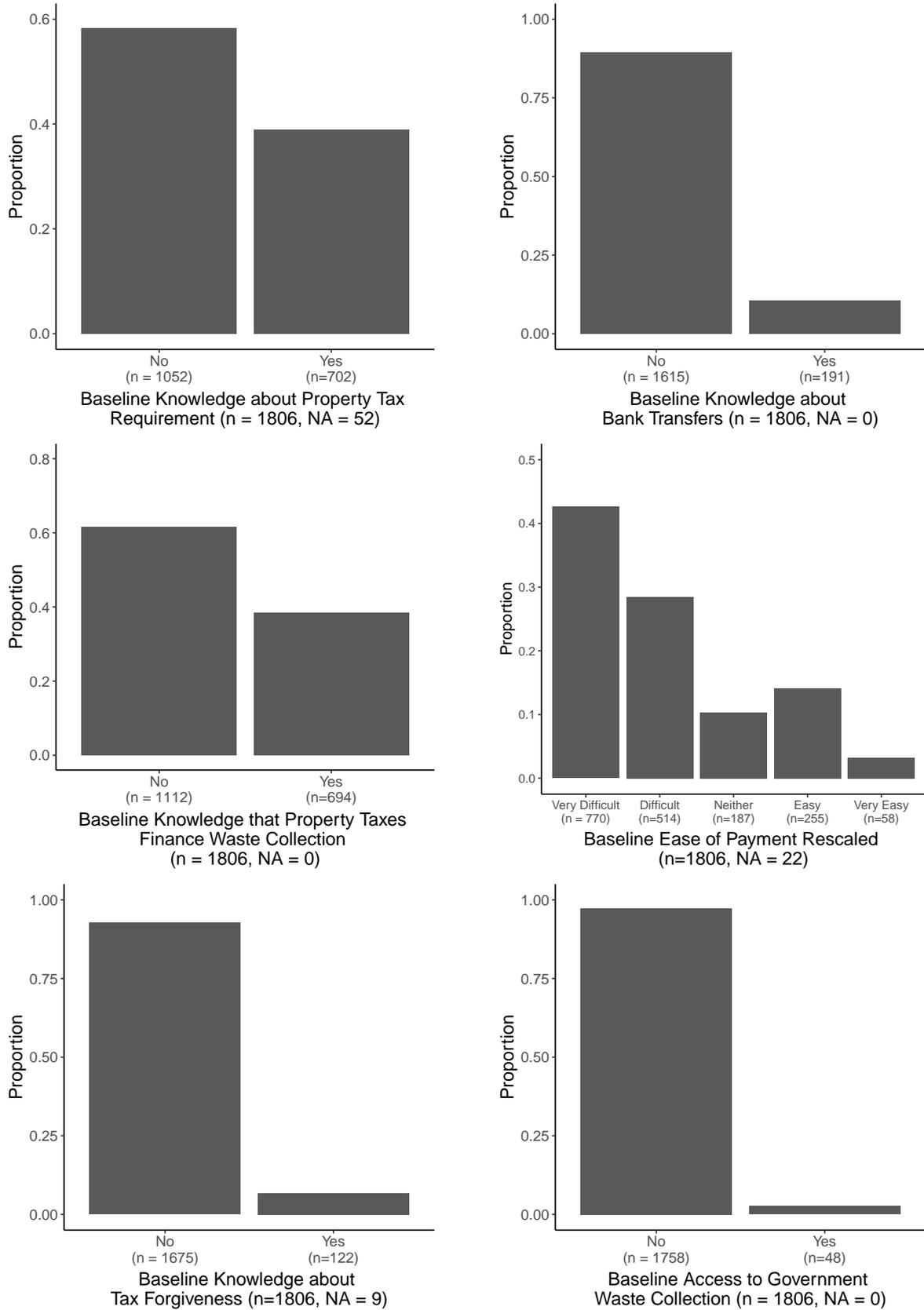


Figure 1: Possible reasons for lack of property tax payment. Data from baseline survey conducted in November-December 2018. Data includes respondents from "pure control" neighborhoods.

4 Intervention

Our formative research and baseline survey suggested that information about tax payments and proof of service provision capability might change the way that citizens thought about tax compliance in Zomba.⁸ We incorporated responses to the four obstacles into an information campaign conducted in the context of increased city-wide service delivery. We first describe the expansion of service delivery and then the information campaign.

We expanded waste collection service in Zomba using a new “skip” system. A skip container is a portable dumpster that can be transported and emptied by a specialized truck called a skip loader. Working directly with Zomba City Council, we placed eight new skip containers in the city, primarily in unplanned areas that had experienced no formal governmental waste services to date. We chose the specific locations in order to minimize the distance between citizens and skips and to serve areas that were accumulating waste.⁹

At the same time that this new service delivery began, we launched an information campaign in which door-to-door campaigners who described themselves as coming on behalf of Zomba City Council visited randomly selected households and left an informational brochure with the household after discussing the contents of the brochure and drawing attention to the new skips (see Figure 2).¹⁰ The informational brochure, which was designed in collaboration with Zomba City Council, primarily focused on the linkages between property taxes and city service provision. By doing so, the brochure also informed households about the existence of the property taxes and their responsibility for paying them.¹¹ To address the issue of barriers to payment, the campaign script and the brochure contained information about the possibility of using bank transfers to pay the property taxes. To address the problem of overdue payments, the campaign script and the brochure contained information about Zomba City Council’s tax forgiveness plan.



Figure 2: Information campaign brochure. English-language version.

⁸We designed the intervention and our measurement instruments in collaboration with the six other teams selected to participate in the taxation round of EGAP’s Metaketa Initiative (<http://egap.org/metaketa/metaketa-ii-taxation>). See De La O et al. (2020b).

⁹Given the size of Zomba and the cost of skips, we could not randomize the placement of enough skips to have statistical power to study the effects of new service delivery alone. Waste collection through skips is a non-excludable public good, such that there was no way to randomly assign access to the skips. Even if we might have been able to assign access to the skips, the government announced the expansion of waste collection in public and any control group would likely have been affected by the treatment of increased service delivery.

¹⁰Half of the campaigners were contractors working for the Institute of Public Opinion and Research (IPOR), the Zomba-based research firm that enumerated the surveys, and half were city employees on temporary contract with IPOR. For a small number of subjects, some of the city employee campaigners deviated from the approved campaign script. We explore this deviation from the protocol below.

¹¹The information campaign was limited to owner-occupied households.

5 Hypotheses

By providing information intended to address the perceived barriers to becoming formalized property tax payers in Zomba in the context of an increase in actual service delivery by Zomba City Council, we aimed to increase rates of tax compliance among those households who received the information campaign.¹² In this section, we enumerate a set of hypotheses explicitly included in our pre-analysis plan. We also describe some outcomes linked to the barriers to formalization that we enumerated above but which were not included in our pre-analysis plan.¹³

At the most basic level, we expect households assigned to receive the information campaign to be more aware of their obligation to pay property taxes.¹⁴ And we expect that this increased awareness will lead to greater perceived salience of property taxes.

- **H₀: The treatment will increase information about city rates among treatment households vs. control households.**
- **H_{0a}: The treatment will increase the salience of city rates among treatment households vs. control households.**

More importantly, we expected the bundle of information contained in the campaign script and the brochure — in combination with the increased service delivery in the city — to motivate residents of Zomba to formalize their relationship with the city as tax-paying property owners. As with the other projects in the Taxation Round of the Metaketa Initiative, we study respondents' intentions to formalize (i.e., become legible taxpayers), their actual formalization, and their continued compliance with their tax obligations. In all three cases, we expect the information campaign to deepen citizens' relationship with Zomba City Council:¹⁵

- **H₁: The treatment will increase the intent to formalize among treatment households vs. control households.**
- **H₂: The treatment will increase formalization among treatment households vs. control households.**
- **H₄: The treatment will increase tax compliance among treatment households vs. control households.**

Since the information campaign also advertised the existence of new waste collection services, we anticipate that those households exposed to the information campaign will become more likely to use those services than those not exposed to the campaign.

- **H₃: The treatment will increase citizen's access to public services tightly related to the formalization process among treatment households vs. control households.**

We expect that the bundle of information contained in the information campaign and brochure will also affect citizens' perceptions of the city government in a number of ways. With the link between service delivery and taxation clearly drawn in the brochure and the scripted interaction with the campaigners, we

¹²The hypotheses that we present here parallel those found in the pre-analysis plan for the Taxation Round of the Metaketa Initiative (De La O et al. 2020a). Our project-specific pre-analysis plan is also registered on the Open Science Framework website (<https://osf.io/8q9mn/>).

¹³We include two of the exploratory hypotheses from the Metaketa Initiative in the [Appendix](#) only: access to services other than waste collection and payment of taxes other than property taxes.

¹⁴In the presentation of the hypotheses, we use the language of "city rates" found in our pre-analysis plan. As mentioned above, this is the local terminology for property taxes in Zomba.

¹⁵Note that, as in our pre-analysis plan, we retain the hypothesis numbering scheme from De La O et al. (2020a) here.

expect citizens to be more likely to perceive the collection of property taxes as legitimate. In line with the findings of Weigel (2017), we expect that the information campaign in the context of new service delivery will cause citizens to perceive that the city has more capacity than they would have imagined before — both in terms of service delivery and in terms of enforcement. Overall, we expect these perceptions to lead to greater satisfaction with the city government’s performance.¹⁶

- **H_{5a}**: The treatment will increase beliefs that the city deserves to collect taxes among treatment households vs. control households.
- **H_{5b}**: The treatment will increase beliefs in city service capacity among treatment households vs. control households.
- **H_{5c}**: The treatment will increase beliefs in city enforcement capacity among treatment households vs. control households.
- **H_{5d}**: The treatment will increase satisfaction with city government among treatment households vs. control households.

If residents of Zomba are updating their beliefs in the manner that we expect and are aware that other residents are also receiving the information campaign, such that they might expect similar updating among their neighbors, then those residents who receive the information campaign should think that overall tax compliance is going up in Zomba.

- **H_{5e}**: The treatment will increase beliefs that others pay city rates among treatment households vs. control households.

As described in our pre-analysis plan, we view H_0 , H_1 , H_2 , H_4 , and H_3 as confirmatory hypotheses (i.e., those that the study is specifically designed to evaluate) and the family of H_5 hypotheses as exploratory hypotheses (i.e., as providing additional evidence about the theory of change and to be confirmed in future studies). In addition, we view H_1 , H_2 , and H_4 as part of a family of hypotheses related to formalization, such that tests of those hypotheses should account for multiple comparisons.¹⁷

6 Experimental Design and Randomization

We partnered with the Institute of Public Opinion and Research (IPOR) to conduct the study. The research team used satellite and on-the-ground inspection of the city to define 91 plausible neighborhoods for inclusion in the study. None of these neighborhoods received organized waste collection from Zomba City Council.

We conducted an initial listing exercise in the 91 neighborhoods, identifying over 2,300 owner-occupied households. Based on budget and statistical power calculations, we selected 78 percent of these households to participate in the study.¹⁸

Our analyses use data from 1,806 respondents in the 91 neighborhoods.¹⁹ Of the 91 neighborhoods, we reserved nine as “pure control” neighborhoods where we did not administer the information campaign

¹⁶In the pre-analysis plan, we also report an aggregate hypothesis — that the treatment will improve attitudes toward government in general. We report only the component hypotheses here and the component outcomes in the analysis section.

¹⁷We do not adjust p-values for multiple comparisons in this paper because the test statistics for all confirmatory hypotheses return $p > 0.05$.

¹⁸We provide more details about our listing and sample selection procedure in the [Appendix](#).

¹⁹1,855 respondents were recruited into the study at baseline. An accidental data breach happened after the collection of midline data. Following a protocol that we developed with the University of Illinois Institutional Review Board, we reported the breach to respondents and offered them the opportunity to be removed from the study when we re-contacted respondents in the endline survey. Forty-nine respondents asked to be removed from the study. We do not use any data from these respondents in this paper or elsewhere. We cannot describe how they differ from the other respondents or otherwise directly evaluate the effect of removing these respondents on our inferences.

intervention. The 182 respondents in these pure control neighborhoods allow us to explore spillover of information or motivation from treated to untreated households in the intervention neighborhoods. Within the remaining 82 neighborhoods, we randomly assigned half of the households selected for the study to receive the information campaign. Households in the control condition did not directly receive the information bundle but were in neighborhoods where other people received the information bundle. All households in the study were plausibly exposed to the increase in service delivery. Figure 3 shows our design.

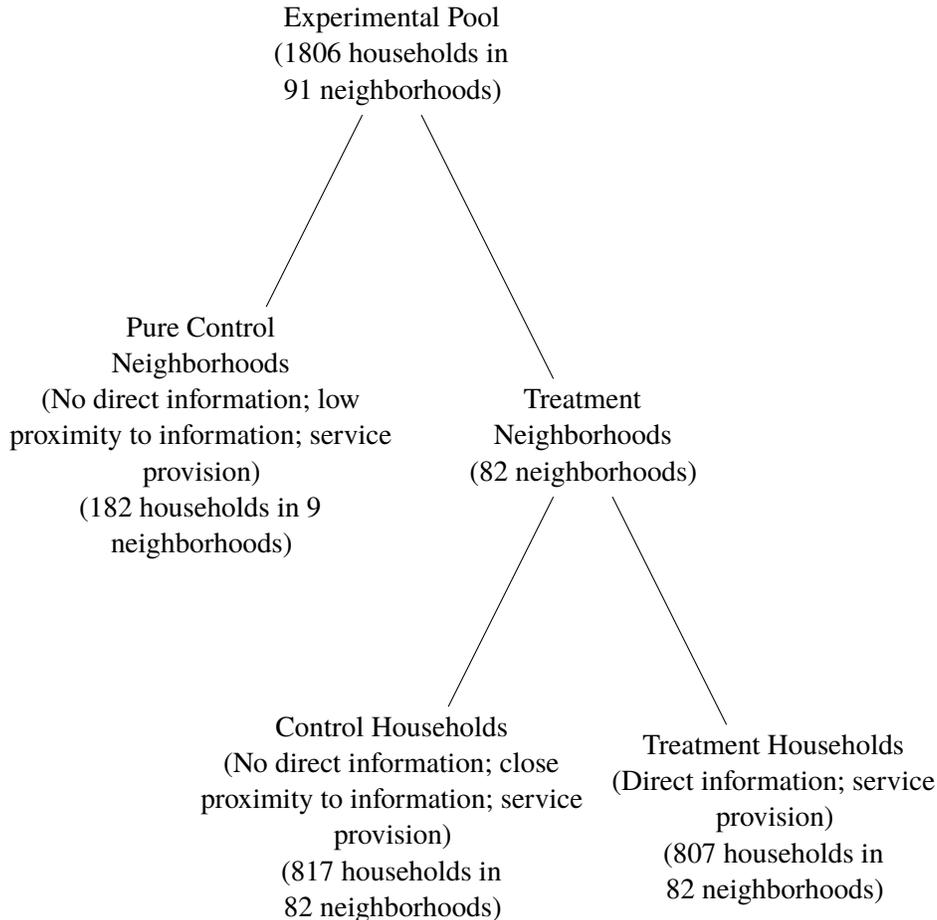


Figure 3: Experimental design. Treated households received visits from campaigners, who provided a brochure and verbal information about the payment of property taxes and waste collection service. The control households in the treatment neighborhoods did not receive these visits but live in close proximity to households that received these visits. The households in pure control neighborhoods did not receive the visits and live far from households that received the campaigner visit. All study households are exposed to the improved waste service provision.

We randomized households to receive in-person information visits (“treatment”) versus no visits (“control”) within neighborhood blocks, assigning 50% of the households in each of the 82 treatment neighborhoods to treatment and 50% of the households in each of the 82 treatment neighborhoods to control. These neighborhoods were demarcated for this study only and are not formal neighborhoods within the city, although we considered geographic features, like main roads and the river, when demarcating neighborhoods. We also required that the neighborhood boundaries not cross formal ward boundaries. As such, we expect that these neighborhoods will capture much of the social and economic variation between different areas of Zomba and give us increased precision when estimating average treatment effects.

7 Data and Methods

We collected both survey and administrative data to study the effect of the intervention. Survey data collection occurred at three time points: before the information campaign, a few weeks after the completion of the information campaign, and approximately ten months after the completion of the information campaign. Table 1 describes the timeline of the activities.

| Activity (N) | Time |
|--|--------------------------------|
| Baseline Survey (1806) | November to December 2018 |
| Information Campaign & Service Delivery Intervention | Early March 2019 |
| Midline Survey (1635) | Late March to Early April 2019 |
| Endline Survey (1535) | January to March 2020 |

Table 1: Survey and intervention timeline. For each survey, enumerators went to all households in the baseline survey. 171 respondents attrited between baseline and midline surveys. 271 respondents attrited between baseline and endline surveys.

We acquired administrative data describing property tax payments made between December 2018 and March 2019 (i.e., before the information campaign), as well as between July 2019 and September 2019.²⁰ In the analysis below, we consider any individual who was up-to-date on tax payments at the end of December 2018 as having already been a formalized taxpayer at baseline. Likewise, we consider any individual who made any payment between July and September 2019 as having been a formalized taxpayer at the time of the midline survey.²¹

Because Zomba City Council was re-mapping the city at the time of endline data collection, administrative data records were incomplete, and there were a number of households in the survey that we were unable to link to the administrative data. Overall, of the 1,806 individuals in the study, we were able to plausibly identify 524 corresponding tax records in the administrative data. As described below, we combine the measure from the administrative records with self-reported survey data to form one of the main outcome measures.

We believe there are two sources of bias with the property tax payment data. First, we suspect that administrative data underreports payments. Multiple survey respondents noted the possibility that a citizen could have a receipt proving payment of taxes without that information being reflected in the city records. Second, we suspect that the survey overreports property tax payments due to social desirability bias.²²

7.1 Measures

We use both survey and administrative data to measure tax formalization, relying on survey reports only if administrative data are missing. We measure other outcomes using survey data. Table 2 describes the measurement of each binary outcome and the proportion of respondents for whom we recorded a positive response at the time of the baseline survey.

The information in table 2 again reflects some of the reasons citizens do not pay taxes as reported

²⁰We are grateful to Zomba City Council for access to these data. A member of the Illinois research team traveled to Zomba to spend two weeks on site working to compile the administrative data linking tax payments with house locations.

²¹Our final measure for administrative property tax payments differs from what we described in our pre-analysis plan. We made this change to: 1) match other studies in the EGAP Metaketa II: Taxation more closely; 2) accomodate the administrative data provided, and 3) allow the administrative data and survey data to match more closely. In the pre-analysis plan, we considered individuals who made 50% of their payments at baseline (no specified date) as having formalized at baseline, and any individuals who made 50% of their payments by March 2019 as having formalized at midline.

²²In the administrative data for the September 2019 pay period, 76% of the administrative data records matched the survey data responses. Overall, 60% of households do not appear formalized in the administrative data and reported that they were not formalized in the endline survey; 20% do not appear formalized in the administrative data but reported that they were formalized; 16% appear formalized in the administrative data and reported that they were formalized, and only 3% appear formalized in the administrative data but reported that they were not formalized.

in figure 1. Relatively few respondents reported knowing that property taxes are owed, and even fewer respondents had administrative records that verify tax payment at baseline. Very few respondents reported having access to waste collection services from the local government (48/1806).

| Outcome | Measure | Proportion | Number of Yes Responses | Number of NA Responses |
|--|--|------------|-------------------------|------------------------|
| Knowledge about Property Tax Requirement (H_0) | Reported property tax is required | 0.39 | 702 | 52 |
| Access to Government Waste Collection (H_3) | Reported access to waste collection from government | 0.03 | 48 | 0 |
| Saliency of Property Tax (H_{0a}) | Reported discussion about property tax or waste collection at community meetings | 0.02 | 44 | 700 |
| Intent to Pay Property Tax (H_1) | Reported intent to pay next round of property taxes. Measured at baseline as the intent to pay by March 2019. Measured at midline as the intent to pay by September 2019. | 0.42 | 755 | 136 |
| Property Tax Formalization (H_2) | Reported or administrative records for payment of property taxes. Measured at baseline as payment for the March 2019 period. Measured at midline as payment for the September 2019 period. | 0.12 | 220 | 24 |
| Property Tax Compliance (H_4) | Reported payment of property taxes for next period. Not measured at baseline. Measured at endline as the intent to pay for the January 2020 period. | - | - | - |

Table 2: Proportion agreement, number of yes responses, and missing data for binary outcome measures at baseline. Total N=1,806. We have no baseline measure of tax compliance. NA responses for Saliency of Property Tax are high because many respondents reported that they did not go to any community meetings.

Table 3 reports the baseline statistics for each attitudinal outcome in H_5 .²³ The attitudinal measures are mainly indices and ordinal variables. To create baseline indices, we impute NA responses using means calculated by neighborhood and treatment assignment. We do not impute for any post-treatment indices. Detailed descriptions about index creation and imputation are in our pre-analysis plan.

²³The pre-analysis plan describes two other hypotheses, and baseline statistics for these (an access to services index (H_6) and a tax morale index (H_7)) are available in the Appendix A2. The tax morale question, one of the items included in the tax morale index, is also included in creating the government legitimacy index (H_{5a}).

| Outcome [Range] | Mean | SD |
|--|------|------|
| Government Legitimacy (Additive Index) [0.00, 1.00] | 0.61 | 0.24 |
| Government Service Capacity (Additive Index) [0.00, 1.00] | 0.33 | 0.34 |
| Government Enforcement Capacity (Additive Index) [0.00, 1.00] | 0.75 | 0.26 |
| Government Satisfaction (Additive Index) [0.00, 1.00] | 0.55 | 0.16 |
| Perceptions of Community Compliance (Single Item) [0.00, 1.00] | 0.37 | 0.33 |

Table 3: Descriptive statistics for attitudinal outcome measures at baseline. N=1,806.

7.2 Estimation

We estimate the intent-to-treat effect of assignment to receive the information campaign. As specified in the hypotheses, our outcomes are awareness of property taxes (H_0), salience of property taxes (H_{0a}), intent to formalize (H_1), formalization with city government (H_2), access to waste collection (H_3), tax compliance with city government (H_4), attitudes about government (set of H_5), and perceptions of community behaviors (H_{5e}).

In line with the pre-analysis plan, we also estimate heterogeneous effects of the information campaign treatment on the outcomes referenced in H_1 , H_2 , H_4 , and set of H_5 for two subgroups. We expect the information campaign treatment to be more effective (1) among individuals who have low satisfaction with their baseline form of waste collection compared to individuals who have high satisfaction with their baseline form of waste collection and (2) among individuals who have low ex ante information about property taxes compared to individuals with high ex ante information about property taxes.

Our estimators of the intent-to-treat effects are mean differences, and, for convenience in producing standard errors and in appropriate weighting, we use the following linear model in equation 1 and ordinary least squares (OLS) to calculate those estimates.

$$Y_{i,j} = \beta_0 + \beta_1 Z_{i,j} + Z_{i,j} * X_{i,j} + \epsilon_{i,j} \quad (1)$$

where i is the individual in neighborhood j , Y is the outcome, X is the mean-deviated baseline outcome and block indicator for individual i , and Z is the treatment indicator.²⁴ When possible, we use a saturated regression like Equation 1 to estimate the average treatment effect, using the mean-deviated baseline outcome and block indicator as covariates (Lin 2013). This regression adjusts for covariates by interacting mean-deviated covariates with the treatment indicator. When this model overfits, we instead follow Equation 2, where we regress the outcome on a treatment indicator and a baseline outcome and use regression weights proportional to the size of the neighborhoods j .²⁵

$$Y_{i,j} = \beta_0 + \beta_1 Z_{i,j} + X_{i,j} + \epsilon_{i,j} \quad (2)$$

For the analysis of heterogeneous effects by baseline satisfaction and knowledge, we estimate intent-to-treat effects among the subgroups of people with low satisfaction and with low information at baseline

²⁴There are two main methods of analyzing data with two time points: (1) predict t_2 outcomes using t_1 outcomes as a covariate, or (2) predict the change from t_1 to t_2 . We prefer to add baseline outcomes as a covariate because this strategy yields more efficient estimates than employing change scores as the outcome variable. We use change scores, however, if treatment and control groups are not balanced on baseline measures of outcome variables (i.e., treatment status correlates with baseline measures of outcomes as it might be in a non-randomized study). If treatment and control groups are not balanced on baseline outcomes, then baseline outcomes will be correlated with the error term, which will bias estimates. Using the change score removes the correlation between the baseline outcome and treatment, providing unbiased estimates (“Use Change Scores or Control for Pre-Treatment Outcomes? Depends on the True Data Generating Process” 2019). The change score equation is the same as 1 or 2, except the change score is the outcome variable and we include only block indicators in the X matrix.

²⁵We use Neyman’s randomization-based standard errors and base our confidence intervals and p -values on the common Neyman-style design-based large-sample approximations.

using Equation 1 or, if overfitting occurs, Equation 2.

8 Results

In the analysis that follows, we first present the results of the intervention. We cannot show that the information campaign had any detectable effect on the tax behavior and government attitude outcomes, which are the main outcomes of our study.

We, therefore, spend the remainder of the paper exploring some possible explanations for why the information campaign had such a small effect on tax payment and government attitudes. We explore four explanations: (1) the intervention was poorly designed and did not provide information or reduce barriers, (2) the intervention was poorly implemented, (3) effects spilled over into the control group, and (4) the theory motivating the intervention needs further development.²⁶

We can reject the explanation that the intervention was poorly designed and did not provide information that should reduce barriers. We show that the information campaign increased direct informational outcomes corresponding to the four barriers that we identified in our fieldwork. The visits increased knowledge about property taxes and about the tax forgiveness plan, and access to waste collection increased throughout the sample. Overall, citizens saw the city increase service delivery and learned about the need to pay property taxes.

We must consider that our overall effect was hindered by inconsistent implementation of the information campaign. The information campaigners consisted of two types of individuals: city staff and non-city staff. During the midline survey, we learned that citizens viewed city staff and non-city staff differently, though all campaigners, with the city's approval, presented themselves as visiting households "on behalf of Zomba City Council." We also became aware that two of the city staff deviated from the information campaign script. Therefore, we unintentionally implemented three sets of experimental interventions that differed by neighborhood rather than a single experimental intervention across all 82 neighborhoods: (1) on-script non-city staff, (2) on-script city staff, and (3) off-script city staff.

If these three different interventions caused opposite reactions among respondents – positive for some and negative for others – then we would observe an overall null effect even though the campaign was successful when properly implemented. To explore whether implementation inconsistencies obscure a treatment effect, we estimate treatment effects for respondents who received each type of campaigner to control respondents in the same neighborhoods. We find that the off-script city staff did not increase knowledge about property tax requirements, decreased tax payment, and decreased access to waste collection – the respondents they talked with were *less likely* to use the new waste services than their neighbors. We cannot conclude, however, that a properly implemented information campaign would increase tax payment because effects for the on-script groups are not consistently positive.

We also must consider the possibility that spillover contaminated our control group and diminished our estimate for the effect of the information campaign. Though knowledge of property taxes did not increase among control respondents in treatment neighborhoods, their intent to pay property taxes increased relative to respondents in the pure control group, who were less likely to be exposed to the information campaign. This spillover effect on intended payments is strongest for control respondents in neighborhoods visited by on-script city staff.

We conduct several analyses to determine if the theory underlying our intervention needs further development. With regard to the possibility that our intervention was misguided because information linking

²⁶Some of these analyses were not pre-registered in our pre-analysis plan. We preregistered the analysis of spillover, a correlational analysis of information on tax and attitude outcomes, and an analysis of heterogeneous effects on subsets of respondents who had low baseline knowledge and low baseline satisfaction with waste collection. We also preregistered an analysis to assess whether or not our intervention increase knowledge, but not the informational index we use to measure knowledge. We did not preregister the analysis of implementation problems or the CACE analysis to determine if the intervention had an effect through increasing information. Nor did we preregister the analysis of heterogeneous effects on the subset of respondents who began using the new waste collection.

taxes and services does not increase tax payment or improve government attitudes, we see mixed evidence. On one hand, we show that informational outcomes correlate with tax payment and positive government attitudes, even controlling for socio-demographic factors. On the other hand, increasing knowledge does not affect tax behavior or government attitudes. We show null effects for a Complier Average Causal Effect (CACE) of the intervention on outcomes through increasing information. We also show that treatment respondents with low baseline knowledge levels or low satisfaction with their baseline method of waste disposal — groups among which we might be particularly likely to observe treatment effects — were no more likely to change attitudes and behaviors than citizens with high baseline knowledge levels or high satisfaction with their baseline method of waste disposal. Although the campaign increased information, the increase in knowledge did not translate into changed attitudes or actions even among those who started out with the least understanding of the Zomba property tax system and who had the most to gain from new service delivery.

Although our intervention increased tax knowledge without affecting tax behavior or government attitudes, we should not reject the idea that some citizens *do* think about a trade of taxes for services. We show that evaluations of government performance and tax compliance improve for citizens who reported using the new waste collection service. Although these citizens were not randomly assigned to use the city service, their attitudes and behaviors improve from baseline to midline.

8.1 Effects of the Information Campaign on Formalization and Attitudes

As reported above, we find that the information campaign does not predict respondent's reported or actual tax payment activity. Nor did it affect attitudes toward the government or perceptions of tax compliance in the community. Figure 4 reports the results for the salience of property taxes (i.e., increased discussion of property taxes), the intent to formalize, actual or self-reported formalization, and intended compliance. Figure 5 reports the results for perceptions of government legitimacy, service capacity, and enforcement capacity; satisfaction with the government; and perceptions of community tax compliance.

In figure 4, we see that respondents treated with the information campaign are slightly more likely to pay or report payment of property taxes at midline (1.6 percentage points) and to say that they plan to pay taxes in March 2020 (2.7 percentage points) compared to control, but both confidence intervals cross zero. In figure 5, we see that perceptions of community compliance also increased a small amount (1.6 percentage points) compared to control, which could suggest the campaign began to change descriptive norms about tax compliance; again, the confidence interval crosses zero.

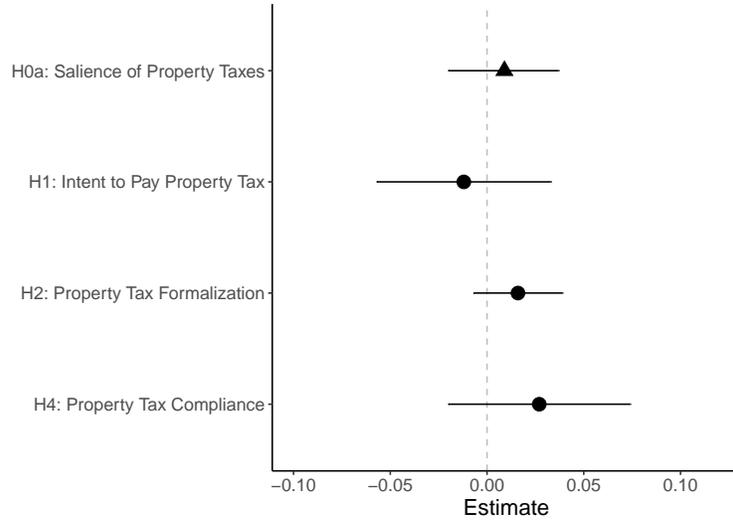


Figure 4: Effect of information campaign on tax outcomes. Estimated intent to treat effects and 95% confidence intervals shown (● represents Equation 1 and Tax Compliance does not have a baseline measure); ▲ represents Equation 2).

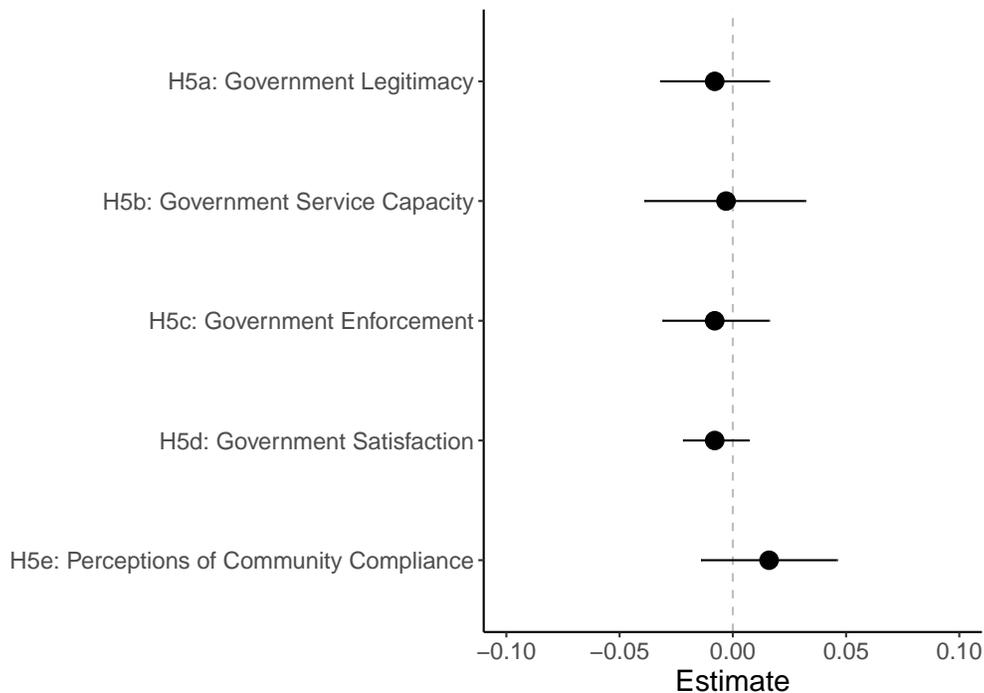


Figure 5: Effect of information campaign on attitudes toward government and public services. Estimated intent to treat effects and 95% confidence intervals shown (● represents Equation 1).

9 Diagnosing the Null Effects

There are several possible explanations for the null effects estimated above. As mentioned previously, we identify and analyze four: (1) aspects of the intervention design meant that it did not provide information that would reduce barriers, (2) poor implementation of the intervention meant that the information was not delivered, (3) effects spilled over into the control group, and (4) the theory motivating the intervention needs

further development.

9.1 Intervention Design

Our hypothesis that the information campaign would increase tax payment rested on the assumption that the information campaign would increase citizens' awareness of their responsibility for paying property taxes (H_0), decrease the perceived difficulty of paying taxes, alert citizens to the city's willingness to restructure their overdue payments, and signal Zomba City Council's increased capacity for waste collection (H_3). By conveying this information, we expected the campaign to reduce barriers to citizens becoming formalized taxpayers in Zomba and thus to see more intended and actual compliance with the tax laws. If the intervention did not provide information that reduced these barriers, then the reduction of these barriers could not lead to changes in attitudes and behavior.

The most direct way the intervention could fail to reduce barriers is if it did not signal the city's capacity to provide a service. We delivered the experimental information campaign in the context of a new and significant increase in waste collection in Zomba. The week of the information campaign, Zomba City Council began collecting garbage from designated collection points with new collection skips using a skip-loading truck. Figure 6 shows that citizens of Zomba saw this increase in city service delivery occurring: reported access to waste collection increased by almost eight percentage points between the baseline and midline surveys.²⁷ Given these increases, it is unlikely that citizens believed the city lacked the capacity to provide waste collection.

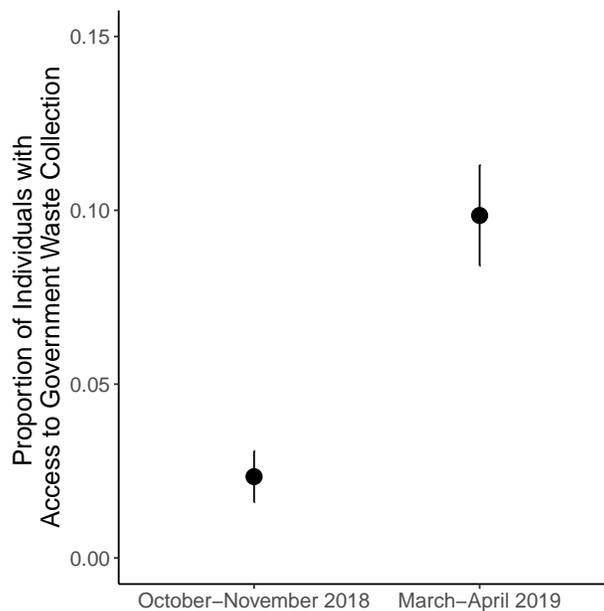


Figure 6: Increase in use of government waste collection from skip system as reported by survey respondents at baseline and midline. Points represent proportion of households who reported use of government waste collection, lines show 95% confidence intervals. Difference in proportion: 0.08 ($p < 0.001$)

We can also reject the possibility that the intervention did not reduce informational barriers. In figure 7, the first point estimate (Informational Outcomes Index) and confidence interval show that our information campaign improved overall information about tax payments and information about waste collection. The Informational Outcomes Index is an additive index of the six informational outcomes: knowledge about property taxes (reflecting H_0), knowledge about bank deposits or transfers as a method of payment, knowledge

²⁷Reported access to waste collection has continued to increase in the last ten months, as well, as measured by our final endline survey.

about property taxes to finance waste collection, perceived ease of payment, knowledge about tax forgiveness, and access to government waste collection (reflecting H_3). Broken down separately, we see that the information campaign affected knowledge of property taxes and about the tax forgiveness plan in particular. It also increased people’s knowledge about ways to pay taxes; namely, that taxes can be paid through bank deposits or bank transfers.

The information campaign did not, however, change perceived ease of paying taxes. The campaign sought to reduce this barrier by telling respondents about the ability to pay property taxes through a bank transfer or bank deposit, rather than a visit to the city’s administrative offices. While a trip to a bank is relatively easier than a trip to the city’s offices, we speculate that knowledge of the ability to pay taxes at a bank did not increase perceived ease of payment because payment still involves a trip outside the house, banks are not known for quick service, and only 51% of respondents report access to a bank account. The campaign itself also did not affect access to government waste collection. We are not surprised by this null: government waste collection was distributed without regard to treatment status and all citizens of Zomba could see the skips and the skip loader at work; Zomba City Council also announced and publicized the new waste collection system publicly in a variety of fora. The intervention increased access to waste collection for all citizens, not just those told about waste collection through the information campaign.

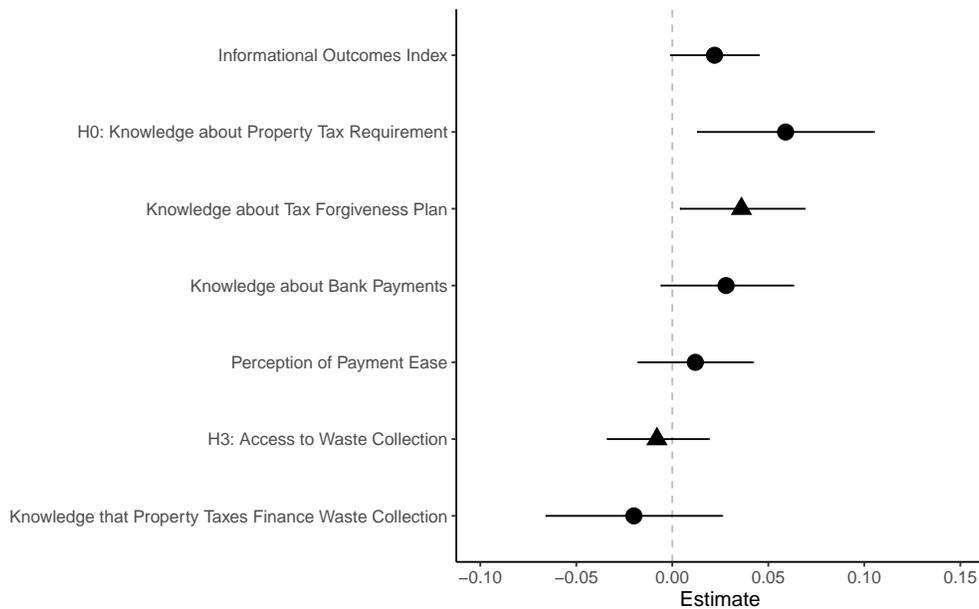


Figure 7: Effects of information campaign on key informational outcomes measured in the midline survey. Points show the estimated average intent to treat effect (● represents Equation 1; ▲ represents Equation 2). Lines show 95% confidence intervals. The “Informational Outcomes Index” is a composite of the other six items.

These results – increased waste collection service and increased knowledge – suggest that the intervention was not badly designed. Access to government waste collection increased from 2.3% at baseline to 12.2% at midline.²⁸ The information campaign increased knowledge about taxes and tax forgiveness even without accounting for spillover, which would dull informational effects by increasing knowledge in the control group. The campaign increased knowledge of ways to pay taxes but did not change perceived ease of paying taxes. If a necessary condition to increase payment of taxes is to increase ease of payment, then the intervention did not achieve its goals. However, we can reject the idea that the intervention did not increase

²⁸ Access to government waste collection rose to 23% at endline, as citizens adjusted their waste disposal methods and substituted burning or throwing trash in the river with use of the skips.

service access or provide information.

9.2 Implementation Issues

As described above, the information campaigners consisted of (1) non-city staff who stayed on script, (2) city staff who stayed on script, and (3) city staff who went off script.²⁹ We have reason to believe that the implementation of the information campaign varied with the identity of the information campaigners. If citizens had many weakly positive interactions with some campaigners but a few strongly negative interactions with others, we might observe null effects overall. A null effect due to implementation problems would be interpreted very differently from a null effect due to a theoretical failure.

To determine if implementation problems cause our null effect, we estimate treatment effects separately for respondents who encountered each type of campaigner. Each type of campaigner operated within a delimited set of neighborhoods; no neighborhood received a mix of on- and off-script campaigners.³⁰ We can therefore compare treatment and control households within neighborhoods visited by each type of campaigner to learn if different types of campaigners had differential effects on outcomes. Figures 8, 9, and 10 shows the results of this analysis. Individuals who received the information campaign from campaigners who stayed on-script (whether they were city staff or not) were more likely than their neighbors to gain information about property taxes and to formalize their relationship with Zomba City Council through paying property taxes. Individuals who received the information campaign from non-city staff were most likely to increase their formalization and intend to comply with property taxes in the future. This pattern could reflect variation in message credibility: of course city staff want citizens to pay property taxes, but citizens take notice if someone else tells them that taxes support services.

Figure 8 suggests that on-script campaigners caused null or negative effects on respondents' intent to pay taxes, but this is misleading. In the next section we show that these null or negative coefficients arise because of spillover to control respondents: all respondents in on-script areas became slightly more likely to say they intended to pay taxes, not just respondents who received the information campaign. Similarly, the intent to pay coefficient for respondents who interacted with off-script city- staff is also misleading. Our analysis of spillover in the next section shows that all respondents in the off-script areas became *less* likely to say they intended to pay property taxes. These individuals were also less likely than their neighbors to formalize and were even less likely to use the new waste collection service. The fact that a negative experience with city staff might have deterred both tax payment and use of city services highlights the adverse effects of negative interactions between citizens and government. It also suggests that citizens do, at some level, understand that the city is trading services for taxes.

Overall, this analysis suggests that implementation problems could partially account for null effects on our primary outcomes. City staff who went off-script may have deterred payment of property taxes, which could have canceled out positive effects. However, implementation problems cannot fully account for the null effects. When we look only at campaigners who stayed on-script, treatment effects for tax payment outcomes are still small and insignificant. In the next section we consider the possibility that our null effects are due to spillover.

²⁹The off-script city staff visited approximately 157 households in 16 neighborhoods, the on-script city staff visited 328 households in 36 neighborhoods, and the on-script non-city staff visited 322 households in 30 neighborhoods.

³⁰Our analyses of the off-script city staff in this section depend on the assumption that those campaigners adhered to the randomization schedule in choosing households even though they used a different script with the assigned households. We have no evidence that the off-script campaigners deviated from the randomization schedule to a greater extent than the on-script campaigners.

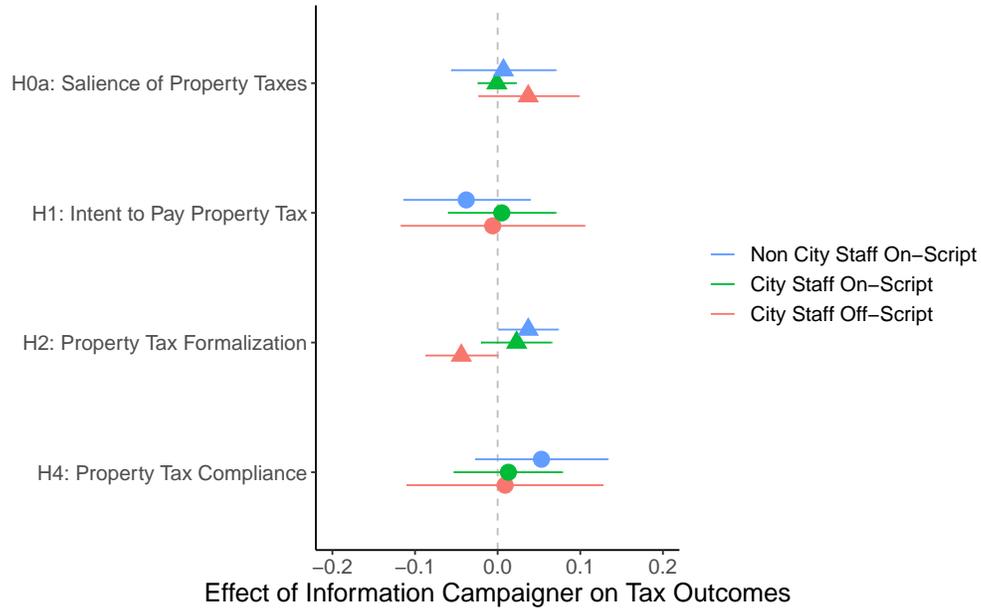


Figure 8: Effect of information campaigner on tax outcomes. Points are intent-to-treat average treatment effects estimated using OLS (● represents Equation 1 and Tax Compliance does not have a baseline measure; ▲ represents Equation 2). Lines are 95% confidence intervals. Campaigner types correspond to separate sets of neighborhoods. Differences between estimates reflect both differences in neighborhoods as well as differences in staff behavior.

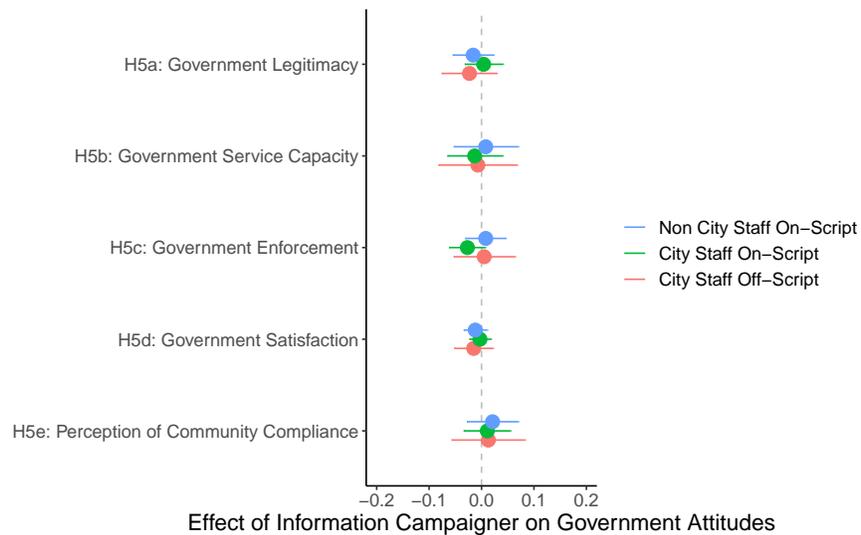


Figure 9: Effect of information campaigner on government attitudes. Points are intent-to-treat average treatment effects estimated using OLS (● represents Equation 1; ▲ represents Equation 2). Lines are 95% confidence intervals. Campaigner types correspond to separate sets of neighborhoods. Differences between estimates reflect both differences in neighborhoods as well as differences in staff behavior.

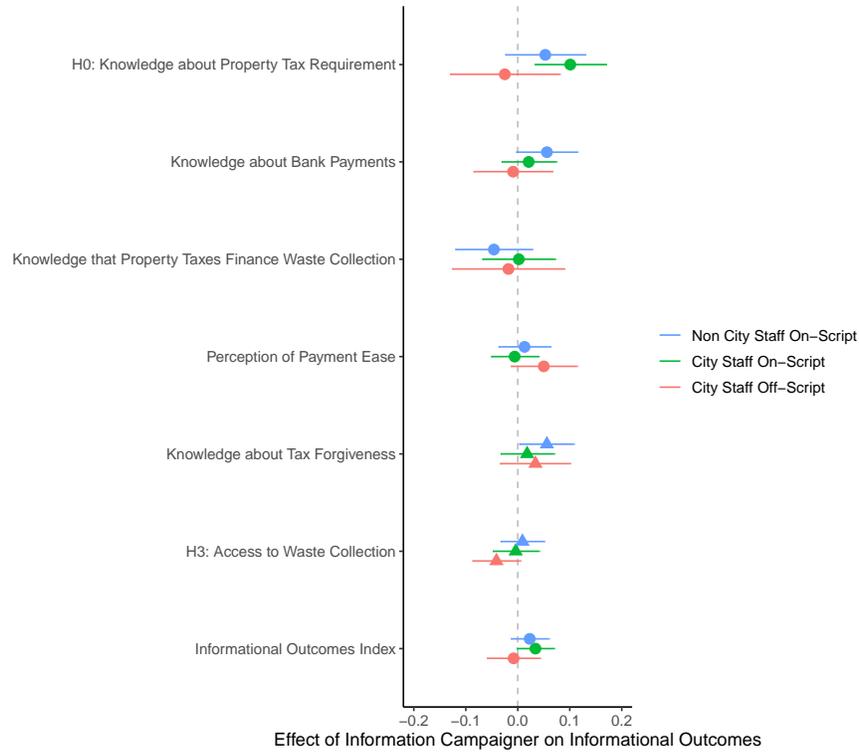


Figure 10: Effect of information campaigner on informational outcomes. Points are intent-to-treat average treatment effects estimated using OLS (● represents Equation 1; ▲ represents Equation 2). Lines are 95% confidence intervals. Campaigner types correspond to separate sets of neighborhoods. Differences between estimates reflect both differences in neighborhoods as well as differences in staff behavior.

9.3 Spillover

Our small and null effects could have arisen because the information campaign also affected the control group. Control group respondents are neighbors of treatment group respondents – they may have seen information brochures or information campaigners in their community; they may have even talked about the campaign visit with their treatment group neighbors. We also expect spillover to differ by type of information campaigner for two reasons. First, we provided some evidence that direct effects on treated individuals differ systematically between on-script city staff, off-script city staff, and non-staff. Any effect derived from direct effects, like spillover, should also differ systematically by implementation group. Second, it is possible that city staff, who tend to talk to neighborhoods generally, did not adhere to the treatment assignment as closely as non city staff who were more accustomed to implementing randomized research designs.

We anticipated that information would spread in the densely packed urban neighborhoods we study and designed our experiment to estimate spillover to the control group. Of the 91 neighborhoods we study, we left 9 completely untreated as a pure control group. No one in these neighborhoods received an information campaign visit. To determine if spillover to control respondents accounts for our small and null effects, we compare the 182 control households in 9 pure control neighborhoods with the 817 control households in 82 treatment neighborhoods. We also break down this exploration by type of campaigner.

Our analysis of spillover is cluster-randomized, whereas other analyses were block-randomized. Assignment to receive an information campaign visit was randomized at the household level, within treated blocks. Assignment to pure control, however, was randomized at the neighborhood level: all households in these neighborhoods are assigned to the same experimental group. All households in each pure control neighborhood are “pure control”, and all households in each treatment neighborhood are “control in treatment

neighborhoods.” Therefore, we cluster standard errors at the neighborhood-level to study these results. This analysis is underpowered to detect statistically significant effects, so confidence intervals will be large, but we consider the effect sizes to learn about how spillover might have affected the study.

Figures 11, 12, and 13 show that there are some spillover effects and that they are conditional on the type of information campaigner. In neighborhoods visited by on-script city staff, control households were more likely, as compared to households in pure control neighborhoods, to intend to pay their property taxes, to continue complying with property taxes, and to know about bank transfers as a method to pay property. We see a similar but smaller spillover in neighborhoods visited by non-city staff who stayed on script. In neighborhoods visited by off-script city staff, control households became less likely to intend to pay their property taxes. We also see government satisfaction and perception of payment ease both decrease for control households in neighborhoods visited by off-script city city compared to pure control neighborhoods. Overall, control households in treatment neighborhoods were more likely to discuss property taxes in community meetings and more likely to know about using bank transfers when compared to the control households in pure control neighborhoods. They were less likely to be satisfied with the government or perceive their neighbors as paying property taxes compared to pure control neighborhoods.

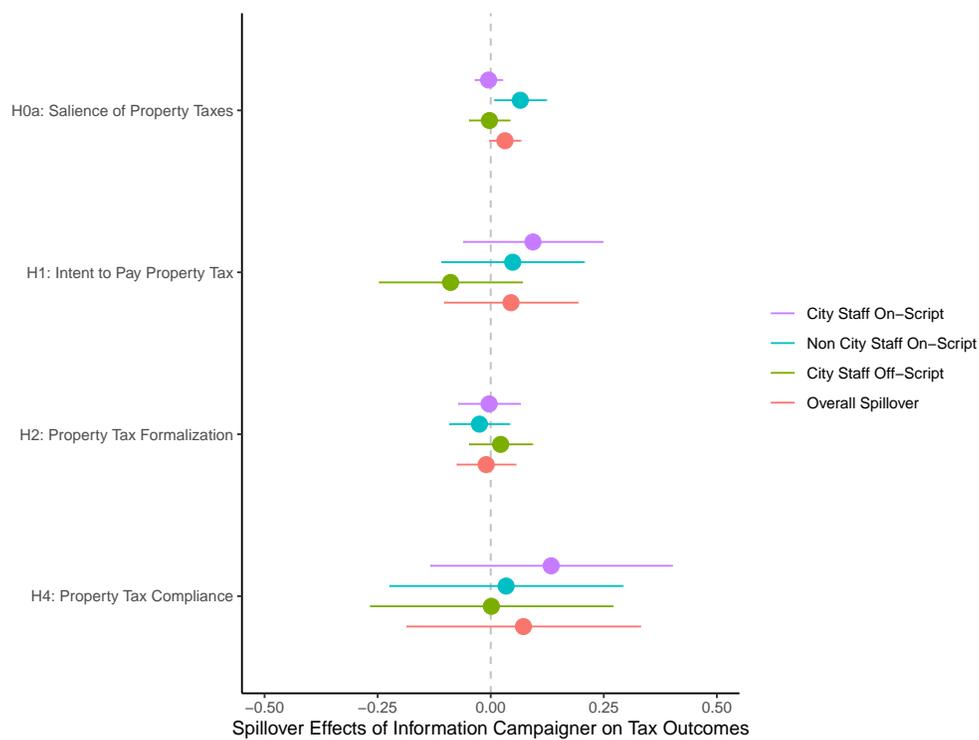


Figure 11: Spillover effects of information campaigner on tax outcomes. Points are control-to-pure control average treatment effects estimated using OLS restricted to respondents in the control conditions (● represents Equation 1 without block-size weights and with clustered standard errors. Tax Compliance does not have a baseline measure). Lines are 95% confidence intervals.

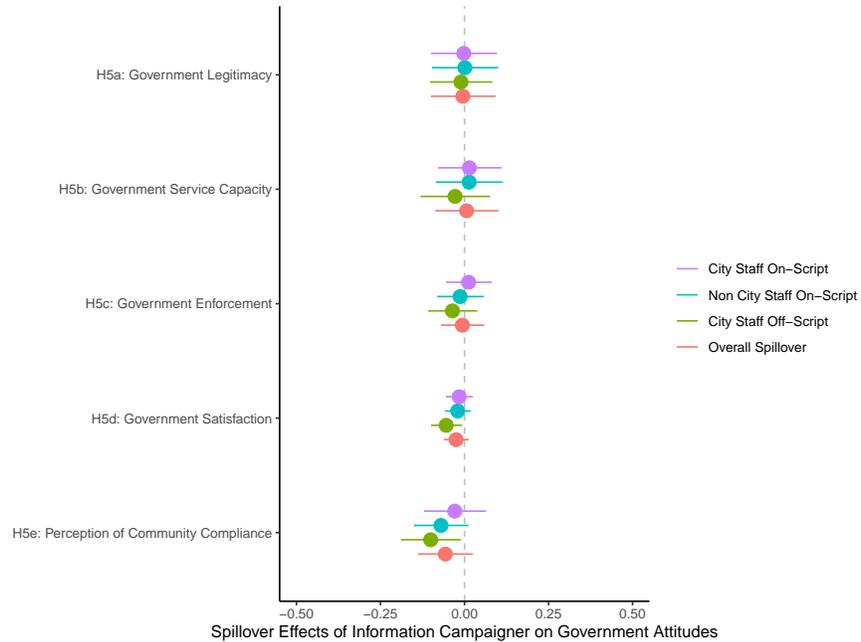


Figure 12: Spillover effects of information campaigner on government attitudes. Points are control-to-pure control average treatment effects estimated using OLS restricted to respondents in the control conditions (● represents Equation 1 without block-size weights and with clustered standard errors). Lines are 95% confidence intervals.

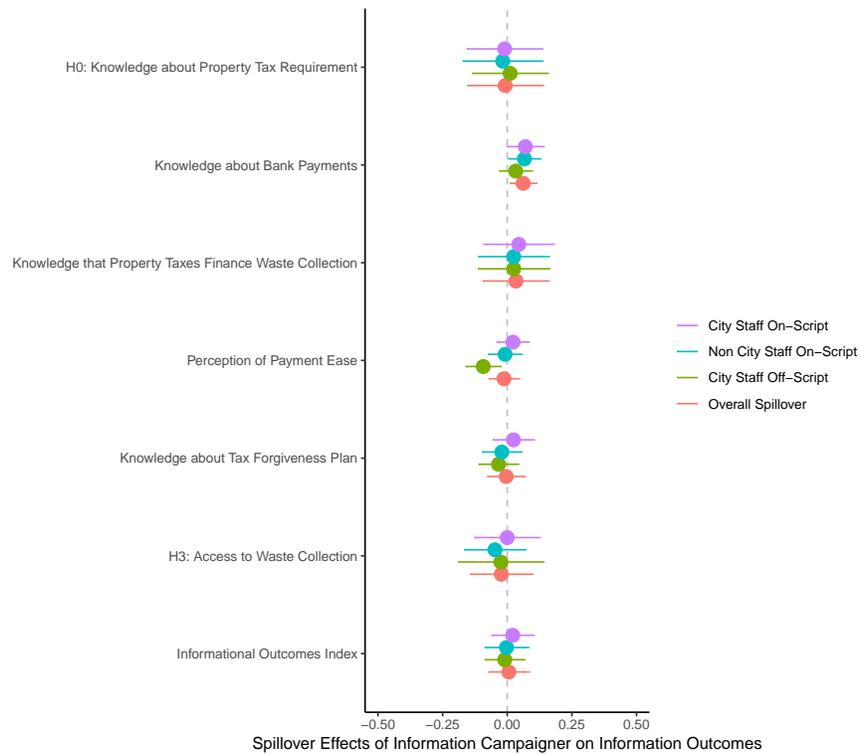


Figure 13: Spillover effects of information campaigner on informational outcomes. Points are control-to-pure control average treatment effects estimated using OLS restricted to respondents in the control conditions (● represents Equation 1 without block-size weights and with clustered standard errors). Lines are 95% confidence intervals.

This spillover analysis suggests that spillover could have canceled out the average treatment effects on tax outcomes (like intent to pay and tax compliance) because spillover caused all households in treatment neighborhoods to look the same on those outcomes. It also corroborates our previous section showing that implementation issues may contribute to our null findings. Specifically, more positive spillover occurred for outcomes like intent to formalize and tax compliance in neighborhoods with on-script city staff. Off-script city staff, on the other hand, decreased intent to pay taxes even for individuals they did not interact with.

We take into account implementation and spillover issues by using models that compare households visited by on-script staff with pure control households on the information outcomes index, intent to pay property taxes, property tax formalization, and property tax compliance. As with the spillover models, these models are underpowered because the households are cluster-randomized — households in treatment neighborhoods are all treated and households in control neighborhoods are all in control — and we cluster standard errors with neighborhood clusters to estimate effects. Though none of the results for these four outcomes are statistically significant, we do see positive effects. We also see effect sizes for the information outcomes index (3.6 percentage points, $p > 0.05$), intent to pay property taxes (6.4 percentage points, $p > 0.05$), and property tax compliance (9.5 percentage points, $p > 0.05$) which are bigger than effects reported in table 4, where we did not consider implementation issues or spillover. The effect size for property tax formalization stays roughly the same (1.5 percentage points, $p > 0.05$). We had expected it to increase because households who had visits from on-script staff tended to increase property tax formalization. However, we also see that there was negative spillover for those households, which suggests that control households in neighborhoods that received on-script staff tended to formalize less on average than control households in control neighborhoods. Therefore, the naturally higher levels of formalization in pure control neighborhoods could explain why the effect size for property tax formalization does not change in these models that account for implementation and spillover issues.

Overall, this study may have had larger, detectable effects if we did not have spillover and implementation issues. However, these issues would not explain all of the null results. In the next section, we consider the possibility that our results provide evidence against the theory underlying our intervention.

9.4 Lack of Theoretical Support

We now turn to the possibility that the theory motivating our intervention needs further development. First, we examine observationally if knowledge about taxes and perception of low barriers predicts tax payment and positive government attitudes. Second, we consider whether the intervention improved tax payment and government attitudes for respondents who gained knowledge, who had low knowledge, and who had low baseline satisfaction with their method of waste disposal. Third, we examine if tax payment and government attitudes improved among respondents who gained access to waste collection, whether or not they received the information campaign.

9.4.1 Does information predict attitudes or behaviors?

Earlier we showed that assignment to the information campaign improves informational outcomes, but do these informational outcomes correspond to tax payment and positive government attitudes? One plausible explanation for our null effects is that these informational outcomes are unrelated to tax behavior and government attitudes.

In Table 4 we show that the informational outcomes correlate with our tax behavior and government attitude outcomes. Individuals who score high on the informational index at midline tend to score higher on all of the tax behavior and government attitude outcomes as well. We report regression estimates that control for basic socio-demographic background variables in the [Appendix](#); the information index remains a strong predictor of behaviors and attitudes. We can reject the idea that information is unrelated to tax behavior and government attitudes.

| Outcome | Predicted by Informational Outcomes |
|------------------------------------|--|
| Saliency of Property Taxes | No ($-0.02, p > 0.05$) |
| Intent to Pay Property Taxes | Yes ($0.63, p < 0.001$) |
| Property Tax Formalization | Yes ($0.08, p < 0.05$) |
| Property Tax Compliance | Yes ($0.66, p < 0.001$) |
| Government Legitimacy | Yes ($0.22, p < 0.001$) |
| Government Service Capacity | Yes ($0.14, p < 0.001$) |
| Government Enforcement | Yes ($0.17, p < 0.001$) |
| Government Satisfaction | Yes ($0.09, p < 0.001$) |
| Perception of Community Compliance | Yes ($0.18, p < 0.001$) |

Table 4: Relationship between informational outcomes and tax and government attitudes outcomes. The table shows p -values from two-tailed hypothesis tests of a regression coefficient from a linear model where the outcome in each row is regressed on the informational outcomes composite measure, baseline outcome (except for property tax compliance, which does not have a baseline value), baseline socioeconomic index, education, and gender.

9.4.2 Does increasing knowledge increase tax payment and improve attitudes?

The preceding sections and previous analyses of the informational outcomes show that (1) the information campaign was able to provide information, (2) information correlates with positive orientation toward the government, including actual and intended payment of taxes, and (3) the information campaign did not, on average, improve orientation toward the government. Even though the information campaign had no effect on these outcomes on average, did it increase tax payments among respondents for whom it conveyed information? Could the small and null effects of figures 4 and 5 have arisen from differences in the amount of information actually received by the homeowners during the information campaign?

Our research design randomly assigned an intent to increase information for the respondents in the study. Yet, not all of them received the same amount of information. We can use random assignment to the information campaign as an instrument to learn about the effect of gaining property tax information on tax payment behavior and attitudes toward the government. This effect is often called the Complier Average Causal Effect because it focuses attention on the people who “complied with” (or actually received a dose of) the intended treatment. In our case, we think of compliance with treatment as continuous — homeowners assigned to treatment in our study received small extra doses of information, and each person absorbed more or less information depending on their own background and on the idiosyncratic circumstances of their particular interaction with a campaigner.³¹

We estimate complier average causal effects (CACE) using two-stage least squares, where tax behavior and government attitude outcomes are regressed on informational outcomes, using treatment assignment as an instrument for informational outcomes. We have null results for all tax behavior and government attitude outcomes (see Appendix, tables A5 and A6). These results suggest that the individuals who had better intervention outcomes due to assignment to treatment were not more likely to show an increase in the tax and government attitude outcomes than those individuals for whom treatment did not increase information.

³¹Our CACE requires that respondents could only receive this informational change via the visits made to their homes in the informational campaign. We satisfy that requirement – our analysis of spillover above showed no evidence for spillover of information. Respondents who were not at home during treatment but who were available for outcome measurement (about 1% of the pool assigned to treatment) are counted as receiving no extra information since assignment to a visit cannot have increased their information if they were not available to speak with the canvasser.

9.4.3 Do households with less prior knowledge or low prior satisfaction increase tax payment and improve attitudes?

Although respondents who gained information from the information campaign were no more likely to pay taxes or improve attitudes than other respondents, could the information campaign have changed attitudes and behavior among respondents for whom the information was novel or useful? To consider these heterogeneous effects, we assess whether effects of the information campaign were stronger among individuals who had low knowledge about property taxes or low satisfaction with government at baseline.³² Citizens who did not know about or understand the link between payment of property taxes and service delivery may be more likely to use the information provided by the campaign, and citizens currently unsatisfied with their status quo form of waste collection should be most likely to want some kind of change to their waste collection system. In both cases, there are some effects on informational outcomes. However, in neither case, could we distinguish the treatment effects for tax outcomes or government attitudes in these groups from those estimated among the rest of the experimental pool. These results are available in the [Appendix](#) figures [A5](#) and [A6](#).

Overall, the CACE and the analyses of heterogeneous effects demonstrate that an intervention can successfully provide information without boosting tax payment or positive attitudes towards the government, even though information correlates with tax payment and positive attitudes towards the government and even among the type of respondent who should be affected by information. We consider the implications of these results in the discussion.

9.4.4 Does use of new government services increase tax payment and improve attitudes?

A key theoretical idea motivating our intervention was that citizens think about a trade of taxes for services and are willing to make this trade if the government credibly commits to providing services. This idea emphasizes personal experience with service delivery as important for tax payment, rather than information about service delivery. Though we have shown that increasing information does not increase tax payment or improve government attitudes, we have not evaluated the possibility that novel use of government services will increase tax payment and improve attitudes towards the government.

To consider this possibility, we look observationally at the effect of citizens gaining new access to waste collection on tax payments and attitudes. For this analysis, we compare respondents who began using government waste collection between the baseline and the midline survey with all other respondents. [Figure 14](#) shows the relationship between using skips and tax outcomes; [figure 15](#) shows the relationship between using skips and attitudes.

Respondents who began using the skips became significantly more satisfied with the government compared to other respondents, who did not enjoy a material change from government services. These respondents also became more likely to comply with property taxes in the future and slightly more likely to have paid property taxes. We cannot necessarily conclude that actual or intended payment of taxes increased because of improved government satisfaction, though these respondents also became more likely to believe that Zomba City Council knew about and would punish tax noncompliance. Plausibly, citizens using the new truck and skips believe that the new services signaled that the city had increased its resources and/or its desire to punish tax noncompliance.

³²This analysis is described in our pre-analysis plan.

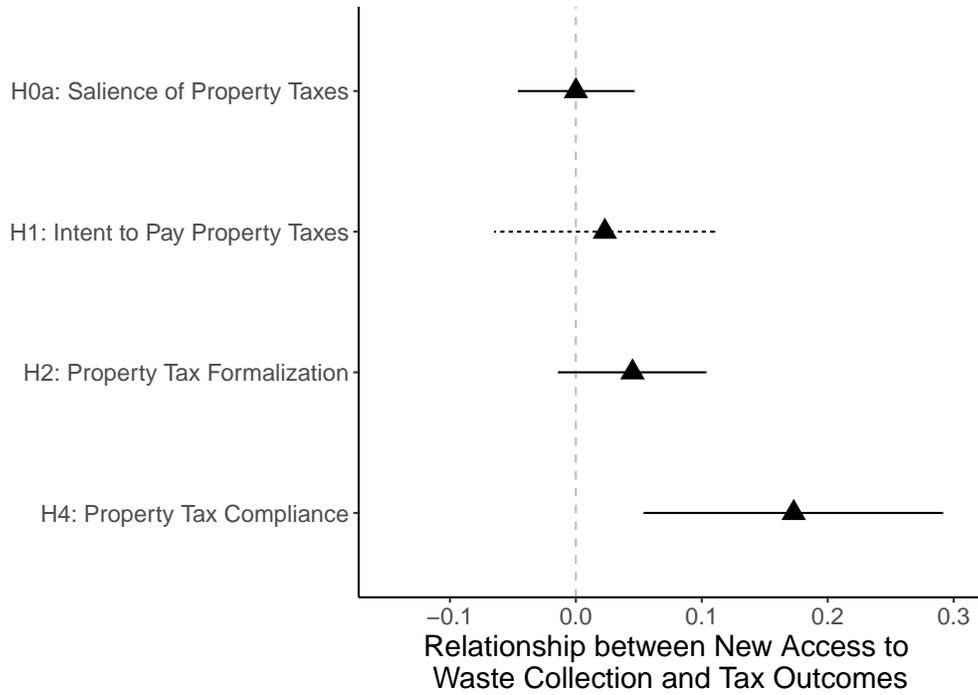


Figure 14: Relationship between new use of government waste collection and tax outcomes. Estimated coefficient and 95% confidence intervals shown (▲ represents Equation 2 without block weights; — represents regression with baseline outcome as a covariate, except Tax Compliance which does not have a baseline measure; --- represents regression with change score as outcome).

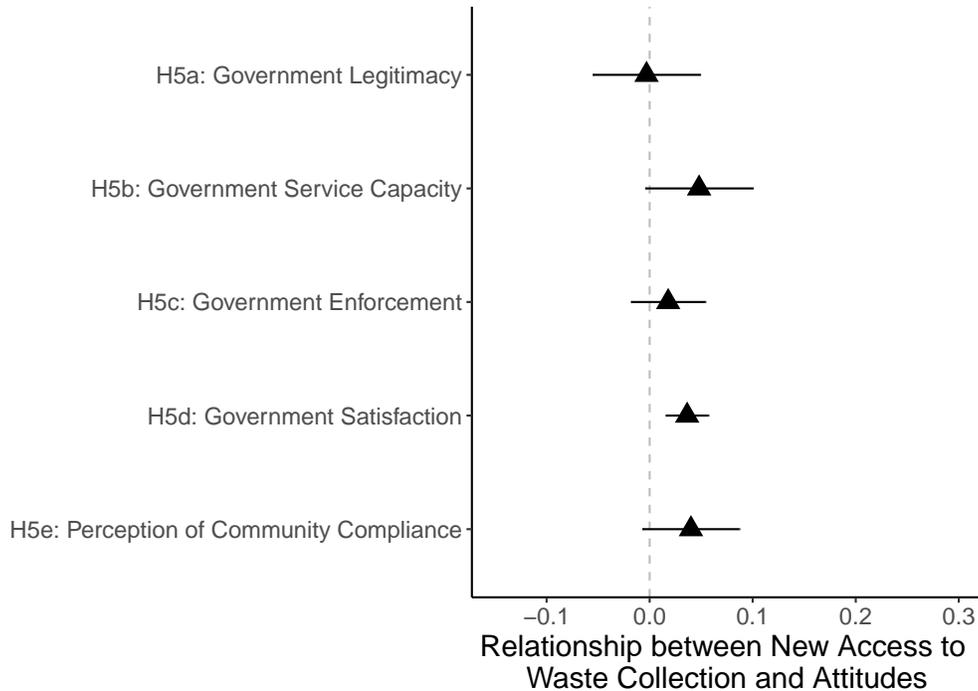


Figure 15: Relationship between new use of government waste collection and attitudes. Estimated coefficient and 95% confidence intervals shown (▲ represents Equation 2 without block weights, — represents regression with baseline outcome as a covariate).

This observational analysis suggests that at least some citizens think about taxation and service delivery as an exchange between citizens and government. Citizens provide the money, government provides the goods. We consider the implications of these results in the discussion.

10 Discussion

Our study supports previous research showing that information about the linkages between taxation and service delivery alone is not enough to convince individuals to pay taxes. While the information campaign did provide individuals with more information about property taxes and the shock to waste collection provided the service to many more households in Zomba, our experimental study that focuses on information alone did not allow us to detect substantively meaningful results for property tax payments or attitudes toward the government.

We further study why the information campaign lacked effects by diagnosing spillover and inconsistencies in the implementation of the intervention. We find some indications that implementation issues and spillover contributed to small and null effects. City staff and non-city staff who stayed on-script delivered information more effectively to neighborhoods; homeowners in those neighborhoods were also more likely to talk to each other and therefore create spillover effects on outcomes like intent to pay property taxes and property tax compliance. As a result, any effects that might have been detectable would have been canceled out because treatment and control households looked very similar in those neighborhoods by the time outcomes were collected. In addition, city staff who went off-script may have deterred households from making property tax payments, countering the positive effects of on-script campaigners.

These implementation and spillover issues should direct future researchers to think carefully about the delivery of interventions, especially service delivery interventions. Specifically, researchers should explicitly model spillover and account for inconsistencies in the implementation of interventions. The delivery itself can induce differential effects on respondents, and spillover is almost inevitable when delivering an

intervention to some households in a geographic area but not others. Service delivery interventions are especially difficult in this regard because most government services are, by their nature, public and non-excludable. Due to the inevitability of spillover and the non-excludable nature of public services, effects of service delivery may be better studied at a level higher than that of the household. These considerations are important for scholars and policymakers as they think about how to deliver information about policy to the public.

In addition to adding another rigorously designed study to the discussion of information and tax compliance, our study provides space to generate new hypotheses about encouraging citizens to pay taxes. Although our informational intervention did not increase tax payment, the information campaign increased informational outcomes and informational outcomes correlate strongly with tax payments and positive government attitudes. Given that relationship, one may expect that our intervention had an effect among respondents who gained information from it. We show, however, that respondents who gained more information from the information campaign were not more likely to pay taxes or improve their attitudes towards the government.

One reason for this null result may be that unobserved traits prompt people to acquire knowledge about taxes, positively evaluate the government, and pay their taxes.³³ For example, more publicly-minded individuals may seek information about paying taxes, perceive the act of paying taxes as less burdensome, appreciate government services, and comply with taxes more than average. In contrast, individuals who gained knowledge through our intervention had that knowledge imposed on them; they are no more publicly minded than before the intervention and therefore no more inclined to pay taxes or appreciate the government. “Information” per se might have no effects because, in the real world, people acquire information for certain purposes — information acquisition requires motivation, and perhaps it is the motivation more than the information which should be targeted in future studies.

In contrast to the null effects of information, citizens who began using the new waste collection service improved their evaluations of the government, were more likely to intend to comply with future taxation, and were more likely to believe the government could and would punish tax noncompliance. Though these results are only correlational and exploratory, this pattern of change could signal that some citizens think about taxes as an exchange for government services. Supporting the idea, respondents who had experiences with off-script city staff were less likely to both pay taxes and use services. A negative interaction with the government persuaded them to forego a service they valued, perhaps because they felt their use of that service could justify the government’s taxation. Conceivably what citizens believe is that the city is offering this trade, but that one can avoid the obligation to pay taxes by not using city services.

An anecdote from our fieldwork further corroborates the idea that some citizens think that the city is offering to trade services for taxes. After the city placed the eight skip containers, some citizens flipped one upside down. That is no small feat – these containers weigh hundreds of pounds. When the researchers went to investigate, the skip had been re-flipped by other citizens. While talking with people in the area, we learned that the skip-tippers believed the city could make residents in that area pay taxes if the residents used city services; they tipped the skip so that people in their area could not benefit from city services, which would prevent the city from levying taxes in their area. Other people tipped it back because they thought the new waste disposal was worthwhile.

If citizens think they are trading taxes for use of services, that offers hope and poses risks for governments. The hope is that providing services can start a virtuous cycle: new services generate new revenue, which can pay for new services, which generate further revenue. The risk is that a temporary halt in

³³Another explanation for the lack of relationship between complying with treatment and tax outcomes are statistical. While assignment to treatment increased informational outcomes and the effect is detectable, the size of the change is small (8 percentage points). Treatment assignment is therefore a weak instrument for gaining information and its weakness may prevent us from detecting the complier average causal effect. However, we find this explanation unlikely. The only CACE we predict with reasonable precision, tax formalization, has a negative coefficient. Even a more powerful instrument cannot detect an effect that is not there.

service provision can start a vicious cycle: diminished services reduces revenue, which further diminishes services, which further reduces revenue.

Though not formally part of our study, we believe that the city's taxation regime created structural barriers that further hindered payment of taxes that our intervention was unable to address. Specifically, it is possible that the intervention did not increase property tax payment because it did not assist the city in soliciting tax payments from citizens. Only about 45% of our survey respondents reported that the city solicited them to pay taxes, and it is not reasonable to expect citizens to pay a tax bill when the bill does not arrive. It is possible that the intervention increased the willingness of respondents to pay taxes, but that respondents did not get the opportunity to display that willingness because other barriers prevented them from closing the action-intention gap. This general lack of tax collection and tax payment can also deter payment by individuals who do receive a tax bill and would otherwise be willing to pay. No one wants to be the sucker paying taxes while her/his neighbors avoid taxes with impunity.

In the end, none of our analyses provide compelling evidence that information, access to services, or a combination of the two increase tax payment. Even citizens who began using new waste collection, who expressed more satisfaction with the government and more intent to comply with taxation in the future, were not detectably more likely to pay taxes than other citizens. This suggests that there may remain additional barriers to tax payment that we have not studied here.

A Appendix

A.1 Additional Hypotheses

The Taxation Round of the Metaketa Initiative included two additional hypotheses not directly relevant for our paper. We consider these exploratory hypotheses.

- **H₆: The treatment will increase citizen's access to other public services among treatment households compared to control households.**
- **H₇: The treatment will improve citizen's tax morale more generally, and increase willingness to pay taxes not directly involved with the formalization process.**

Since the information campaign highlights the role of the city government in providing services generally and the relationship between services and taxation more generally, we expected that the information campaign might (a) lead respondents to seek out other services provided by the city and (b) generally increase tax morale and willingness to pay other taxes not specifically mentioned in the information campaign.

A.2 Measurement

| Hypothesis | Outcome Type | Outcome (Y) | Measures | Measures Variable Type |
|---|------------------------|--|--|-------------------------------|
| H_0 : Increase in information about city rates among treatment households vs. control households. (Confirmatory) | Primary | Information Awareness | “Do you think you are required to pay city rates?” (1 if yes; 0 if no) | Binary |
| H_{0a} : Increase in salience of city rates among treatment households vs. control households. (Exploratory) | Exploratory | Salience of Property Taxes | “What issues were discussed at that the community meeting?” (1 if city rates mentioned; 0 otherwise; NA if did not attend meeting) | Binary |
| H_1 : Increase in intent to formalize among treatment households vs. control households. (Confirmatory – Formalization Hypotheses Family) | Primary | Intent to Formalize | “Do you intend to pay city rates [the next time they are collected]?” (1 if yes; 0 if no) | Binary |
| H_2 : Increase in formalization among treatment households vs. control households. (Confirmatory – Formalization Hypotheses Family) | Primary (Supplemental) | Formalize with city government compiled score (admin data + self-report, 0 if admin data and self-report are 0; 1 otherwise) | Administrative data for city rates payment by household for July-September 2019 payment period. (1 if any paid; 0 otherwise); “Did you pay city rates the most recent time the tax was due?” (1 if yes; 0 if no) | Binary |
| H_3 : Increase in citizen’s access to public services tightly related to the formalization process among treatment households vs. control households. | Primary | Public services access | “Does your household have access to trash collection? Who collects your trash? How do you currently dispose of your trash?” (1 if city government collection; 0 otherwise) | Binary |
| H_4 : Increase in tax compliance among treatment households vs. control households. (Confirmatory – Formalization Hypotheses Family) | Primary | Compliance with city government | “Do you intend to pay city rates [the next time they are collected (January-March 2020)]?” (1 if yes, 0 if no) | Binary |

| | | | | |
|--|-------------|--|---|---------|
| | | | “Imagine that the local market had been badly damaged by fire. Do you think the local government would fix this problem within three months?” (1 if yes; 0 if no) | Binary |
| H_{5c} : Increase belief in city enforcement capacity among treatment households vs. control households.(Exploratory) | Exploratory | Government Enforcement Capacity Index (Additive) | “Do you think Zomba City Council knows who pays and does not pay City Rates?” (1 if yes; 0 if no) | Binary |
| | | | “How likely is it that Zomba City Council will punish people who do not pay City Rates?” (1 if yes; 0 if no) | Binary |
| | | | “How much do you trust Zomba City Council?” (No trust at all, No trust, Trust, Trust a lot, Refuse) | Ordinal |
| H_{5d} : Increase in satisfaction with city government among treatment households vs. control households (Exploratory) | Exploratory | Government Satisfaction Index (Additive) | “How much do you trust your local ward councilor?” (No trust at all, No trust, Trust, Trust a lot, Refuse) | Ordinal |
| | | | “How much do you trust local tax collectors?” (No trust at all, No trust, Trust, Trust a lot, Refuse) | Ordinal |
| | | | “Imagine that Zomba City Council asked citizens to attend a meeting in your area on an important topic. How likely or unlikely would you be to participate?”(Very unlikely, Unlikely, Likely, Very likely, Neither likely nor unlikely, Refuse) | Ordinal |

| | | | | |
|---|-------------|--|--|------------|
| | | | <p>“Now I would like to ask you what you think Zomba City Council will do with the money it receives from City Rates. Imagine that these marbles represent the Kwacha that Zomba City Council receives in City Rates. How many of these marbles will be used to provide services to all its constituents? And how many will be stolen or wasted by public officials?” (Proportion: Services/Total)</p> | Proportion |
| | | | <p>“How do you evaluate the work of the Zomba City Council?” (Very good, Good, Bad, Very bad, Neither good nor bad, Refuse)</p> | Ordinal |
| | | | <p>“How do you evaluate the work of the Zomba Mayor?” (Very good, Good, Bad, Very bad, Neither good nor bad, Refuse)</p> | Ordinal |
| | | | <p>“How do you evaluate the work of your local ward councilor?” (Very good, Good, Bad, Very bad, Neither good nor bad, Refuse)</p> | Ordinal |
| <p>H_{5e}: Increase belief that others pay city rates among treatment households compared to control households. (Exploratory)</p> | Exploratory | Social Norms | <p>“Out of 10 people in this neighborhood who are supposed to pay City Rates, how many do you think actually pay the City Rates owed to Zomba City Council?” (Proportion: people/10)</p> | Proportion |
| | | | <p>“Does your household have access to sewage disposal? How do you dispose of your sewage?/Who disposes your sewage?” (1 if response includes government; 0 otherwise)</p> | Binary |
| <p>H_6: Increase in citizen’s access to other public services among treatment households compared to control households. (Exploratory)</p> | Exploratory | Access to Public Services Index (Additive) | <p>“Does your household have access to piped water in the home?” (1 if yes; 0 if no)</p> | Binary |

| | | | | |
|---|-------------|--|---|---------|
| | | | “Does your household have access to electricity in your home? Which sources of electricity do you use?” (1 if response includes government/ESCOM; 0 otherwise). | Binary |
| | | | “Does your household have access to public transportation within walking distance?” ³⁴ (1 if yes; 0 if no) | Binary |
| | | | “Does your household have access to healthcare?”(1 if yes; 0 if no) | Binary |
| | | | “Does your household have access to education?”(1 if yes; 0 if no) | Binary |
| | | | “Does your household have access to a retirement fund?”(1 if yes; 0 if no) | Binary |
| | | | “Which of these two statements is closer to your own view? Statement 1: Citizens should always pay their taxes and fees, even if they disagree with the government or its actions. Statement 2: Citizens should only pay their taxes and fees if they agree with the government or its actions. How strongly do you prefer the statement you just picked?” (0 - Statement 2, not strong; 1 - Statement 2, strong; 2 - Statement 1, not strong; 3 - Statement 1, strong) | Ordinal |
| <i>H₇</i> : Improvement in citizen’s tax morale more generally, and increases willingness to pay taxes not directly involved with the formalization process. (Exploratory) | Exploratory | Tax Morale Index (Additive & ICW, endline only ³⁵) | | |
| | | | “Did you pay Income Tax the most recent time the tax was due?” (1 if yes; 0 if no) | Binary |
| | | | “Did you pay Vehicle Tax the most recent time the tax was due?” (1 if yes; 0 if no) | Binary |

³⁴Public transportation is not provided by the government in Malawi. This question is included because other countries in the Metaketa Taxation II project will ask this question

³⁵Income tax, vehicle tax, business license, and market vendor fees questions are asked at endline only.

“Did you pay business license fees the most recent time the tax was due?”(1 if yes; 0 if no) Binary

“Did you pay market vendor fees the most recent time the tax was due?”(1 if yes; 0 if no) Binary

Table A1: : Hypotheses, Outcomes, and Measures

| Outcome [Range] | Mean | SD |
|--|------|------|
| Access to Services (ICW) [0.11, 1.00] | 0.59 | 0.14 |
| Tax Morale Index (Additive) [0.00, 1.00] | - | - |

Table A2: Descriptive statistics for other outcome measures at baseline. Many of the questions for the tax morale index were asked at midline only and therefore are not available at baseline. N=1,806.

A.3 Main Results: Other Outcomes

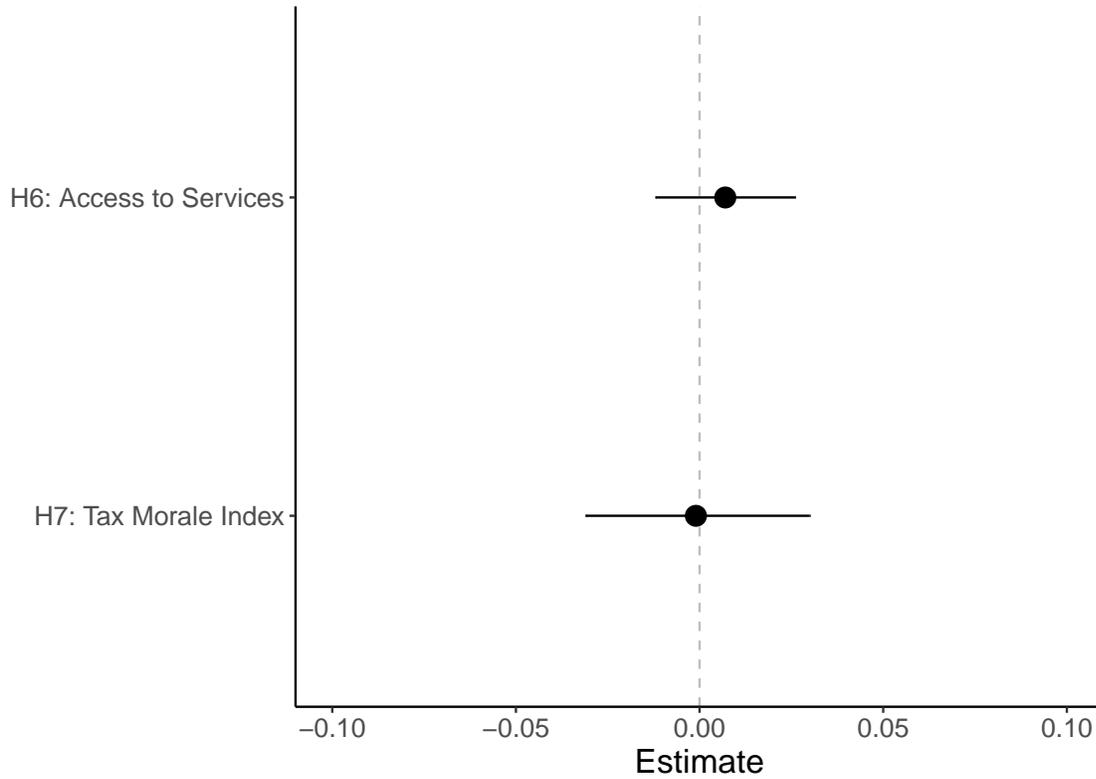


Figure A1: Effect of information campaign on other outcomes. Estimated intent to treat effects and 95% confidence intervals shown (● represents Equation 1).

A.4 Access to Waste Collection and Information and Tax Morale Index Outcomes



Figure A2: Relationship between new use of government waste collection and information and tax morale. Estimated coefficient and 95% confidence intervals shown (▲ represents Equation 2 without block weights, — represents regression with change score as the outcome, --- represents regression with no baseline outcome).

A.5 Campaigner and Spillover Results: Other Outcomes

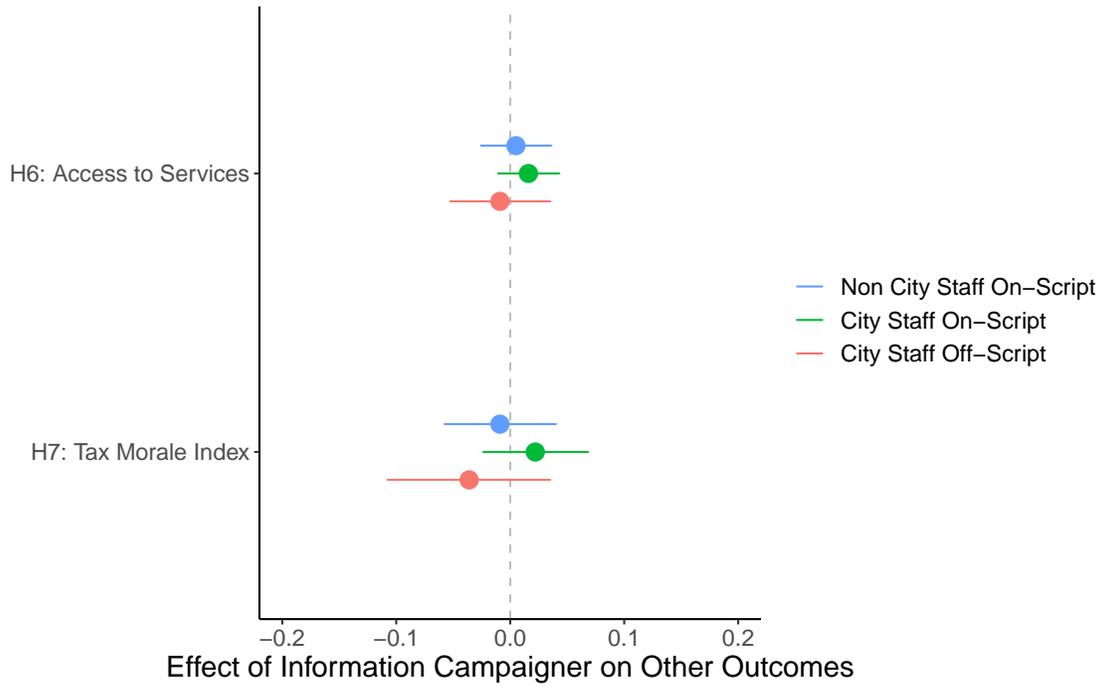


Figure A3: Effect of information campaigner on other outcomes. Estimated intent to treat effects and 95% confidence intervals shown (• represents Equation 1).

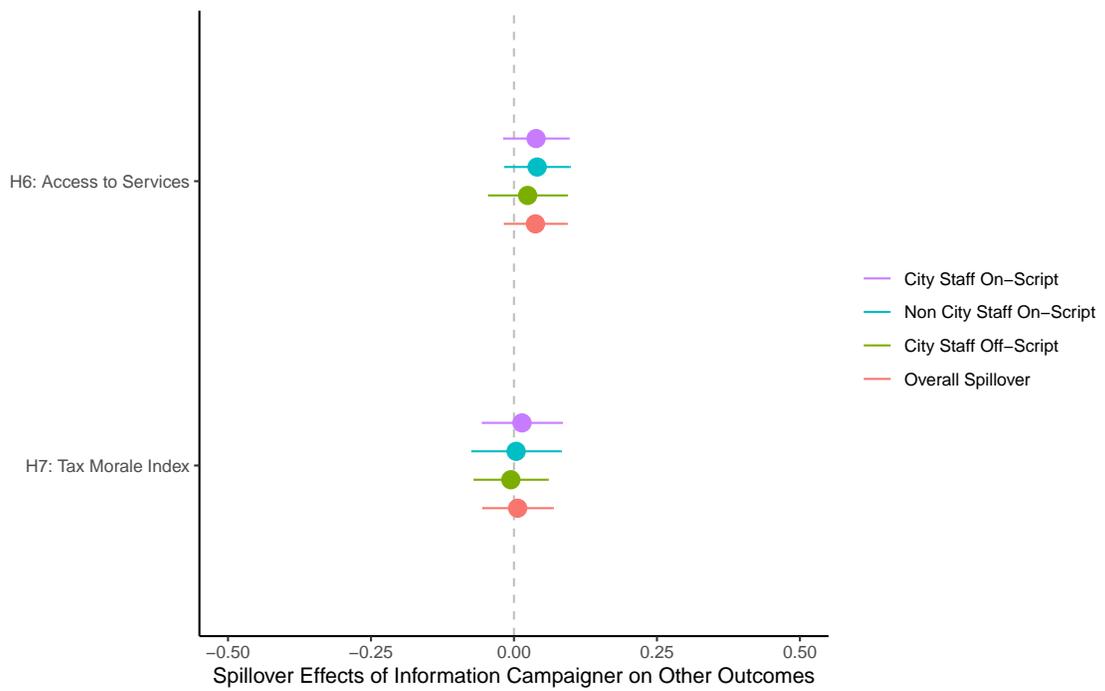


Figure A4: Spillover effect of information campaign on other outcomes. Estimated intent to treat effects and 95% confidence intervals shown (• represents Equation 1 without block-size weights).

A.6 Heterogeneous Effects

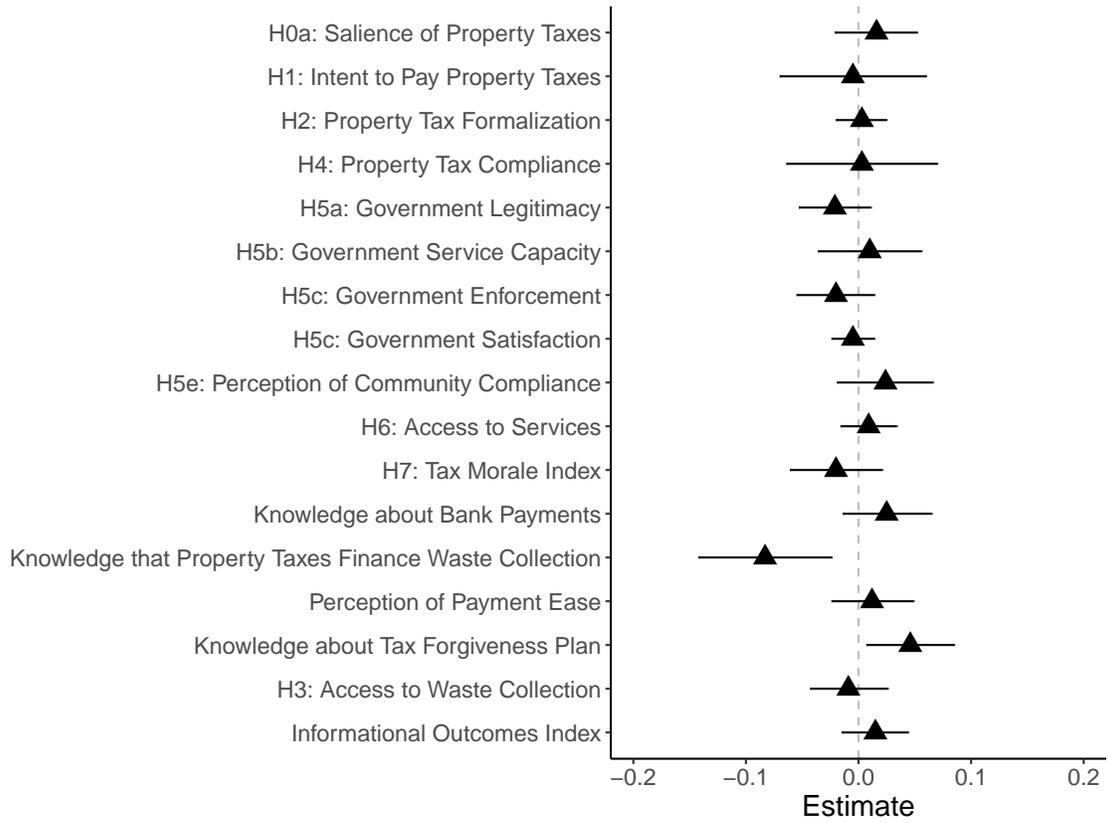


Figure A5: Effect of information campaign on outcomes among households with low information about property taxes at baseline. Estimated intent to treat effects and 95% confidence intervals shown (▲ represents Equation 2).

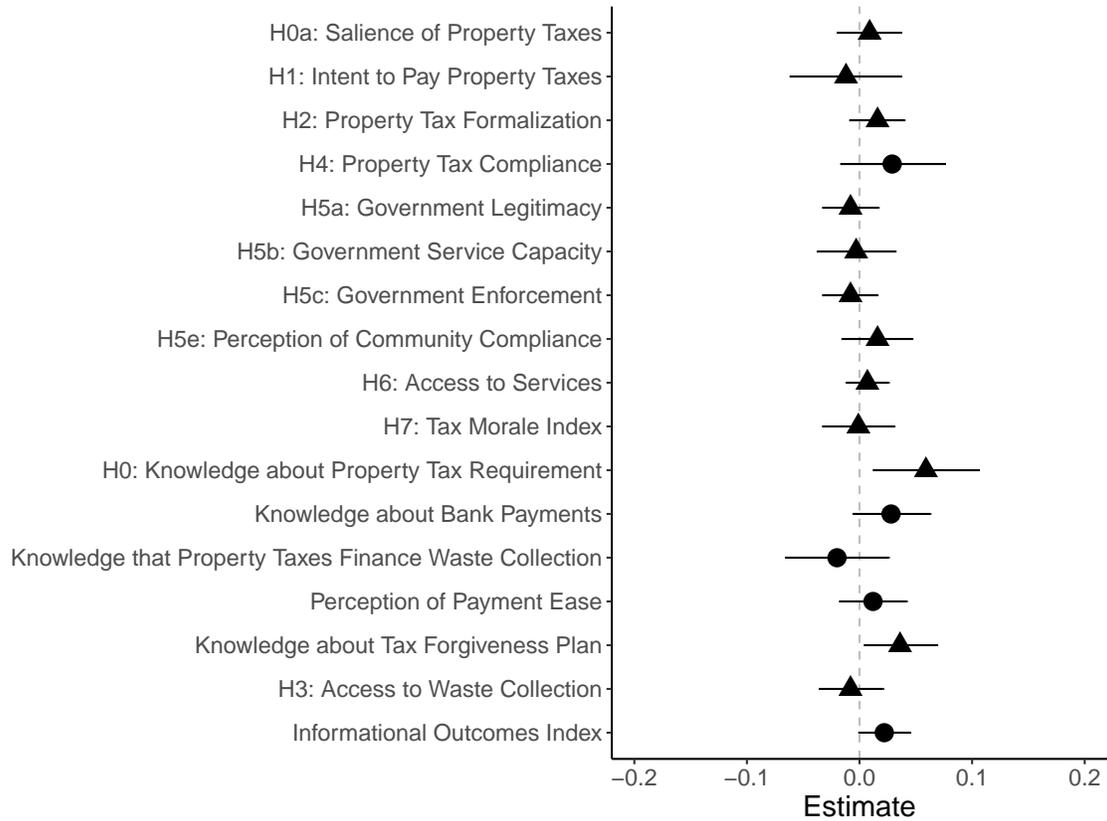


Figure A6: Effect of information campaign on outcomes among households with low government satisfaction at baseline. Estimated intent to treat effects and 95% confidence intervals shown (● represents Equation 1; ▲ represents Equation 2)

A.7 Informational Outcomes and Tax and Government Attitude Outcomes

Table A3 shows how the information outcomes index at midline predicts tax outcomes and Table A4 shows the relationship with the government attitude outcomes.

| | Saliency | Intent | Formalize | Compliance |
|------------------------------|-----------------|-------------------|-----------------|-------------------|
| Informational Outcomes Index | -0.02 (0.03) | 0.63*** (0.05) | 0.08* (0.03) | 0.66*** (0.05) |
| Intercept | 0.08* (0.03) | -0.01 (0.04) | -0.03 (0.03) | -0.05 (0.05) |
| R ² | 0.04 | 0.27 | 0.53 | 0.19 |
| Num. obs. | 826 | 1315 | 1305 | 1213 |

Table A3: Relationship between intervention outcomes index and tax outcomes. Baseline outcomes, baseline socioeconomic index, education, and gender are included as covariates for each model, except Tax Compliance which does not have a baseline measure.

| | Government Legitimacy | Government Service Capacity | Government Enforcement | Government Satisfaction | Perception of Community Compliance |
|------------------------|--------------------------|-----------------------------------|---------------------------|----------------------------|--|
| Informational Outcomes | 0.22*** (0.02) | 0.14*** (0.04) | 0.17*** (0.02) | 0.09*** (0.02) | 0.18*** (0.03) |
| Intercept | 0.34*** (0.03) | 0.16*** (0.03) | 0.59*** (0.03) | 0.34*** (0.02) | 0.29*** (0.03) |
| R ² | 0.13 | 0.11 | 0.06 | 0.20 | 0.07 |
| Num. obs. | 1425 | 1394 | 1407 | 1425 | 1425 |

Table A4: Relationship between intervention outcome index and government attitudes indices. Baseline outcomes, baseline socioeconomic index, education, and gender are included as covariates for each model.

A.8 Causal Average Complier Effect

Table A5 shows CACE results for tax behavior. Table A6 shows CACE results for government attitudes.

| | Salience | Intent | Formalize | Compliance |
|-----------------------|-----------------|-----------------|-----------------|----------------|
| Intervention Outcomes | 0.93 (2.62) | -0.57 (1.51) | 0.92 (1.03) | 0.01 (1.50) |
| Intercept | -0.51 (1.55) | 0.63 (0.82) | -0.51 (0.62) | 0.44 (0.94) |
| R ² | -1.18 | -0.12 | 0.15 | 0.01 |
| Num. obs. | 826 | 1315 | 1305 | 1213 |

Baseline outcome included as covariate for each model except Compliance.

Table A5: Complier average causal effects for tax outcomes.

| | Government Legitimacy | Government Service Capacity | Government Enforcement | Government Satisfaction | Perception of Community Compliance |
|-----------------------------|--------------------------|-----------------------------------|---------------------------|----------------------------|--|
| Intervention Outcomes Index | -0.46 (0.79) | -0.19 (0.85) | -0.35 (0.68) | -0.50 (0.56) | 0.94 (0.89) |
| Intercept | 0.68 (0.42) | 0.34 (0.54) | 0.87* (0.37) | 0.65 (0.34) | -0.17 (0.51) |
| R ² | -0.40 | 0.06 | -0.25 | -0.72 | -0.33 |
| Num. obs. | 1425 | 1394 | 1407 | 1425 | 1425 |

Baseline outcome included as covariate for each model.

Table A6: Complier average causal effects for government attitudes outcomes

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