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Essays on Political Economy and Development Economics in Haiti

by

Benjamin E. Krause

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Agricultural and Resource Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Jeremy Magruder, Co-chair

Professor Edward Miguel, Co-chair

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Associate Professor Benjamin Faber

Summer 2024

Essays on Political Economy and Development Economics in Haiti

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## Abstract

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University of California, Berkeley

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Development requires the establishment of a functioning state. To strengthen state capacity, governments in low income countries must raise tax revenue while maintaining political stability. The risk of inciting political unrest when attempting to increase taxes may trap governments in a low-tax equilibrium, but public goods provision may improve both tax compliance and political stability. To test these questions empirically, in Chapter 1, I partner with the national tax authority and a local mayor's office in Haiti to cross-randomize both tax collection and public goods across one of the country's largest cities. Effects are measured both via administrative data on tax revenue as well as through novel measures of political unrest. In the chapter's main result, I show that hand-delivering property tax invoices reduces individual tax compliance by 48%, and increases independently observed measures of localized political violence by 192%. In contrast, providing a valuable and visible public good (namely municipal garbage removal) increases tax compliance by 27%, and reduces localized political violence by 85%. Importantly, public goods provision significantly mitigates the adverse effects of tax collection in neighborhoods receiving both treatments. A cost accounting exercise suggests that providing the public good in this setting could pay for itself within the first year. These findings suggest that it may be possible to peacefully shift to a new equilibrium of higher tax compliance with a sufficient initial investment in public goods provision perhaps financed through foreign aid or other transfers.

State formation also requires navigating the at times competing interests of society and the state. While social norms have been successfully leveraged to support governments in established states, different norms may exist in settings of early state formation characterized by low rates of pro-social coordination and limited state credibility. Appeals to social norms in such settings may result in backlash as society reveals itself to be an adversary to state formation. Conversely, the government may be able to shift prevailing norms by first providing relevant services. I test these questions empirically in Chapter 2 by extending the experiment in the previous chapter with individual-level randomization over the universe of properties in my study city's tax registry. Effects are measured by matching previously siloed

administrative data. Through extensive original qualitative and quantitative data collection, I document differences in social norms and develop novel corresponding treatments. In the chapter's main result, I show that appealing to social norms by increasing social exposure of the tax compliance decision further reduces individual tax compliance beyond the effects found in the previous chapter leading to a net loss of 66%. I find evidence that this negative effect potentially reverses when randomly combined with receiving an increase in public services. I interpret this as evidence of antisocial punishment, a mechanism from psychology that I introduce to the tax literature. I indirectly test other motivations for tax compliance by randomizing the framing of tax collection engagement. I find evidence that each appeal tested reduced the adverse tax collection response even in the absence of public goods. The largest response I find is from appealing to local proverbs that make salient obligations and shared responsibility which result in net tax payment increases of 40%. A cost accounting exercise cautions that the appeal to social exposure could have lost the city 3/4 of its property tax revenue but that the culturally relevant appeal to obligations could have resulted in \$210,000 in additional annual revenue. These findings highlight the need for the state to learn when it can rely on society's support as an ally and how to avoid society's backlash as an adversary.

Due to the acute challenges of governance in low income countries, donors are increasingly promoting public-private partnerships to provide public goods. However, these hybrid arrangements create a different set of governance challenges as officials seek to hold private sector actors accountable for delivery. In Chapter 3, I examine one such effort in Haiti where the government attempted to achieve universal primary education by providing 200 million USD in subsidies directly to education entrepreneurs. I find evidence that the program's roll-out coincided with a more than 30 percentage point increase in primary school enrollment and use a cohort study to identify causal evidence of both increased enrollment as well as improvements in household welfare. I make use of multiple novel datasets and a natural experiment to find that audited participants reduced grant claims by 18.9% – interpreted as a reduction in fraud – resulting in a directly observed savings of 875,000 USD per year for a conservative return of 11 USD for every dollar spent on auditing. I do not find any evidence of spillover effects. I provide additional support for my findings through an event study. These findings suggest that public-private partnerships can be a means for low-capacity states to quickly increase public goods provision, and though such an approach risks substantial fraud, relatively simple to implement interventions can cost-effectively mitigate some of those losses.

To Samantha, Satya, and Isaiah

Nou se lapli ki te leve mayi sa a.  
Life is better with you.

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All errors are my own.

# Chapter 1

## Balancing Purse and Peace

### *Tax Collection, Public Goods, and Civil Unrest*

#### 1.1 Introduction

*Little else is requisite to carry a state to the highest degree of opulence from the lowest barbarism, but peace, easy taxes, and a tolerable administration of justice.*

—Adam Smith

*It's not in our habit to pay taxes. Haitians haven't been educated to pay taxes  
... Who here can throw the first stone?*

—(now former) Prime Minister Jean-Henry Céant

State capacity has been a central focus of the literature on economic development ([Fukuyama, 2011](#); [de Janvry and Dethier, 2012](#); [Acemoglu et al., 2012](#); [Thomas, 2015](#)), and strengthening it requires effective taxation ([Pomeranz, 2015](#)). Two levers supported by the literature and available to policy makers to increase tax compliance are tax collection effort itself and public goods provision ([Besley and Persson, 2009](#)). Theory suggests that political instability and the threat of violence in response to efforts to increase taxes may trap governments in a low-tax equilibrium ([Besley and Persson, 2013](#)). Other work has predicted that public goods provision may improve political stability ([Acemoglu, 2005](#); [Besley and Persson, 2011](#)). This chapter tests the effects of both tax collection and public goods, as well as their interaction, on both tax compliance and political unrest by cross-randomizing these interventions at-scale and for multiple years across one of the largest cities in Haiti.

Though the empirical literature has explored some aspects of these relationships separately ([Khan et al., 2015, 2019, 2020](#); [Pomeranz and Vila-Belda, 2019](#)), challenges of implementation, setting and measurement have limited their full experimental examination. This chapter overcomes implementation and setting issues by combining international assistance and multiple overlapping partnerships with government offices in one of the world's

lowest-capacity states<sup>1</sup> to coordinate interventions over several years. The interventions were implemented by the mayor of this city and the national tax authority with additional financial support from the national government and in-kind donations from a sister city in the Caribbean. The design is a stratified cluster cross-randomization of the interventions over all census blocs of the urban city. This includes a population of more than half-a-million people though interventions primarily focused on the owners of the approximately 50,000 buildings in the tax registry. This chapter overcomes the measurement challenges by first identifying, accessing and matching previously siloed and uncoordinated data sets to measure tax compliance in this low-capacity setting. Then to independently measure political unrest and violence, this chapter leverages findings from qualitative data collection to introduce two sets of novel metrics. To measure political speech, I census and geo-locate all graffiti in the city and estimate effects on localized political graffiti. To measure the more destructive and violent forms of unrest common in Haiti, I track and estimate effects on the localized construction of protest barricades and roadblocks which are often also lit on fire to magnify the signal of political mobilization in this context.

To motivate and rationalize the empirical analysis, I provide a simple model of the optimal tax evasion decision in a setting of low-compliance. The standard assumption in the theory literature is that increasing tax collection effort with its associated threat of financial penalties unambiguously lowers evasion and increases tax payments (Allingham and Sandmo, 1972a). Empirical evidence has consistently supported this assumption (Slemrod, 2019). Instead, building off of theory developed for low-compliance settings (Frey, 1997) and informed by my qualitative and baseline data, in my model tax collection may potentially crowd-out non-pecuniary incentives which are determined in part by the level of public goods (Besley, 2020). As a result, if public goods are absent and the credibility of the state's threats are low, my model's first prediction is that increasing tax collection effort reduces tax payments. As a result of my modeling the non-pecuniary incentive as dependent on the level of public goods, my model's second prediction is that increasing public goods mitigates the crowding-out effect and increases tax payments.

To test the effects of tax collection, I randomize efforts to increase compliance for the principal source of funding for local governments in this setting, property taxes. The mayor's agents hand-deliver invoices for property taxes to those randomly assigned to receive such visits. In settings with much higher levels of baseline tax compliance, interventions like this one have been shown to have statistically and economically significant positive impacts on tax payments (Pomeranz, 2015; Castro and Scartascini, 2015; Holz et al., 2020). In contrast, I find taxpayers reduce compliance by 48%. This negative effect can similarly be shown across nearly all metrics and is consistent with my model's predictions of enforcement crowding-out non-pecuniary incentives in the absence of public goods or credible penalties.

The literature has shown tax collection increases political engagement. There is experimental evidence of tax collection increasing attendance in local government meetings and

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<sup>1</sup>Consistently near the bottom, in the years leading up to this study, 2014-2017, Haiti ranked in the bottom 1% of the World Bank Governance Indicators for Government Effectiveness <https://databank.worldbank.org/source/worldwide-governance-indicators/preview/on>

submissions to city comment boxes (Weigel, 2020) as well as suggestive evidence of it increasing demands for representation (Ross, 2004) and willingness to punish political officials (Martin, 2013). Building on these findings, I find tax collection significantly increases political unrest and destructive or violent action. Treated households report nearly tripling time spent protesting. I further find increases in independently observed measures of unrest: I find political graffiti doubles in treated areas and becomes more negative in tone. I also find that the localized construction of protest barricades increases by 192%.

To test the effects of public goods provision, I work with the mayor's office to randomize a highly salient service which is also the only public good broadly provided by local government in this setting, door-to-door garbage removal. Suggestive evidence from heterogeneity analysis has indicated that public goods provision may increase tax payments (Castro and Scartascini, 2015). At baseline in my setting, respondents noted both that the lack of public services was their primary justification for not paying property taxes and that increasing public services would be the best way to motivate their compliance. When asked which service should be the government's top priority, a plurality of respondents cited garbage removal. Consistent with these findings and my model's predictions, I provide experimental evidence of public goods increasing tax compliance by 27%. I also find evidence of the effect increasing with the intensity of treatment. Using a reduced form estimate leveraging the quasi-random variation in number of visits by a garbage removal team generated by maintenance needs, gang hijackings and staffing conflicts leading to missed routes on an otherwise randomized schedule, as well as an IV estimate, I find an additional visit by the garbage removal vehicle causes a 1% and 1.3% increase in tax payments respectively.

Experiments randomizing relatively small increases in public goods have found suggestive evidence of increased support for public officials (Blattman et al., 2017). As demonstrated by an incentive-compatible willingness-to-pay experiment at baseline as well as additional survey responses, my public goods intervention is highly valuable and visible. I find evidence that it reduces self-reported protesting by 50% and localized political unrest and violence in the form of barricade construction by 85%. I find evidence that for barricade construction as well as for political graffiti, public goods mitigate the tax collection effects in areas receiving both. I find that the remaining political graffiti in these areas is more positive in tone. Both political graffiti and barricade construction are also found to be affected by increases in intensity of treatment with the former increasing by 2% over the mean and the latter by 3.5% over the mean for an additional visit by a garbage removal vehicle.

I test for spillover effects that might influence the interpretation of my principal findings on tax compliance. Leveraging both randomization at the census bloc-level and as well as at the property-level, I compare the responses of control properties co-located in blocs with other properties receiving the tax collection treatment to those in blocs randomly assigned to not have any properties visited by tax agents. I find evidence supporting the sign, magnitude, and significance of my estimated tax collection effects. Using a novel placebo treatment through which instead of delivering and discussing a tax invoice, government agents deliver a letter with the same personal identification and property information on it but no mention of taxes, I find evidence that visits by government officials alone do not decrease tax payments.

This suggests that the negative tax collection effect is the results of the explicit discussion of taxes during the government agent's visit. I leverage geographic variation to test for spillovers from properties not receiving public goods by first limiting analysis to properties farther away from bloc boundaries and find evidence that my preferred specification may be an underestimate. I then analyze the responses only among blocs not assigned to receive the public goods intervention and compare the responses of properties in blocs adjacent to public goods treatment blocs to those that do not border public goods treatment blocs. The results provide evidence against large negative responses from properties farther removed from the public goods treatment and are consistent with my preferred specification being an underestimate.

Before concluding, I review an accounting exercise to assess the costs and benefits for policy. My findings suggest that even the significant initial capital costs for garbage removal could be repaid from revenue generated - possibly even in the first year. Nevertheless, these settings are by definition poorly resourced, and thus international institutions or better-funded central governments may be required to make these initial investments possible. Lastly, evidence from this chapter indicates that public goods reduce destructive unrest and may also induce pro-social behavior. This suggests that even if not resulting in positive net revenue at first, if these effects over time decrease losses or increase the rate of return, investments in public goods may still yield a positive return for the government. In short, this chapter presents experimental evidence that public goods provision provides a possible pathway for governments to maintain political stability while transitioning to a higher-tax equilibrium – thus balancing purse and peace.

In what follows, I first discuss the most relevant literature and my contributions in 1.2. Then in 1.3, I present the model motivating this research. Section 1.4 explores the context of Haiti and introduces my study city, Carrefour. In 1.5, I describe my methodology and research design. Section 1.6 presents results followed by robustness checks in 1.7. I discuss an accounting exercise of the costs and benefits for the government in 1.8, and I conclude in 1.9.

## 1.2 Literature Review

Understanding how to increase tax revenue, especially in low-tax equilibrium settings, has been identified in the literature as both key to state capacity building and an open area of empirical inquiry (Besley and Persson, 2013). Standard models of tax compliance based on expected utility maximization of evasion given a perceived probability of detection (Becker, 1974; Allingham and Sandmo, 1972a; Srinivasan, 1973) predict much lower levels of tax compliance than observed in the empirical data – especially in developed countries. More recent models incorporating public good provision, social norms, moral sentiments, or other ‘tax morale’ motivations have demonstrated greater ability to more accurately predict compliance rates (Erard and Feinstein, 1994; Torgler, 2005; Kirchler, 2007; Luttmer and Singhal, 2014; Bernheim and Taubinsky, 2018).



Empirical work has successfully increased tax compliance primarily by focusing on tax collection directly. A recent review article concluded that the “incontrovertible” finding of the empirical public literature is that “raising the chance of getting caught high enough deters evasion” (Slemrod, 2019). Field experiments implementing behavioral nudges to induce higher tax participation have shown significant returns (Hallsworth, 2014; Pomeranz, 2015; Pomeranz and Vila-Belda, 2019) even in middle and lower-income countries for both firms (Shimeles et al., 2017; Carrillo et al., 2017; Holz et al., 2020) as well as for individuals (Castro and Scartascini, 2015; Kettle et al., 2016; Hernandez et al., 2017). These effects have been shown to hold even in the few low-compliance settings where work has been done to date (Dwenger et al., 2016; Weigel, 2020; Bergeron et al., 2020). There is also work examining the relative efficiency of different tax policies which are more accessible to lower capacity states which have similarly been successful in increasing compliance through tax collection effort (Khan et al., 2015; Best et al., 2016; Khan et al., 2019).

According to one branch of the literature, the modern democratic state arose from a negotiated bargain of formal representation in exchange for taxes (Levi, 1988; North and Weingast, 1989; Tilly, 1990). Some have even made explicit causal claims that either the level of taxation (Buchanan, 1980; Huntington, 1991) or the ratio of taxation to services provided (Bates and Lien, 1985) leads to an increased demand for representation. Cross-country analyses (Besley and Persson, 2013; Ross, 2004), experiences in weak states with extractive resources (Morrison, 2005), and results from a lab-in-the-field (Martin, 2013) have provided some empirical support to these theories.

However very little has been done to experimentally test these relationships. Related work has shown significant political and electoral effects resulting from cash transfers (Manacorda et al., 2011), windfalls from the central government (Brollo et al., 2013), and interventions to curb tax evasion (Casaburi and Troiano, 2016). More recent experiments have found evidence that increased public service provision may improve public opinion of local leaders (Blattman et al., 2017; Khan et al., 2020), and increased tax collection efforts can increase interactions with the local government (Weigel, 2020). How interventions impact democratic accountability, and in particular the engagement of the poor, remain open empirical questions (Pande, 2020).

Existing experimental work on these topics have generally been limited to settings of relatively high pre-existing compliance within relatively established states. The papers that have attempted to randomize tax collection effort in low-compliance settings have focused primarily on relatively small taxes as a percentage of total income or wealth. In addition, the papers that have randomized public goods have focused on interventions that have gone largely unnoticed by the treated population. I am not aware of any previous work that has randomized both types of interventions in the same study. In this chapter, I randomize both collection effort for a financially significant tax that is the principal source of funding for the local government, hand-delivering invoices for personal property taxes, as well as public goods provision of a highly salient service that is effectively the only public good broadly provided by the local government, door-to-door garbage removal. I do this in a setting of extremely low tax compliance and limited state capacity.

The setting is particularly significant because, with regards to tax compliance, theory and even some empirical work in other disciplines suggest that the incentives faced in low-compliance, low-capacity states may be quite different than in higher-compliance settings. For instance, theory by [Frey \(1997\)](#) cautions that increasing enforcement of government policies in settings that initially have few external incentives may crowd-out the ‘civic virtue’ motivating baseline levels of pro-social behavior and result in a net decrease in the desired compliance. I am able to test for this by randomizing tax collection effort for a financially significant amount of money in just such a setting. [Besley \(2020\)](#) separately models the related concept of ‘tax morale’ that is generally regarded as incentivizing greater compliance to instead be dependent upon the level of public goods provided so that in low-capacity states with little-to-no public goods, the incentive further depresses tax compliance. By randomizing public goods starting from a baseline of effectively zero, I am able to test this theory as well.

Finally, with regards to effects on political unrest, though of significant interest, this sort of behavior has been very difficult to independently measure ([Cantoni et al., 2017](#)). To overcome this challenge, I introduce two novel metrics for independently measuring political unrest. First, to measure political speech, I conduct a census of and geo-tag the graffiti across the city. I then use the presence, prevalence, and tone of political graffiti specifically as outcomes of interest. Second, to measure the most violent or destructive political unrest, I track the construction of barricades in neighborhoods which are built, and often lit on fire, as a form of protest in this setting. Tracking both where these are constructed and which areas are affected provide additional outcomes of interest. As a result, I am able to provide novel experimental evidence of the effects of both tax collection and public goods on political unrest – and on violent or destructive unrest in particular.

## 1.3 Model

Background and intuition for the set-up are provided in [Model Motivation and Set-up](#). The model is formally presented with Equation 1.1, the First Order Conditions in Equation 1.2, and the comparative statics and resulting predictions for my primary research questions regarding tax compliance in [Model Predictions](#). Extensions of the model to further explore proposed mechanisms are presented in the following Chapter in [2.3](#).

### Model Motivation and Set-up

To motivate this research, I model the taxpayer’s decision to evade taxes. I enrich the microfoundations developed for the dynamic model of state capacity building in [Besley \(2020\)](#) by modifying the ‘civic-minded’ incentive so that under the appropriate circumstances the net effects can be crowded-out by enforcement as in [Frey \(1997\)](#).

In my model, the taxpayer chooses a level of evasion,  $e \in [0, 1]$  to maximize the monetary value of the sum of public goods received plus net income:

$$\max_e g + w \left( 1 - (1 - e) \left( t - \tau (g - wt) \right) \right) - \tau f e \quad (1.1)$$

where  $g \geq 0$  are the public goods received and  $w$  is the taxpayer's given level of private income. The actual tax rate set by the government is  $t \in [0, 1]$ .

However the weight of the tax rate, in other words its perceived cost for the taxpayer, is adjusted based on non-pecuniary incentives. The perceived fairness of the tax,  $(g - wt)$ , is determined by subtracting the total tax bill,  $wt$ , from the amount of public goods received,  $g$ . When the taxpayer receives in public goods a monetary value equivalent for her of exactly what she is being asked to pay, then  $g - wt = 0$  and the motivation has no effect on the perceived tax as she is indifferent. However, when she receives more than she is being asked to pay, then  $g - wt > 0$  this motivation decreases the weight of the tax rate as she perceives it to be generous. Conversely, when the taxpayer receives less in public goods than she is asked to pay in taxes,  $g - wt < 0$ , then this motivation increases the weight of the tax as she perceives this tax to be unfair. This incentive is multiplied by the level of effort the government expends on tax collection,  $\tau \in [0, 1]$ , as it has the effect of increasing the salience of the perceived fairness for the taxpayer.

Finally, the model includes a financial penalty when caught evading composed of the perceived fine rate,  $f$ , the level of evasion chosen,  $e$ , and the taxpayer's given private income,  $w$ , the amount of effort the government spends on tax collection,  $\tau$ .

The resulting first order condition (*FOC*) maximizing with respect to evasion,  $e$ , is:

$$FOC_e : e^* = \frac{t - \tau(g - wt) + \tau f}{2} \quad (1.2)$$

## Model Predictions

The comparative statics from totally differentiating the *FOC* reveal the following relationships and corresponding predictions regarding effects on tax evasion,  $e$ .

I begin with the effect of increasing tax collection effort,  $\tau$ . Note that when the tax is perceived to be generous ( $wt - g < 0$ ), compliance is motivated through the sense of obligation. However, when the level of taxation is perceived to be unfair ( $wt - g > 0$ ), the effect on tax evasion motivated by fairness will be positive, encouraging more evasion. Increasing tax collection effort also increases the perceived penalty faced when evading, which would have the opposite effect of discouraging evasion. The ambiguity of the resulting net effect can be seen in the comparative static:

$$\frac{\partial e}{\partial \tau} = \frac{(wt - g) - f}{2} \quad (1.3)$$

The sign on this relationship is dependent on the relative magnitudes of the total tax bill,  $wt$ , public goods received,  $g$ , and fine rate,  $f$ . Specifically, for tax collection effort to increase

compliance, the combined level of public goods and financial penalties must be sufficiently high so to offset the tax bill. However, in low-tax compliance states, the level of public goods received is quite low or even zero. Similarly, the perceived fine faced may also be approaching zero if the state lacks credibility or capacity to enforce punishment when an evader is identified. With these simplifying assumptions, the comparative static becomes:

$$\frac{\partial e}{\partial \tau} = \frac{wt}{2} > 0 \quad (1.4)$$

and the resulting prediction can be summarized:

**In the absence of public goods or credible fines, increasing tax collection effort *increases* evasion.**

This leads directly into my second research question, the effect of increasing public goods,  $g$ , on tax evasion,  $e$ . Building off of the intuition discussed above regarding the perceived fairness of the tax, the resulting comparative static is:

$$\frac{\partial e}{\partial g} = -\frac{\tau}{2} < 0 \quad (1.5)$$

and the resulting prediction can be summarized:

**Increasing public good provision *decreases* evasion.**

Additional predictions for exploring mechanisms are discussed in the following Chapter in [2.3](#). Evidence from both qualitative data collection and my baseline survey supporting the modeling assumptions made here are also presented in the following Chapter in [2.4](#).

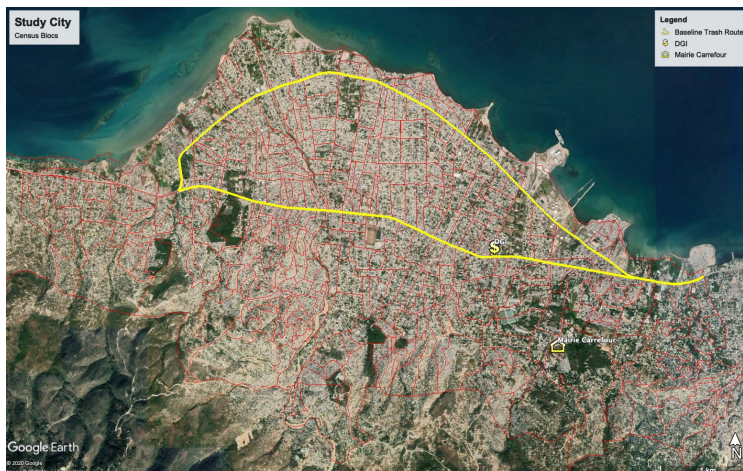
## 1.4 Context

Haiti's history and institutions are briefly summarized in first section [History and Institutions](#). The study city is introduced and baseline summary statistics are then presented in [Study City](#).

### History and Institutions

Haiti's political history has been characterized by violence, instability, poverty and an antagonistic relationship between the state and its citizens ([Girard, 2010](#); [Dubois, 2012](#)). Famously established via a slave rebellion, the country has since struggled to consolidate democratic institutions due to international exclusion, interference and occupation as well as domestic in-fighting, corruption and, for much of the mid-twentieth century, autocratic rule. Since

Figure 1.1: Map of Study City and Units of Randomization



Red lines indicate census bloc boundaries which are the first level of randomization in the study. DGI is the national tax authority local office where taxes are paid. Mairie Carrefour is the Mayor's Office. The bold yellow line traces the national highway running through downtown as well as the ring road that bypasses it. Prior to the start of the intervention, these were the only roads receiving regular garbage removal services and residents were responsible to get their trash to the road.

emerging from dictatorship in 1988 with a new constitution, the country has suffered a series of coups, prolonged periods of political unrest and failed democratic transitions (Farmer, 2005). In the intervening three decades, there has been only one peaceful handover of power between political parties via election and the average Prime Minister has served less than 18 months before being ousted - often in the face of unrelenting protesting and political unrest (Maguire and Freeman, eds, 2017). Today, Haiti is one of the poorest countries in the world<sup>2</sup>. It also suffers among the lowest levels of government effectiveness ranked by the World Bank as being in the bottom 1%. Among countries in the Western Hemisphere, Haiti has the lowest levels of trust in state institutions, participation in elections and levels of taxation (Cohen et al., 2017). Non-state institutions in Haiti have proven more resilient. Haiti has the highest level of civic engagement in the Western Hemisphere (Cohen et al., 2017). Survey responses indicate that households contribute significant portions of their resources in terms of money, materials and time to informal provision of public goods (Figures A1 and A2).<sup>3</sup>

## Study City

My study city is Carrefour, Haiti (Figure 1.1). It is one of the six (6) independently governed municipalities that make up the greater Port-au-Prince metropolitan area. It has an estimated population of between 500,000-1,000,000 people making it one of the largest cities in a country of 10 million total inhabitants. There are approximately 50,000 properties in the tax registry, though the mayor’s office estimates that this potentially represents less than half of the total built property in the city. Though globally poor with median monthly per capita household expenditures of about \$65 USD or just over \$2 USD/day (Figure A3), the city is considered middle class by Haitian standards with many professionals and trades persons who commute into the capital each day.

In 2011, with funding from the United States Agency for International Development (USAID) the Government of Haiti launched a digital system for municipal finance oversight. The focus has been on individual property taxes which represent the primary source of municipal tax revenue and effectively the only progressive tax in Haiti. The current payment rate in the study city is officially about 10% though estimated to be about half that when unregistered properties are also considered, and the new mayor, eager to increase tax compliance, outsourced promotion of tax payment to the private Haitian data science firm that USAID originally contracted to build and implement the system, [SOLUTIONS, SA](#).

Table 1.1: Property Tax Brackets

Tax Bracket	Min Value USD	Max Value USD	Tax Rate	# Properties	% Properties
1	\$ 257	\$ 769	6%	2,403	6%
2	\$ 770	\$ 1,538	7%	33,650	84%
3	\$ 1,539	\$ 2,307	8%	1,602	4%
4	\$ 2,307	\$ 3,076	9%	1,402	3%
5	\$ 3,077	NA	10%	1,103	3%

All property values are in whole USD (calculated using 65HTG : 1USD). Property values are estimated market return for one year of renting the property as determined by a uniform calculation performed by the municipality each year which takes as input data collected on the housing market and a set of observable criteria for each property including square meters of space, number of rooms, location in city, access to street, water, power, construction materials, and roofing materials.

Property taxes are the principal source of funding for local governments and the only effective progressive tax in the country. As can be seen in Table 1.1, properties pay an increasing percentage of that estimated annual market rental rate for the property starting

<sup>2</sup>All reports of Gross Domestic Product (GDP) Purchasing Power Parity (PPP) in this chapter are in 2018 US Dollars as calculated by the World Bank. Accessed 29 April 2018: <https://data.worldbank.org/indicator/NY.GDP.PCAP.PP.CD?locations=ET-US-CL-AR-HT-CD-IT>.

<sup>3</sup>Indeed, my qualitative work identified more than a dozen different ways that people talk about or refer to collective action activities in my research city. The most common being: “kombit”, “kove”, “douvannjou”, “kolon”, “tet ansanm” and “team”. The concept is roughly analogous to the Kenyan “harambee” more commonly referenced development economics.

Table 1.2: Administrative Tax Data Summary Statistics

Statistic	Mean	Median	N
Water Access	0.60	1.00	33,763
Electricity	0.71	1.00	33,763
Road Access	0.70	1.00	33,763
Located on Bloc Boundary	0.31	0	40,060
Improved Roof	0.57	1	40,060
Number of Floors	0.18	0.00	33,763
Paid FY17	0.11	0	40,060
Paid FY18	0.12	0	40,060
Rental Value/Yr (USD)	1,246.87	769.23	40,060
Tax Rate	0.07	0.07	38,601
Tax Bracket 1	0.06	0	40,060
Tax Bracket 2	0.84	1	40,060
Tax Bracket 3	0.04	0	40,060
Tax Bracket 4	0.03	0	40,060
Tax Bracket 5	0.03	0	40,060
Total Tax Bill (USD)	89.46	53.85	40,060

N indicates the number of properties for which there was data. Analysis is limited to those blocs eligible for garbage removal services. Some household characteristics are only available for a subset of the city.

with 6% in the lowest tax bracket (6% of properties) up to 10% for properties who estimated annual rental value is greater than 3,077 USD. Note that a considerable majority of properties, 84%, fall into the second step and owe 7% of their annual rental value or between \$54-108 USD annually. The top 3 tax brackets combined only account for 10% of all properties.

Summary statistics<sup>4</sup> for my study city are provided in Table 1.2. Table 1.3 compares those who had paid at least once in the three years prior to the start of interventions (Paid) to those who had not paid during that period. Properties that have paid are more likely to have access to electricity, be located on a boundary road between blocs, have improved roofs, and are more likely to be multi-story. These are all easily observed characteristics which suggest that the visibility of the property may play a role in compliance. Of note, there is

<sup>4</sup>Though data for tax billing in the mayor's office and tax payment in the national tax authority were both digitized as part of the 2011 USAID project mentioned, these two data sets were not linked to each other. Therefore, one of the obstacles this project had to overcome was joining the two data sets based only on the variables contained within each. This was a non-trivial task for several reasons. First, there are multiple types of identification number issued by different government offices, none have wide-spread adoption in the population and neither data set was consistent in collecting or labeling these identifiers. Second, even for common terms like popular names, titles and addresses the data sets have a variety of spellings because Haiti officially has two distinct orthographies (one based on French and the other on Haitian Kreyol), mixing the two even within the same word is not uncommon and only a little more than half of the population can read and write in either language. Furthermore, there is no convention as to the order of writing one's name as one or more family names may alternatively be written before or after one or more given names, and many family names are also common given names. Third, properties can be identified by either their traditional address or by a census code comprised of three distinct three-digit numbers, but rarely were both collected and often what was collected was a nonstandard mix of the two. There is not even a convention for the order of writing the three different numbers in the census code, indeed, it was relatively rare to find all three in any one entry. To overcome these challenges, I employed a combination of data science and field verification to map more than 90% of payments from the 5 years of data made available so far for this study back to tax billing. This created what I understand to be the first city-wide matched set of administrative data in Haiti.

Table 1.3: Comparing Recent Tax Compliers to Recent Tax Non-Compliers

Covariates	Not Paid	Paid	p-value	N
Water Access	0.59	0.61	0.08	33763
Electricity	0.71	0.75	0.00	33763
Road Access	0.70	0.71	0.51	33763
Located on Bloc Boundary	0.30	0.33	0.00	40060
Improved Roof	0.56	0.60	0.00	40060
Number of Floors	0.17	0.23	0.00	33763
Paid FY17	0.05	0.61	0.00	40060
Rental Value/Yr (USD)	1255.81	1180.22	0.36	40060
Tax Rate	0.07	0.07	0.00	38601
Tax Bracket 1	0.04	0.17	0.00	40060
Tax Bracket 2	0.85	0.71	0.00	40060
Tax Bracket 3	0.04	0.05	0.17	40060
Tax Bracket 4	0.03	0.03	0.78	40060
Tax Bracket 5	0.03	0.04	0.00	40060
Total Tax Bill (USD)	89.23	91.13	0.65	40060

Comparing the mean values of those properties in the registry but had not paid in the previous 3 years (Not Paid) to those in the registry who had paid at least once in the previous 3 years prior to the start of interventions (Paid). Statistical significance of the difference in means is indicated by the p-value. N indicates the number of properties for which there was data. Analysis is limited to those blocs eligible for garbage removal services.

no statistically significant difference in total tax bill which suggests that these differences above are not driven by more valuable properties systematically being more likely to pay. Indeed, as is indicated in the differences in representation across tax brackets, there is greater representation of both less expensive and more expensive properties among those who paid.

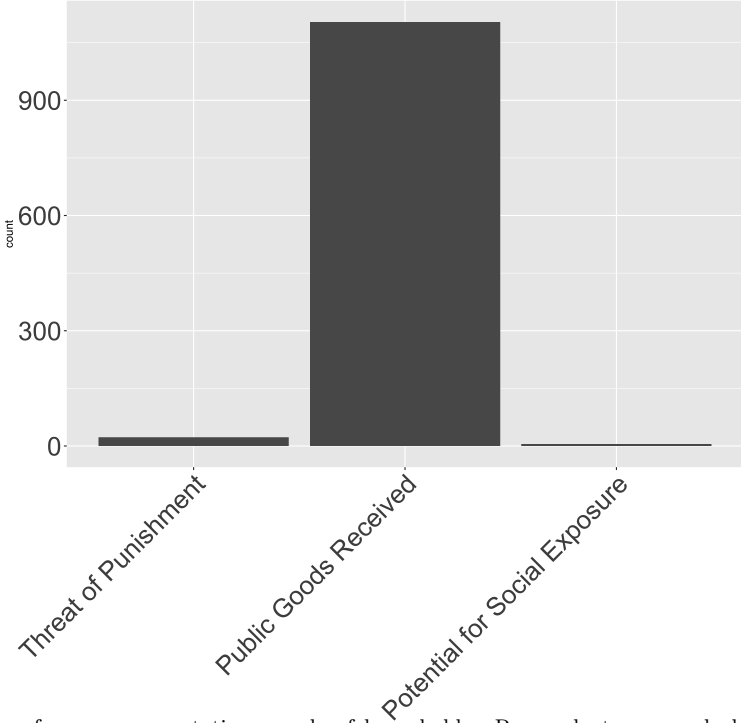
Beyond characteristics gleaned from these Summary Statistics, I was also able to collect both qualitative data and a quantitative baseline survey to generate hypotheses regarding motivations for tax compliance. Discussed in greater detail in the following Chapter in 2.4, responses indicated incentives consistent with my modeling assumptions presented in 1.3. As an example, in response to the open ended question of “What would motivate you to pay property taxes?”, nearly all referenced public goods (Figure 1.2).

## 1.5 Methodology

In partnership with the mayor’s office and national tax authority, I cross-randomized both tax collection and public goods at-scale and over two fiscal years and the 331 census blocs that comprise the urban territory of my study city. Interventions began prior to the start of the 2019 fiscal year in August of 2018 and ran through the end of the 2020 fiscal year in September 2020. The focus is primarily on the owners of the approximately 50,000 buildings for which personal property taxes are owed (Table 1.2 for summary statistics and Figure A4



Figure 1.2: “What would motivate you to pay property taxes?”



Baseline Survey Responses from a representative sample of households. Respondents were asked the open ended question, “What would motivate you to pay property taxes?” Only the most common responses are presented.

for geographic density of properties by bloc). Effects on independently observed measures of political unrest are collected at the census bloc-level. Effects on tax compliance are measured at the property-level from the administrative data from the central tax authority. Furthermore, a representative sample of properties were randomly selected for a panel of survey data.

The experimental design is a clustered randomization with each census bloc representing one cluster. Blocs were stratified by crossing bloc-level mean history of past payment rate {above median, below median} with three divisions based on property value: {above median, below median, no property value data} and initial public goods provision {bordering baseline garbage services, not}. Blocs in each stratified grouping were then randomly assigned to one of the treatment arms show in Figure 1.3. This provides four different experimental conditions for analysis: 1) a set of pure control blocs wherein no new activities take place, 2) a set of blocs with only increased public goods provision, 3) a set of blocs with only increased tax collection effort and finally 4) a set of blocs where both public goods and tax collection effort are increased to identify any interaction effects.

Conditional on being randomly assigned to receive the tax collection treatment at the bloc-level, properties were further randomized at the property-level to either receive a visit

Figure 1.3: Factorial Design for Treatments (2x2)

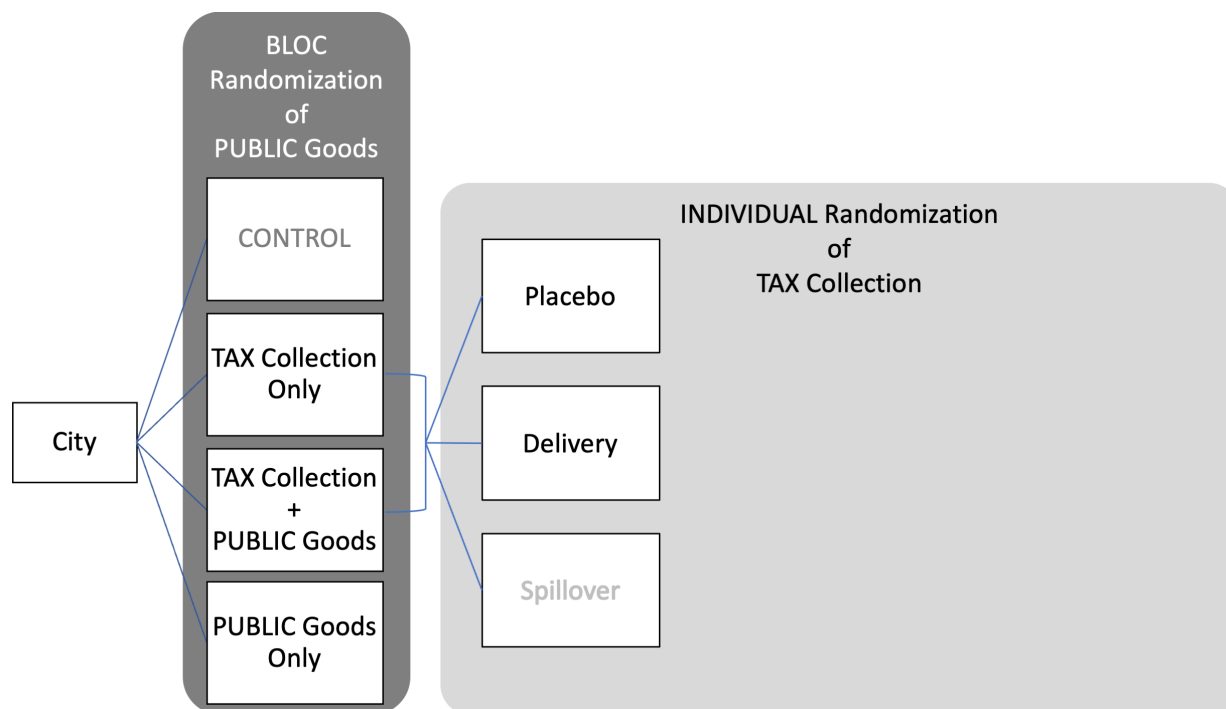
		TAX Collection Treatment	
		No	Yes
Public Goods Treatment	No	<b>CONTROL</b>	<b>TAX Collection Only</b>
	Yes	<b>PUBLIC Goods Only</b>	<b>TAX Collection + PUBLIC Goods</b>

Stratified randomization performed at the bloc-level.

from a tax agent who hand-delivered a personalized tax invoice for the property taxes due for the fiscal year 2019 (“Delivery”), a personalized visit from a representative of the local government who hand-delivered a personalized bank holiday calendar (“Placebo”), or no visit at all (“Spillover”) as illustrated in Figure 1.4. The standard invoice included a general reminder of property tax obligations, the deadline and information for how to proceed with payment as well as one’s name, address, estimated property value, and total tax bill (Figure A.3). To further identify mechanisms, the type of invoice delivered and additional follow-up were further randomly assigned, and those findings will be explored in Chapter 2.

With respect to public goods, in blocs randomly assigned to this treatment, the mayor increased the primary public good provided by the municipal government: garbage removal services and solid waste management. This is the only city-wide service provided by mayors in Haiti to the general public (other services are provided via explicit fee-for-service mechanisms). Furthermore, though not considered the biggest problem in my study city (Figure 1.5), my qualitative research and subsequent baseline survey indicated that this is the service most associated with the government and which the most people would like to see the government prioritize (Figure 1.6). In contrast, water and electricity are almost exclusively provided by private companies, public lighting infrastructure reaches only a small percentage of streets within most cities and is unreliable on all but the most essential thoroughfares.

Figure 1.4: Experimental Design Tree – Summary



Bloc randomization in dark gray depicts the same design information as presented in the “Factorial Design for Treatments (2x2)” in Figure 1.3. The light gray area depicts the additional individual-level randomization conditional on being assigned tax collection treatment (either alone or with public goods) at the bloc-level. “Placebo” indicates that a property is assigned to receive a bank holiday calendar. The findings for this treatment are found in 1.7. “Delivery” indicates that a property is assigned to receive an invoice. Conditional on being assigned to “Delivery” the type of invoice and additional follow-up is further randomized and these treatments are the subject of Chapter 2 and presented in depth in 2.6 and a full summary of individual-level treatments are also summarized in A.3.

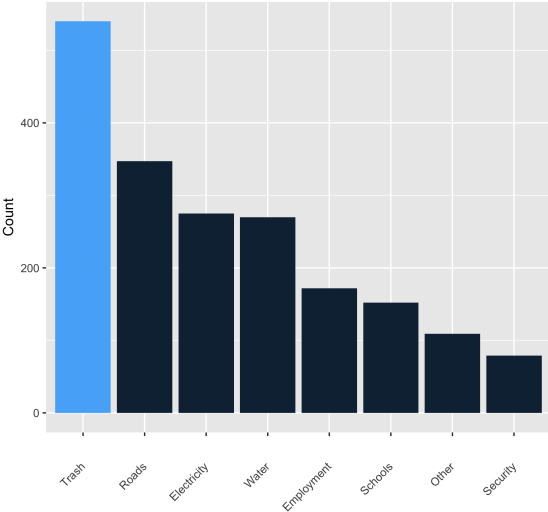
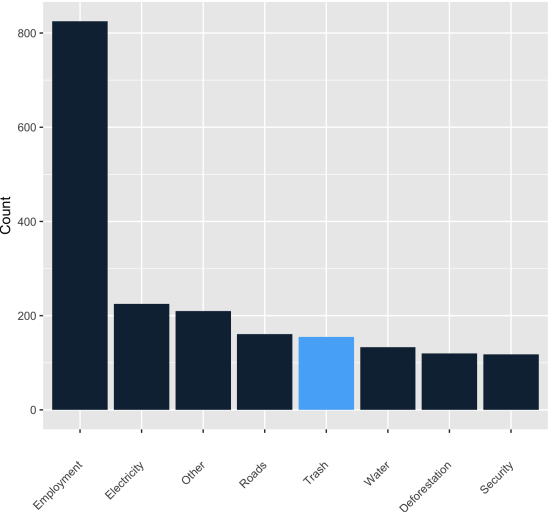
80% of all schools are private, and 95% of all students who pass the 4th grade national exam attend private schools. Other services, like the maintenance of public spaces, ravines, and canals, are geographically-specific.

To further test the potential of this particular public good prior to the start of interventions, I included an incentive compatible Becker–DeGroot–Marschak willingness to pay (WTP) solicitation for garbage removal services in my baseline survey. I find support across the full tax schedule suggesting that even if people were only to receive garbage removal services, WTP for those services would be sufficient for many to pay the equivalent of their full property tax bill (Figure A5).

Treatment was defined by first driving all streets in the study city to determine which blocs were accessible for the garbage removal vehicles and which streets within each bloc were accessible for door-to-door services. From the total set of 331 census blocs in the urban area of my study city, 241 were deemed accessible and thus eligible for public goods treatment. These were then randomized per Figure 1.3 and the stratification discussed in ???. I then designed a

Figure 1.5: “What is the biggest problem in your city?”

Figure 1.6: “What should be the government’s top priority?”



Baseline Survey Responses from a representative sample of households. Respondents were asked the open ended questions indicated above and responses were categorized by the enumerators.

series of 24 routes incorporating only those blocs randomly assigned to receive public goods (either only public goods or public goods and tax collection together). Assignment to public goods treatment meant that all drivable roads within the bloc - including boundary roads - were incorporated into the garbage routes. The order of the routes was then randomized into a schedule designed to visit half of the routes each week - or every route on a bi-weekly basis. I then digitized the routes and loaded them onto a smartphone application that provided turn-by-turn directions even when offline so long as a GPS connection was maintained. I then oversaw the training of the drivers for the routes and the use of the smartphone application.

To track service delivery, I worked with the mayor to install GPS tracking devices in all thirteen (13) public vehicles which remotely monitor location, velocity, acceleration, and whether or not the engine is on. I further had data collectors with a second GPS tracker ride along with removal routes periodically to verify compliance. I also collected baseline trash delivery data at the local dump on origin, quantity, type of vehicle, and source of funding for each delivery received.

No blocs experienced a reduction in services as a result of the randomization introduced in this study. Rather, the mayor directed additional resources, most notably six (6) new trash compacting trucks donated from a sister city in Martinique, to increase the frequency and intensity of service. Indeed, most treatment blocs started receiving garbage removal services for the first time during this study.

Finally, to make as salient as possible the connection between tax payments and public goods, the Mayor’s Office painted the sides of garbage removal vehicles with the equivalent of, “Property Taxes + Business Taxes = Services”. In addition, agents were assigned to walk

alongside the vehicles with a megaphone to call to the people to bring out their garbage as the vehicle passed by. These agents were also given brief scripts developed by the Mayor’s communications team with messages emphasizing the connection between tax collection and public goods.

## 1.6 Results

I test and report findings for the effects of tax collection, public goods, and their interaction on tax compliance in 1.6 and on political unrest in 1.6. I then provide additional causal estimates of the intensity of public goods treatment in 1.6 on both tax compliance and political unrest.

### Effects on Tax Compliance

I begin by testing the intent to treat effect using the following regression:

$$Y_{ib} = \beta TAX_{ib} + \delta PUBLIC_b + \gamma FRAME_{ib} + \mathbf{X}_{ib}\Theta + \mathbf{Z}_b\Psi + \phi_s + \epsilon_{ib} \quad (1.6)$$

Where  $Y_{ib}$  is the outcome of interest for property  $i$  in bloc  $b$ .  $TAX_{ib}$  is an indicator variable equal to 1 if property  $i$  in bloc  $b$  is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise.  $PUBLIC_b$  is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise.  $FRAME_{ib}$  is the indicator variable for random assignment to receive a invoice with additional framing or ‘nudges’ accompanying the property tax invoice. In addition,  $\mathbf{X}_{ib}$  is a series of individual covariates including the valuation of the property, the total tax bill, and past tax payment history.  $\mathbf{Z}_b$  are bloc covariates incorporating the population density, number of registered properties, and average level of access to services like roads, power, and water. As discussed in the Section 1.5, the randomization was stratified by mean bloc property value, mean payment history, and geographic presence of other salient public services. As a result, I keep stratum fixed effects,  $\phi_s$ , in all specifications. Furthermore, in all estimations, I cluster the standard errors,  $\epsilon_{ib}$ , at the bloc-level to correspond to the primary level of randomization.

I start by testing the predictions outlined in my model introduced in 1.3. I use amount of taxes paid (in USD) per property as the outcome of interest and regress this on the model described in Equation 1.6. From this equation, the coefficient  $\beta$  on  $TAX_{ib}$  can be interpreted as the causal effect of being assigned to the tax collection treatment. As a result, finding an estimate of  $\beta < 0$  would provide evidence supporting the prediction that [In the absence of public goods or credible fines, increasing tax collection effort increases evasion](#). Furthermore, the coefficient  $\delta$  on  $PUBLIC_b$  can be interpreted as the causal effect of being assigned to

Table 1.4: Effects of Tax Collection and Public Goods on Amount of Taxes Paid by Property

	Amount of Tax Paid in USD	
	FY19	FY19-FY20
	(1)	(2)
Tax Collection	-6.29** (3.17)	-10.02** (5.06)
Public Goods	3.55** (1.80)	3.53 (2.51)
Mean	13.1	21.86
F-Stat	38.98	35.13
Observations	40,060	40,060
Adjusted R <sup>2</sup>	0.02	0.03

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

the public goods treatment. Thus an estimate of  $\delta > 0$  would provide evidence supporting the prediction that [Increasing public good provision decreases evasion](#).

Results are reported in Table 1.4. In column 1, I present my preferred specification. I test effects on property tax payments made in the first full fiscal year of the experiment, FY 2019 corresponding to the period from October 1, 2018 - September 30, 2019. I find support for my model’s prediction on tax collection. Being randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes decreased tax payments by \$6.29 in FY 2019. This effect is statistically significant by conventional standards and represents a sizable economic reduction of 48% on an average tax payment during the period of \$13.10. I also find statistically and economically significant evidence supporting my model’s prediction on public goods. Being located in a bloc randomly assigned to receive garbage removal services during this period increased tax payments by \$3.55 or 27% over the mean.

In column 2, I take advantage of an additional 6 months of administrative data collected and test effects over the 18 months including all of FY 2019 (October 1, 2018 - September 30, 2019) as well as the first half of FY 2020 (October 1, 2019-March 30, 2020). Over this longer period, I find further support for Prediction 1 as the tax collection effect is of the same sign and magnitude. Being randomly assigned to have a tax agent from the mayor’s

office hand-deliver an invoice for property taxes decreases payments by 47% in this case or \$10.02 on an average payment of \$21.86. For public goods I find an effect over this longer period that is the same absolute increase but much less precise. It suggest a \$3.53 increase from being located in a property randomly assigned to receive garbage removal services. This point estimate is only a 16% increase on the \$21.86 average. Though still economically significant, the effect is not statistically significant by conventional standards. This may suggest that the public goods effect is attenuating with time.

In order to test if the effects of receiving both tax collection and public goods together are different than simply adding their individual effects, I introduce the interactive terms to regression Equation 1.6:

$$\begin{aligned}
 Y_{ib} = & \beta TAX_{ib} + \delta PUBLIC_b + \gamma FRAME_{ib} + \\
 & \mu TAX_{ib} \times PUBLIC_b + \nu PUBLIC_b \times FRAME_{ib} + \\
 & \mathbf{X}_{ib}\Theta + \mathbf{Z}_b\Psi + \phi_s + \epsilon_{ib}
 \end{aligned} \tag{1.7}$$

With this new equation, it is important to note that the interpretations of  $\beta$  and  $\delta$  change. Now  $\beta$  is the effect of being randomly assigned to *only* receive the standard tax collection intervention and  $\delta$  is the effect of being randomly assigned to *only* receive the public goods treatment. The effect of receiving both together is  $\beta + \delta + \mu$  with  $\mu$  identifying any additional effect arising from the combination of the two interventions. So then  $\mu = 0$  would indicate that the combined effect is equal to the sum of the individual effects.  $\mu > 0$  would indicate that the combined effect is greater than the two separately, in other words that there are resulting complementarities of combining the two.  $\mu < 0$  would indicate that in combination the result is less than simply adding the two effects together.

Results are reported in Table 1.5. Column 1 is again testing the effect in the first fiscal year and Column 2 includes the full 18 months of administrative data available. The estimated effect of tax collection alone is negative, statistically significant and larger than the estimates in Table 1.4. This is especially true in Column 1 for FY2019 for which the estimated effect from tax collection is nearly 50% larger in magnitude. This indicates that the negative effects of tax collection estimated in Table 1.4 are driven by the effect in areas without public goods. Though positive in point estimate, the effects of public goods alone in both columns of Table 1.5 are much smaller and much less precisely estimated. The interaction effects are also estimated to be positive and much larger. For instance, in FY2019 the interaction effect alone has a larger point estimate than the absolute value of either of the estimated effects in Table 1.4. This suggests that the positive public goods effects estimated in 1.4 are largely driven by positive interaction effects in areas also receiving the tax collection treatment. The interaction effects are also imprecisely estimated and not statistically significant meaning that though I cannot reject a null effect, I also cannot rule out potentially large positive complementarities. This lack of precision with estimates involving public goods could be expected. The treatment itself is randomized at the bloc-level, the estimated effects of each intervention alone push in opposite directions so the combination tends toward zero and the

Table 1.5: Interaction Effects of Tax collection and Public Goods on Amount of Taxes Paid by Property

	Amount of Tax Paid in USD	
	FY19	FY19-FY20
	(1)	(2)
Tax Collection	-9.22** (4.53)	-12.47* (7.40)
Public Goods	0.55 (1.54)	1.37 (2.56)
PUBLIC X TAX	6.96 (5.66)	5.76 (9.89)
Linear Combination	-1.71 (3.43)	-5.33 (5.75)
Mean	13.1	21.86
F-Stat	30.24	31.22
Observations	40,060	40,060
Adjusted R <sup>2</sup>	0.02	0.03

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. “Linear Combination” presents the estimated coefficient of a two-sided general linear hypotheses test of the linear combination of the coefficients on “Tax Collection”, “Public Goods”, and “PUBLIC X TAX” equaling zero, and the corresponding clustered standard errors are presented in parentheses. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

estimate is losing power by dividing the sample. As noted in the lower section of the table, the estimated linear combination of the two individual effects and the interactive effect are negative though noisy for both the first fiscal year and the full 18 months indicating that I cannot reject that the total effect is equal to zero. Taken as a whole, this table provides additional support for the prediction [In the absence of public goods or credible fines, increasing tax collection effort increases evasion](#), and in particular the nuance of the condition that the negative effect of tax collection is strongest in the absence of public goods and attenuates as public goods are introduced.



## Effects on Political Unrest

I now turn my attention to measuring how these interventions affect political unrest.<sup>5</sup> In this section I first provide evidence for survey responses and then propose two novel measures of political participation.

I again estimate Equations 1.6 and 1.7, but now with survey responses regarding protest participation as the outcome of interest. Results are reported in Table 1.6. In Columns 1 and 2, I estimate effects on responding “yes” to the question, “Have you or any member of your household participated in a protest in the past 30 days?”. In Column 1, I find that public goods reduced the likelihood of a household answering “yes” by 0.10, more than a 50% reduction on an average of 0.19. Neither tax collection in Columns 1 and 2 nor the interaction in Column 2 have a statistically significant affect on the likelihood of a household reporting to have participated in protests. However, when controlling for the interaction, I find the effect from public goods to be statistically indistinguishable from that of Column 1. I interpret this as evidence that the reduction in protesting from public goods is primarily occurring in areas without tax collection.

Columns 3 and 4 test effects on responses to the question, “How many hours in the previous month have you or any member of your household spent protesting?” In Column 3, I find evidence that tax collection increases the amount of time spent protesting by 1.2 hours which is a 176% increase over the mean value of 0.68 hours. Column 4 controls for the interaction and finds that tax collection alone increases reported time protesting by 0.87 hours or 127%. Consistent with my findings from Columns 1 and 2, I find public goods alone reduces time reported protesting by 0.43 hours or 60%. The interaction effect in this case is large and positive, and though noisy, it does suggest the potential for a compounding effect wherein providing providing public goods while attempting to increase tax collection does not move more households to protest (Column 2), but for those that do protest, the interaction may increase the amount of time those households spend protesting.

Political participation can often take the form of public speeches and debates. My qualitative research indicated that in this setting political graffiti (Figure 1.7) is a principal forum for this expression. Research in other disciplines support this interpretation, notably the analysis of De Ferrari (2020) who finds graffiti in this particular setting to serve as “public poetry” leading to “political praxis” by providing a medium for political debates to evolve over time across the walls of the city. I conducted a census of all streets in my study city and geo-tagged all graffiti identified resulting in a bloc-level outcome of interest for which I can test the effects of tax collection and public goods using the bloc-level randomization discussed in 1.5. I estimate effects using Equations 1.8 and 1.9, and control for bloc-level propensity for political graffiti by controlling for the presence of messages referencing the previous election which took place prior to the start of interventions. In this way I introduce geo-tagged political graffiti as a novel independent metric of political engagement and unrest.

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<sup>5</sup>This RCT was conceived of and designed to also directly test effects on voter turnout and electoral outcomes in the next election in Haiti. However, due to a confluence of events, the elections have been delayed into the new year. Though it is my intention to continue following the effects of these interventions into the election, those results will be the subject of a companion paper.

Table 1.6: Effects of Tax Collection, Public Goods, and their Interaction on Self-Reported Protest Participation

	Protest Participation in the Past Month (Self-Reported)			
	Indicator		Hours	
	(1)	(2)	(3)	(4)
Tax Collection	0.07 (0.04)	0.07 (0.06)	1.27* (0.70)	0.95* (0.52)
Public Goods	-0.11*** (0.04)	-0.11** (0.04)	-0.16 (0.46)	-0.44** (0.22)
PUBLIC X TAX		-0.003 (0.09)		0.74 (1.19)
Linear Combination		-0.04 (0.06)		1.24 (1.2)
Mean	0.19	0.19	0.68	0.68
F-Stat	3.04	2.7	2.44	2.27
Observations	445	445	445	445
Adjusted R <sup>2</sup>	0.02	0.02	0.01	0.01

“Indicator” is a binary indicator equal to 1 if a household answered “yes” to the question, “Have you or any member of your household participated in a protest in the past 30 days?”. “Hours” is the amount of time in hours given in response to the question, “How many hours in the previous month have you or any member of your household spent protesting?”. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. “Linear Combination” presents the estimated coefficient of a two-sided general linear hypotheses test of the linear combination of the coefficients on “Tax Collection”, “Public Goods”, and “PUBLIC X TAX” equaling zero, and the corresponding clustered standard errors are presented in parentheses. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

$$Y_b = \beta TAX_b + \delta PUBLIC_b + \mathbf{Z}_b \Psi + \phi_s + \epsilon_b \quad (1.8)$$

$$Y_b = \beta TAX_b + \delta PUBLIC_b + \mu TAX_b \times PUBLIC_b + \mathbf{Z}_b \Psi + \phi_s + \epsilon_b \quad (1.9)$$

Before testing my outcomes of interest, I begin by testing for effects on graffiti in general to address concerns that any estimated effects on political graffiti are driven by the overall prevalence of graffiti in a given bloc. The results, reported in Columns 1-2 of Table 1.7, are small and not statistically significant.

Second I test effects on graffiti supporting a social movement unrelated to local government or the interventions being tested. Specifically, during the graffiti census a message continued to turn-up, ultimately comprising nearly 10% of the total amount of graffiti in the

Figure 1.7: Partisan Graffiti



The two most common forms of political graffiti in the city. The left saying “Down with Mayor Jude” and the right saying “Long Live Jude, One Love”.

city. The message, most commonly in the form “Le Poeme Tue” or “La Religion Tue” (“The Killed Poem” or “The Killed Religion”) was not known to any of my field team and turned out to be a nascent, nonpartisan social movement organized by local artists calling for the end of violence in the country (Figure 1.8). Given the movement’s national focus and the orthogonal relationship of its aim and the interventions of this study, I use these specific graffiti as an additional placebo. The results are reported in Columns 3-4, and they again are not statistically significant.

Turning the experimental treatments, estimated effects of tax collection and public goods on political graffiti are reported in Table 1.8 and provide further evidence of tax collection increasing political unrest and public goods mitigating this effect. The estimates in Column 1 are not statistically significant by conventional measures, but the point estimates are consistent with the effect of tax collection increasing political graffiti while public goods decrease political graffiti. Introducing the interaction in Column 2 provides clearer evidence that the interventions impact political graffiti in this setting. Tax collection alone has the statistically significant effect of doubling the probability of finding political graffiti in a bloc by increasing 0.117 on a mean of 0.116. Though the effect of public goods alone is quite small and imprecise if positive, the interaction is a relatively large and statistically significant reduction in the probability of finding political graffiti in a bloc by 0.183 providing evidence that public goods significantly mitigates the large positive effect of tax collection on political graffiti. As seen in the lower section of the table, the estimated linear combination of the total effect is negative though not statistically different from zero itself.

Columns 3-6 estimate effects on the tone of political graffiti finding results consistent with tax collection increasing political discontentment and additional evidence of public goods mitigating those effects. In Columns 3-4, I estimate effects on the probability in a given bloc

Figure 1.8: Social Movement Graffiti



Examples of the principal message for a social movement calling for an end to violence in the country. Roughly translated as, "The Killed Poem", I use this message as one of the placebo tests for my analysis of the effects of tax collection and public goods on political graffiti.

Table 1.7: Placebo Tests for Effects of Tax Collection, Public Goods, and their Interaction on Graffiti

	Graffiti			
	Any		Social Movement	
	(1)	(2)	(3)	(4)
Tax Collection	-0.011 (0.051)	0.022 (0.066)	-0.048 (0.055)	-0.070 (0.068)
Public Goods	-0.026 (0.052)	0.016 (0.072)	-0.048 (0.050)	-0.075 (0.070)
PUBLIC X TAX		-0.084 (0.105)		0.055 (0.105)
Linear Combination		-0.05 (0.08)		-0.09 (0.08)
Mean	0.75	0.75	0.2	0.2
F-Stat	6.78	6.1	2.91	2.64
Observations	241	241	241	241
Adjusted R <sup>2</sup>	0.191	0.190	0.068	0.065

“Any” is a binary indicator equal to 1 if any graffiti was identified in the bloc and zero otherwise. “Social Movement” is a binary indicator equal to 1 if a message identified as part of a nascent social movement calling for an end to violence was identified in the bloc and zero otherwise. “Tax Collection” is an indicator variable equal to 1 if a bloc is randomly assigned to have tax agents from the mayor’s office hand-deliver invoices for property taxes in the bloc and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a census bloc is randomly assigned to receive garbage removal services and equal to zero otherwise. “Linear Combination” presents the estimated coefficient of a two-sided general linear hypotheses test of the linear combination of the coefficients on “Tax Collection”, “Public Goods”, and “PUBLIC X TAX” equaling zero, and the corresponding clustered standard errors are presented in parentheses. Analysis is limited to only census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. Columns 3 and 4 control for propensity for political graffiti with an indicator variable equal to 1 if messages referring to the previous election completed prior to the start of interventions was identified in the bloc and zero otherwise. All regressions also include additional bloc controls and strata fixed effects.

of finding political graffiti expressing support, generally in the form of “long-live” followed by the named official or policy. In Column 3, I find suggestive though imprecise evidence that tax collection is reducing the prevalence of positive messages while public goods are increasing such messages. This interpretation is consistent with Column 4 where, with the interaction added, tax collection alone essentially eliminates positive political graffiti in treated areas with an estimated coefficient of  $-0.058$  on a mean of  $0.054$ . Moreover, in the interaction, I find a statistically significant two-fold increase in positive political messages of  $0.112$  though the linear combination of the full effect is not statistically different than zero. In Columns 5-6, I estimate the effects on political messages condemning or expressing dissatisfaction with the government or individual officials. For the most part these estimates are smaller and even less precise than the previous two columns. The estimated effect of public goods in Column 5 suggests that it reduces the probability of negative political graffiti in the bloc.

Table 1.8: Effects of Tax Collection, Public Goods, and their Interaction on Political Graffiti

	Graffiti					
	Political		Positive Political		Negative Political	
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Collection	0.046 (0.043)	0.117** (0.057)	-0.014 (0.027)	-0.058* (0.032)	-0.008 (0.033)	0.044 (0.044)
Public Goods	-0.058 (0.040)	0.034 (0.060)	0.014 (0.029)	-0.043 (0.043)	-0.040 (0.031)	0.028 (0.052)
PUBLIC X TAX		-0.183** (0.087)		0.112* (0.066)		-0.135* (0.069)
Linear Combination	-0.032 (0.049)		0.012 (0.05)		-0.063 (0.03)	
Mean	0.116	0.116	0.054	0.054	0.066	0.066
F-Stat	2.74	2.8	1.78	2.25	1.29	1.97
Observations	241	241	241	241	241	241
Adjusted R <sup>2</sup>	0.018	0.034	0.005	0.016	-0.015	-0.002

“Political” is a binary indicator equal to 1 if any graffiti coded with a political message was identified in the bloc and zero otherwise. “Positive Political” is a binary indicator equal to 1 if the message was identified as political and was positive in content (generally calling for support with ‘long-live’) was identified in the bloc and zero otherwise. “Negative Political” is a binary indicator equal to 1 if the message was identified as political and was negative in content (generally condemning with ‘down with’) was identified in the bloc and zero otherwise. “Tax Collection” is an indicator variable equal to 1 if a bloc is randomly assigned to have tax agents from the mayor’s office hand-deliver invoices for property taxes in the bloc and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a census bloc is randomly assigned to receive garbage removal services and equal to zero otherwise. “Linear Combination” presents the estimated coefficient of a two-sided general linear hypotheses test of the linear combination of the coefficients on “Tax Collection”, “Public Goods”, and “PUBLIC X TAX” equaling zero, and the corresponding clustered standard errors are presented in parentheses. Analysis is limited to only census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions control for propensity for political graffiti with an indicator variable equal to 1 if messages referring to the previous election completed prior to the start of interventions was identified in the bloc and zero otherwise. All regressions also include additional bloc controls and strata fixed effects. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Figure 1.9: Barricade Construction



A relatively common form of protest over the past two years, in particular during the period of 'Lock-Down'.

This interpretation is again supported by adding the interaction in Column 6. The result is a statistically significant and economically large reduction of 0.135 on a mean of 0.066. Furthermore, as can be seen in the lower section of the table, the linear combination of all three coefficients in Column 6 results in a statistically significant reduction in the probability of finding negative graffiti in a bloc of 0.063 on the mean of 0.066. So in summary, I find evidence that tax collection increases political graffiti overall while also causing the tone to be more negative. I also find evidence that public goods mitigate this effect from tax collection in the interaction.

Haiti had, and in many ways continues to, experience a particularly difficult period which culminated in all major cities including my study city being shut down for two months in the fall of 2019. One of the primary means of protest in Haiti is filling the streets with debris and other hazards. In the more extreme forms, neighbors construct barricades that

Table 1.9: Effects of Tax Collection, Public Goods, and their Interaction on Protest Barricade Construction

	Protest Construction					
	Any		Barricades		Access Blocked	
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Collection	0.060 (0.038)	0.083 (0.051)	0.099*** (0.032)	0.135*** (0.047)	0.051 (0.060)	0.105 (0.078)
Public Goods	-0.056 (0.038)	-0.026 (0.052)	-0.066** (0.029)	-0.019 (0.029)	-0.149** (0.058)	-0.078 (0.083)
PUBLIC X TAX		-0.060 (0.081)		-0.093 (0.063)		-0.139 (0.117)
Linear Combination		-0.002 (0.054)		0.023 (0.039)		-0.112 (0.082)
Mean	0.124	0.124	0.071	0.071	0.357	0.357
F-Stat	2.05	1.93	2.75	2.46	4	3.84
Observations	241	241	241	241	241	241
Adjusted R <sup>2</sup>	0.190	0.188	0.108	0.112	0.129	0.130

“Any” is a binary indicator equal to 1 if any form of protest construction was identified in the bloc and zero otherwise. “Barricades” is a binary indicator equal to 1 if significant protest construction to bloc the flow of vehicles was identified in the bloc and zero otherwise. “Access Blocked” is an indicator variable equal to 1 if a the primary road into a bloc was blocked by a “Barricade” and equal to zero otherwise. “Tax Collection” is an indicator variable equal to 1 if a bloc is randomly assigned to have tax agents from the mayor’s office hand-deliver invoices for property taxes in the bloc and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a census bloc is randomly assigned to receive garbage removal services and equal to zero otherwise. “Linear Combination” presents the estimated coefficient of a two-sided general linear hypotheses test of the linear combination of the coefficients on “Tax Collection”, “Public Goods”, and “PUBLIC X TAX” equaling zero, and the corresponding clustered standard errors are presented in parentheses. Analysis is limited to only census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions also include bloc controls and strata fixed effects. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

significantly inhibit or completely bloc traffic from entering an area. Commonly built from old vehicles, used construction materials and worn out tires that are piled together and often set on fire, these barricades are a very public and destructive form of asserting independence and dissatisfaction with the state (Figure 1.9). Having mapped the universe of barricades during this period (and their relative magnitude), I introduce my second independent metric for measuring political engagement and unrest: barricade construction.

I estimate Equations 1.8 and 1.9 with barricade construction as the outcome of interest. Table 1.9 presents the results providing further evidence of tax collection causing increased unrest which is reduced by the introduction of public goods. Columns 1-2 estimate affects on the probability that “Any” protest construction was identified in the bloc. Though none of these estimates are statistically significant, they do all provide point estimates with signs consistent with tax collection increasing unrest while public goods mitigate this engagement



with more of the effect coming through the interaction. Columns 3-4 focus only on the most extreme forms of barricade construction that completely bloc the road from being accessed. I find statistically significant and large effects of tax collection increasing barricade construction by 0.099 or about a 135% increase over the mean of 0.071. Furthermore, I find that public goods reduce barricade construction by 0.066 or more than 90%. Column 4 indicates that the tax collection effect is driven by those blocs only receiving tax collection which increase barricade construction by 0.135 or 190% over a mean of .071. Though the estimated effects of both the public goods only and interaction are not statistically significant, they are both negative and the interaction's point estimate is much larger in absolute value suggesting that the positive public good effect from Column 3 is largely the result of public goods mitigating the positive effect of tax collection. Finally, in Columns 5 and 6, I estimate effects on having access blocked by barricade construction. Though less precise, I find point estimates consistent with tax collection, especially tax collection by itself, increasing the probability of having primary access blocked due to barricade construction. I find statistically significant evidence that public goods reduce this probability by 0.149 on a mean of 0.357 or about a 40% reduction.

Though the effects estimated are derived from the exogenous variation induced by the experimental design, there may be concerns regarding the causal pathway and interpretation of effects from public goods. For instance, the garbage removal service itself resulting from the public goods treatment may be simply eliminating the prevalence of materials for barricade construction and thus mechanically reducing barricades. It would still be an effect of interest for the local government regardless as it indicates a reduction in a particularly salient and difficult to respond to form of disruptive and often violent protest. However, this mechanical explanation is unlikely as the overwhelming majority of garbage collected on the routes are household refuse loaded by the neighbors themselves while barricades are generally constructed from tires, old vehicles and old construction materials (see Figure 1.9). Another potential explanation for the effect may be that the community wants to avoid losing access to this service and as a result tempers their proclivity to build barricades not out of a sign of gratitude but out of a desire to maintain services. Again, from the perspective of the government, the pathway would not change the value of the finding. However, in this case the emphasis would be on not merely providing public goods, but providing public goods that specifically and transparently require road access into order to be provided.

## Public Goods Intensity on Tax Compliance and Political Unrest

I now test the effects of the relative intensity of the public goods treatment on my outcomes of interest. To do this I leverage the quasi-random variation in the number of times garbage vehicles passed through each bloc on removal routes. The schedule for these routes, including the vehicles and personnel, was randomly assigned over a two week schedule at the start of the intervention. As a result, the variation in routes passing through a given bloc arise from the lack of availability of assigned vehicles or personnel. The majority of missed routes were due to vehicle breakdowns, with flat tires being the primary issue. Longer periods

were missed on a few occasions when gangs hijacked vehicles for fuel or ransom. Occasional staffing issues also interfered with route completion due to sick days or unexcused absences.

$$INTENSITY_b = \rho_1 TAX_{ib} + \rho_2 PUBLIC_b + \rho_3 FRAME_{ib} + \mathbf{X}_{ib}\Lambda + \mathbf{Z}_b\Pi + \phi_s + u_{ib} \quad (1.10)$$

$$Y_{ib} = \beta TAX_{ib} + \delta INT\widehat{EN}SITY_b + \gamma FRAME_{ib} + \mathbf{X}_{ib}\Theta + \mathbf{Z}_b\Psi + \phi_s + \epsilon_{ib} \quad (1.11)$$

$$INTENSITY_b = \rho_1 TAX_b + \rho_2 PUBLIC_b + \mathbf{Z}_b\Pi + \phi_s + u_b \quad (1.12)$$

$$Y_b = \beta TAX_b + \delta INT\widehat{EN}SITY_b + \mathbf{Z}_b\Psi + \phi_s + \epsilon_b \quad (1.13)$$

Table 1.10: First Stage Estimates for Instrumenting Public Goods Intensity

	First Stage Regression to Instrument Public Goods Intensity:			
	Taxes Paid	Political Graffiti	Barricades	Access Blocked
	(1)	(2)	(3)	(4)
Tax Collection	-0.81 (1.01)	-0.72 (1.15)	-0.72 (1.16)	-0.72 (1.16)
Public Good Intensity	27.01*** (1.25)	26.26*** (1.31)	26.43*** (1.28)	26.43*** (1.28)
Mean	12.86	13.08	13.08	13.08
F-Stat	72.83	73.71	83.3	83.3
Observations	40,060	241	241	241
Adjusted R <sup>2</sup>	0.66	0.69	0.69	0.69

All regressions have “public goods Intensity” as the outcome of interest. The model labeled “Taxes Paid” is the first stage for the second stage estimating “Amount of Tax Paid USD in FY19”. The models labelled “Political Graffiti”, “Barricades” and “Access Blocked” each are the first stages for the second stages estimating the outcome of interest of the same name. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a census bloc is randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

The first stage results are presented in Table 1.10. Note that across all four outcomes of interest, the relationship with public good intensity is both economically and statistically significant while also being characterized by relatively large F-Statistics. These results suggest that the specification does not suffer from a weak first stage.

In Table 1.11, I present the results of testing the effects of public goods intensity on my independently observed outcomes of interest. The results are consistent with my primary findings. The estimated effects of tax collection remain qualitatively similar in sign and magnitude as before, and the estimated effects of public goods intensity are consistent with the previous public goods estimates. In the odd numbered columns are the reduced-form estimates of Equation 1.6 in Column 1 and Equation 1.8 in Columns 3, 5 and 7 replacing the public goods indicator variable for bloc  $b$  with the public goods intensity count variable for bloc  $b$ . Assuming the variation in intensity to be as-good-as-random, the interpretation of the resulting coefficient estimates is the marginal causal effect of one additional removal route passing through the bloc. If the variation were not as-good-as-random, the interpretation would be one of correlation instead. I also estimate more flexible forms but do not find increased explanatory value. To ensure I am identifying effects from random variation, in the even numbered columns, I present the second stage of the instrumental variables regression described for Column 2 in Equation 1.11 and for Columns 4, 6 and 8 in Equation 1.13. Both of these regressions make use of my randomly assigned public goods indicator in a first-stage to instrument the public goods intensity per Equations 1.10 and 1.12 respectively. This instrument satisfies the exclusion restriction as it was randomly assigned, and it also serves as a very strong instrument as can be seen in the first stage regression output (Appendix Table 1.10). The interpretation of the resulting coefficient estimates for these models is the local average treatment effect (LATE) of one additional removal route passing through the bloc induced by the random assignment of the bloc to the public goods treatment.

Columns 1 and 2 present findings consistent with public goods increasing tax compliance. Both columns indicate an economically and statistically significant increase in tax payments caused by an additional garbage removal route passing through the bloc. The reduced form OLS estimate indicates an additional \$0.18 per property in tax revenue while the IV estimate indicates an LATE of \$0.13 per property. This range of 1-1.3% increase from the mean value suggests between \$5,210 - \$7,210 in additional revenue could be earned from one route passing through the entire city. As I will discuss more Section 1.8, this far exceeds both current and projected operating costs and suggests the activity could provide substantial positive revenue for the city.

Columns 3-8 present findings consistent with public goods reducing political unrest. Columns 3 and 4 both indicate a reduction in political graffiti caused by an additional garbage removal route though only the reduced form estimate is statistically significant by conventional standards indicating a 2% reduction from the mean. Though Column 5 is imprecisely estimated, it suggests an additional garbage removal route leads to a reduction in Barricade construction, and the IV estimate in Column 6 is statistically significant with an additional route induced by the exogenous variation from my RCT decreasing the probability of localized barricade construction by 3.5% from the mean value. Finally both Columns 7

Table 1.11: Effects of Tax Collection and Intensity of Public Goods on Amount Tax Paid and Political Unrest

	Amount Tax Paid USD in FY19		Political Graffiti		Barricades		Access Blocked	
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)
Tax Collection	-6.22* (3.18)	-6.18* (3.19)	0.0442 (0.0425)	0.0442 (0.0424)	0.0974*** (0.0320)	0.0967*** (0.0321)	0.0472 (0.0600)	0.0467 (0.0601)
Public Good Intensity	0.18** (0.09)		-0.0023** (0.0011)		-0.0011 (0.0010)		-0.0046** (0.0019)	
IV Intensity		0.13** (0.065)		-0.0022 (0.0015)		-0.0025** (0.0011)		-0.0056** (0.0022)
Mean	13.1	13.1	0.12	0.12	0.071	0.071	0.36	0.36
F-Stat	38.39	38.9	2.87	2.75	2.4	2.71	3.86	4
Observations	40,060	40,060	241	241	241	241	241	241
Adjusted R <sup>2</sup>	0.02	0.02	0.02	0.02	0.10	0.09	0.13	0.13

“Amount of Tax Paid USD in FY19” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “Political” is a binary indicator equal to 1 if any graffiti coded with a political message was identified in the bloc and zero otherwise. “Barricades” is a binary indicator equal to 1 if significant protest construction to bloc the flow of vehicles was identified in the bloc and zero otherwise. “Access Blocked” is an indicator variable equal to 1 if a the primary road into a bloc was blocked by a “Barricade” and equal to zero otherwise. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “public goods Intensity” is an count variable of the number of times a garbage collection vehicle entered the census bloc based upon the random assignment of route schedule and the quasi-random variation arising from the interruptions to that schedule (vehicle maintenance, insufficient staffing and the hijacking of vehicles by local gangs). “IV Intensity” is the estimated value of “public goods Intensity” instrumented by the “Public Goods” random assignment variable in a firststage regression. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{it}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

and 8 find statistically significant negative effects on the probability of road access to an area being blocked by barricades with the IV estimate indicating an LATE of a 1.5% reduction from the mean value.

Taken together, my results show that tax collection, especially in the absence of public goods, reduces tax payments and increases negative political engagement, political protesting and the more destructive and violent forms of unrest. Furthermore, all of these adverse effects are found to be largely mitigated by public goods which, both as a whole treatment and on the margin, lead to increased tax payments as well as positive or supportive political engagement while reducing protesting - especially the more destructive or violent unrest.

## 1.7 Robustness

Leveraging several experimental elements of my design, I first test for and find evidence against tax collection spillovers in 1.7. I then make use of geographic variation to test for spillovers resulting from the public goods treatment in 1.7 and find evidence suggesting that my preferred specification may be an underestimate.

### Testing for and Finding Evidence against Tax Collection Spillovers

Continuing with the examination of mechanisms, I next test for spillovers potentially affecting the interpretation of my estimated negative effect of tax collection on tax payments. It may be an underestimate if the taxpayer who is not visited by a tax agent also reduces tax payments in response. This might be because the taxpayer interprets the lack of visit as a further sign of state weakness or if a sense of solidarity or sympathy causes a similar crowding-out of tax morale in taxpayers who are not visited. The negative tax collection effect could also be an overestimate though if this taxpayer that was not visited responds by increasing payments. This might be because she interprets the lack of visit as a signal of trust from the state and thus her feeling of obligation or tax morale increases and with it her payments. It could also be in response to anticipating that her neighbors will reduce payments and potentially attract additional attention of the state to the area increasing the chance of facing a financial penalty if found in noncompliance.

I first test for tax collection spillovers by leveraging the multiple levels of randomization in my experimental design. Specifically, I compare the responses of properties that are located in blocs randomly assigned at the bloc-level to not receive tax invoices to those randomly assigned into tax collection blocs but further randomly assigned at the property-level to not receive a visit from the mayor's tax agent. I call this second group my 'spillover' properties as their response is the combination of not receiving a visit and also being close to someone who does receive a visit.

Table 1.12 presents the results of these first spillover tests. In all regressions I use bloc fixed effects so that my identifying variation is the property-level randomization allowing me to compare the responses of 'spillover' properties to those randomly assigned to receive a

Table 1.12: Testing for Spillovers of Tax Collection on Amount of Taxes Paid by Property with Bloc Fixed Effects

	Amount of Tax Paid in USD			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-9.16** (3.56)		-14.83** (5.82)	
ONLY TAX		-9.75*** (2.81)		-14.02*** (4.65)
TAX + PUBLIC		-8.33 (5.94)		-15.96 (9.80)
Mean	13.1	13.1	21.86	21.86
Bloc FE	Yes	Yes	Yes	Yes
Observations	40,060	40,060	40,060	40,060
Adjusted R <sup>2</sup>	0.02	0.02	0.03	0.03

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “ONLY TAX” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned only to the tax collection treatment and equal to zero otherwise. “TAX + PUBLIC” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned both the tax collection and public goods treatment and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{it}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

tax collection visit. In Columns 1 and 3, I find estimated effects of tax collection that are statistically significant and almost 50% larger than my preferred specification in 1.4. The coefficients are similar in magnitude to the estimated effects of tax collection only in Table 1.5. This is especially the case for FY2019 while over the full 18 months the point estimate here is both larger and more statistically significant. In Columns 2 and 4, I divide the sample between blocs only receiving tax collection and those receiving both tax collection and public goods. The resulting coefficients for only tax collection are even more precisely estimated and of the same magnitude. While the point estimates for blocs receiving tax collection and public goods together are of the same magnitude, they are much less precisely estimated and not statistically significant by conventional standards. To further test if the ‘spillover’ properties are lowering the absolute value my estimates, in Table 1.13, I drop them from my estimates. The results in Columns 1-4 do not vary significantly from my estimates in Tables 1.4 and 1.5. Taken together, these estimates are consistent with my preferred specification suggesting that the estimates are neither an under nor an overestimate.

In Columns 5-6 of Table 1.13 I also test the effect of a placebo tax letter delivery treatment I developed with the Mayor’s office (See A.3). This treatment isolates the effect of a

government agent visit from the information effect specifically caused by discussing taxes. Though the effect is imprecisely measured, it is positive and large. This is evidence that the visit by the government itself is not driving the large negative effect of tax collection on tax compliance. This suggests the effect is instead driven by the discussion of taxes or the information only about taxes contained within the invoice or discussed during the visit. In fact, it indicates that the tax information treatment may be considerably larger in magnitude than the estimate suggests as it also includes the noisy large positive effect from the government visit. This is consistent with my model in that the visit itself communicates state capacity and thus increases the perceived probability of facing a fine. When the state specifically uses that visit to discuss tax payments, this triggers the crowding-out of tax morale and results in a net negative effect.

## Testing for Spillovers from Public Goods and Finding an Underestimate

I now test for spillovers from the public goods treatment by leveraging geographic variation. One might expect the estimated positive public goods treatment effects on tax compliance to be an underestimate due to ‘contaminated’ control properties that are located on roads that form boundaries with treated blocs and as a result are actually as-good-as-treated. Similarly, as the trash routes reach into the city, they likely reduce the total distance needed to travel to a garbage removal point so that many control properties are effectively being treated with a lesser intensity. Another potential reason to expect an underestimate is that there was a net increase in garbage removal services across the city and this anecdotally resulted in an overall reduction in the amount of trash in the center of the city and along the main roads of the city while also increasing the overall visibility of government services. As a result even properties in control areas might be treated to some extent by this net improvement. However, if instead, households in control areas resent being left out or otherwise respond to their status of being randomly assigned to control by reducing their tax payments, then the estimated effect on public goods would be an overestimate.

To test for spillovers, I first drop all boundary properties from my analysis. In Table 1.14, I present results. In all Columns I find public goods effects that are larger and in most cases more statistically significant than in my preferred specification. In Column 1, I estimate that public goods increase tax payments by \$5.32 or more than 40% over the mean as compared to my preferred specification showing only a 27% increase in Table 1.4. Column 3 is similarly larger and in this specification statistically significant. Of note, in both Columns 2 and 4 the point estimates on interaction effects are smaller both in terms of comparing to my preferred specification as well as when comparing their relative magnitudes to that of the public goods alone in their respective regressions. This provides suggestive evidence of potentially heterogeneous effects of public goods on interior properties as compared to boundary properties with the former directly increasing their tax payments in response to public goods while the later may be more likely to mitigate their negative tax collection

Table 1.13: Testing for Spillovers of Tax Collection on Amount of Taxes Paid by Property by Dropping ‘Spillover’ Properties

	Amount of Tax Paid in USD					
	FY19		FY19-FY20		FY19-FY20	
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Collection	-6.07*	-10.16**	-9.71*	-14.29*	-9.49*	-11.56
	(3.45)	(5.07)	(5.40)	(8.31)	(5.03)	(7.26)
Public Goods	3.81**	0.74	4.01	2.13	3.54	2.68
	(1.84)	(1.62)	(2.45)	(2.47)	(2.50)	(2.39)
Placebo					12.12	23.19
					(12.79)	(20.96)
PUBLIC X TAX		9.50*		10.38		4.63
		(5.72)		(9.70)		(9.77)
PUBLIC X Placebo						-28.39
						(21.36)
Mean	12.98	12.98	12.98	21.62	21.86	21.86
Drop ‘Spillover’ Properties	Yes	Yes	Yes	Yes	No	No
Observations	37,903	37,903	37,903	37,903	40,060	40,060
Adjusted R <sup>2</sup>	0.02	0.02	0.03	0.03	0.03	0.03

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “ONLY TAX” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned only to the tax collection treatment and equal to zero otherwise. “TAX + PUBLIC” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned both the tax collection and public goods treatment and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{it}$  indicator which control for additional randomly assigned variations in implementation of the “tax collection” treatment beyond the simple delivery and explanation. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$



Table 1.14: Testing for Spillovers of Public Goods on Amount of Taxes Paid by Property by Dropping all Boundary Properties

	Amount of Tax Paid in USD			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-3.91 (3.26)	-6.16 (4.23)	-4.07 (4.52)	-6.69 (6.33)
Public Goods	5.32** (2.53)	2.03 (1.88)	5.21* (3.16)	3.25 (2.73)
PUBLIC X TAX		5.55 (4.47)		6.10 (6.84)
Mean	12.82	12.82	21.22	21.22
Drop all boundary properties	Yes	Yes	Yes	Yes
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.03	0.03	0.05	0.05

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

responses in the interaction. In the Appendix in Table A.7, I also drop just contaminated properties from control and find qualitatively similar results. Taken together, this evidence suggests my estimated public goods treatment effect on tax compliance is an underestimate of the real effect.

To further test for spillovers, I restrict analysis to just properties that have been randomly assigned to not receive the public goods treatment. I then create an indicator variable equal to 1 if the bloc borders any of the blocs randomly assigned to receive public goods (and thus border some length of the assigned garbage removal routes) and zero otherwise. The resulting distribution is 101 blocs that border routes in some way and 42 blocs that do not. The intuition for this test is that if there are either positive or negative spillover effects from being just outside of the public goods treatment which dissipate over distance or if being close to treatment is as-good-as being treated and only the response farther removed would be the true control response, then there should be statistically significant differences in tax payments between those properties just beyond treatment and those even farther away.

Results are presented in Table 1.15. Most notably, none of the coefficients estimated from the “Bordering Public Goods Blocs” indicator are statistically significant. Thus I fail reject

the null hypothesis of there being no spillovers in either direction resulting from differential responses of untreated properties close to and far away from treatment. All of the coefficient estimates are relatively small in magnitude, for instance in Column 1 the point estimate of \$0.21 is less than 2% the mean value of \$13.10. Finally, the sign of the border effect flips when controlling for the interaction in Columns 2, 4 and 6 which is inconsistent with a large spillover effect in one direction or the other.

Notably in Column 2 when controlling for the interaction, the point estimate increases by an order of magnitude, and though it is still not statistically significant, it is the only estimate larger than its cluster-adjusted standard error in magnitude. Then Columns 3-4, when I drop properties that are actually on the roads that are receiving services, the point estimate for this same isolated set of blocs reduces to just \$0.68. These findings are consistent with the evidence above of my public goods estimate being an underestimate as the border properties appear to be increasing payments in response to treatment. This effect appears to diminish quickly as the rest of the bloc does not respond in kind. This can be seen in Columns 5-6 with all boundary properties dropped and the coefficients on the border indicator remaining similar in magnitude. In summary, I do not find evidence of significant negative spillovers driving up my estimate, but I do find some suggestive evidence that I have underestimated the true public goods effect on tax compliance<sup>6</sup>.

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<sup>6</sup>For estimated effects on alternative measures of tax compliance, see Appendix Tables starting with [A.8](#).

Table 1.15: Testing for Spillovers of Public Goods on Amount of Taxes Paid by Property by Comparing Blocs Bordering Public Goods Blocs to those Not Bordering Public Goods Blocs

	Amount of Tax Paid in USD in FY19					
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Collection	-10.39** (4.11)	-6.97 (6.86)	-10.81** (4.36)	-7.93 (6.97)	-9.95*** (3.62)	-15.84 (9.97)
Bordering Public Goods Blocs	-0.21 (1.62)	2.06 (1.89)	-1.07 (1.62)	0.68 (1.77)	-1.45 (1.69)	0.43 (1.57)
Bordering Public Goods Blocs X TAX		-4.92 (8.90)		-4.31 (9.40)		8.84 (10.71)
Linear Combination		-9.83 (5.27)		-11.56 (5.49)		-6.58 (3.05)
Mean	13.1	13.1	13.1	13.1	13.1	13.1
All Blocs are Control Bloc for Public Goods	Yes	Yes	Yes	Yes	Yes	Yes
Dropped Contaminated	No	No	Yes	Yes	Yes	Yes
Dropped All Boundary Properties	No	No	No	No	Yes	Yes
F-Stat	34.07	27.89	32.58	26.25	41.66	32.65
Observations	24,374	24,374	22,093	22,093	17,131	17,131
Adjusted R <sup>2</sup>	0.06	0.06	0.07	0.07	0.07	0.07

“Amount of Tax Paid in USD in FY19” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization and further restricted to only those census blocs randomly assigned to NOT receive the public goods intervention. “Bordering public goods Blocs” is an indicator variable equal to 1 if a property is located in a census bloc bordering at least one census bloc that was randomly assigned to receive garbage removal services and equal to zero otherwise. All regressions include the  $FRAME_{it}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

## 1.8 Discussion: Costs and Benefits

The cost effectiveness of implementing these interventions is also relevant for policy. The tax collection intervention was approximately \$2 per delivery inclusive of printing, transport and labor. Per Column 1 of Table 1.4, each delivery resulted in approximately \$6.29 of lost tax payments in the first year. This points to an initial net loss of approximately \$8.29 per taxpayer.

The public goods intervention has cost about \$3,000 per month for maintenance, fuel, and labor. This is equivalent to approximately \$2.30 per property per year in variable costs - significantly lower than the average property tax bill of about \$90, and not even 1/5 the average annual tax payment of \$13.10. Returning to Column 1 of Table 1.4, this suggests that public goods resulted in a net increase of \$1.25 per property in the first year. This implies the treatment may have been able to net more than \$50,000 in increased returns had it been implemented across the entire city. However, much of that positive estimated effect came from mitigating the negative tax collection effect in the interaction. Taking instead the point estimate from only public goods in Column 1 of Table 1.5, then the variable costs of the intervention would have lost more than \$70,000 for the city in that first year.

There are also initial start-up costs – specifically the 6 vehicles donated at the start of the experiment. Each of these would cost on the order of \$250,000 if purchased new for a total of \$1.5 million. However, the vehicles that were donated had been used by the sister city in Martinique for several years. It is possible that had the Mayor attempted to purchase something equivalent on the secondary market he could have done so for something closer to \$50,000 per vehicle or \$300,000 total. Furthermore, with better management and more efficient routes, it could be possible to accomplish most of what the current team is managing with one well-functioning vehicle which would suggest a minimum investment on the order of \$50,000. Recalling that my findings in Table 1.14 suggest that estimated revenue increases are an underestimate, and that the results from Table 1.11 testing the intensity of public goods also indicated that an additional garbage collection route across the city, which generally takes two weeks, would yield between 170% and 240% of the monthly implementation costs. Even before accounting for the potential savings of running more efficient, non-randomized routes, these findings imply a plausible pathway for sufficiently increasing net revenue to repay the initial start-up investment by the end of the first year of implementation.

Even if the intervention were a net accounting loss for the government in terms of revenue directly raised for money directly spent, the investment in public goods would likely still result in a net benefit for several reasons. As discussed in 1.4, the primary predictor for paying taxes this year is having paid taxes last year. Public goods may then be able to move more people to pay, and pay more, thus establishing a habit of payment potentially leading to positive revenue over time. Lastly, Tables 1.6-1.9 as well as 1.11 all indicate public goods decrease or mitigate political unrest including the more violent and destructive actions. Given that my study city was completely shut down by protests for the equivalent of 3 months over the 24 months of implementation, periods of unrest come at significant financial costs for the citizens and the government. So insofar as public goods provision can

promote pro-social behaviors resulting in increased revenue while reducing the productive losses due to protesting, they will likely generate net benefits - and this is before taking into account the potential welfare gains from public goods provision in this context.

## 1.9 Conclusion

The literature has indicated state capacity building to be a central challenge and delicate balancing act that all developed countries historically had to solve and poor countries today must face. It requires raising revenue without inciting political backlash. Two tools discussed by the literature and available to policy makers that are relevant in this context are increased tax collection effort and public goods provision. In this chapter, informed by qualitative research and economic theory, I provide evidence through an RCT at-scale and lasting multiple years covering the urban area of one of the largest cities in one of the world's weakest states. I cross-randomize both tax collection and public goods in the same setting and follow results in administrative data, panel surveys and through novel metrics of political unrest that I introduce.

Contrary to the established literature from more modern states and high-income settings, I find evidence of statistically and economically significant negative effects on individual tax payments caused by increased tax collection effort. This is consistent with theory and my modeling predictions based on a setting wherein at baseline compliance levels are low and there are few-if-any external incentives. I find that tax collection also increases political unrest across several measures: both the prevalence of political graffiti as well as the negativity of its tone, and participation in political protests, especially the more destructive and violent types of unrest resulting in the construction of protest barricades which are often then set on fire.

With respect to public goods, I find they cause statistically and economically significant increases in tax payments while also reducing political unrest, especially barricade construction during protests. I find further evidence that public goods mitigate many of the adverse tax collection effects both on tax revenue as well as on protest behavior.

Though my findings provide a cautionary tale for governments seeking to increase tax revenue, an accounting exercise on the interventions suggests provision of public goods provide an opportunity to generate positive revenue for the government and even potentially recover initial investments. However, with the lack of financing available in these settings, it is likely that international aid, multilateral or bilateral lending or even intragovernment transfers may be required to provide short-term initial investments.

Open questions and next steps for this research lie in identifying what new equilibrium for tax compliance is possible as well as identifying if and under what circumstances these effects on political unrest caused by tax collection might lead to increased democratic accountability. Most directly, I will be following this and other governments as they attempt to leverage public goods provision to balance purse and peace.

## Chapter 2

# Society as Ally and Adversary for the State

### *Pro-social Norms and Anti-social Punishment in Tax Collection*

#### 2.1 Introduction

Development requires navigating distinct, and at times competing, interests of society and the state (Fukuyama, 2014; Acemoglu and Robinson, 2019). In Chapter 1, I show that during early stages of formation and consolidation the state may need to balance “purse and peace”, mitigating threats of civil unrest as it pursues effective taxation and state capacity. This chapter further explores how, during this period characterized by limited government credibility and enforcement, society and the norms that regulate it can act as both an ally and adversary for the state. I find evidence that the state can leverage pro-social norms in such settings to support its objectives, but that some norms can inhibit the state’s development efforts.

The tax literature has generally assumed social norms are either neutral or supportive of state consolidation and capacity building. Traditional models of tax compliance have generally ignored the role of social norms (Becker, 1974; Allingham and Sandmo, 1972a; Srinivasan, 1973). More recent models that more accurately predict responses by incorporating additional motivations (Erard and Feinstein, 1994; Torgler, 2005; Kirchler, 2007; Luttmer and Singhal, 2014; Bernheim and Taubinsky, 2018), if they include social norms, assume their effect is to reinforce the state’s objectives. These assumptions have largely held in the field experiments that have successfully achieved greater revenue mobilization (Hallsworth, 2014; Pomeranz, 2015; Castro and Scartascini, 2015; Kettle et al., 2016; Hernandez et al., 2017; Shimeles et al., 2017; Carrillo et al., 2017). For instance, Slemrod et al. (2019) follows a natural experiment and finds that appealing to pro-social norms – increasing social exposure

of tax compliance by informing neighbors of one's tax payments – leads to increased tax compliance. However, this empirical work has been conducted in settings of relatively high pre-existing compliance where the dominant behavior is pro-social<sup>1</sup>.

Social norms are neither fixed nor are they always pro-social. Economic research has shown that perceptions of prevailing norms influence decision making and those perceptions can be updated resulting in different outcomes (Bursztyn et al., 2017). As already seen in Chapter 1, society's response to state actions can impede state capacity building and with it, potentially, development as well. This is consistent with psychology research in settings with low rates of pro-social coordination where a mechanism known as antisocial punishment has been identified which contributes to the stability of these low-compliance equilibria (Herrmann et al., 2008).

This chapter builds on the experiment in Chapter 1 to again overcome challenges with respect to implementation and context to provide new evidence on the role of social norms in a setting with low rates of pro-social coordination. First I identify a setting of near-zero tax compliance and very low state capacity<sup>2</sup>. I develop multi-year partnerships with the national tax authority, the mayor of one of the largest cities in the country, and a local software firm to develop and refine the necessary tools, systems, and interventions. Through extensive engagement with the local community and relevant stakeholders including focus group discussions and conducting a representative survey of the population, I document evidence suggesting differences in relevant social norms from what has been found in the literature in high-compliance settings. Informed by this preliminary research, I develop a model and a series of interventions to test the corresponding predictions. For analysis, I join my survey data with multiple, previously-siloed administrative datasets. I extend the experimental design of Chapter 1 with the stratified cluster cross-randomization over the more than half-a-million people living in the study city. In the setting of cross-randomizing tax collection effort and public goods provision at the census bloc-level, this chapter focuses on the results identified by further individual-level randomization within-cluster of the approximately 50,000 properties in the tax registry.

I introduce the mechanism of antisocial punishment to the tax literature. The intuition is that when a group of people perceive that the social norm is one of non-compliance, then a desire to avoid being perceived by the others as contravening that norm will further erode existing levels of compliance. I model, test and find evidence of this mechanism by introducing a novel treatment that experimentally increases social exposure of tax compliance. I randomly assign tax invoices and resulting tax invoice deliveries to inform the taxpayer that if they pay their taxes, the mayor will place a sign on the exterior of their house acknowledging

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<sup>1</sup>In the tax literature, norms are considered to be “pro-social” when they create incentives for individuals to act in ways that reinforce state formation and consolidation. Norms of this directional effect would be more accurately referred to as “pro-state social norms”, but for the purposes of this chapter, I will use the convention. I will then describe as “anti-social” those norms which will create incentives of the opposite sign encouraging individuals to behave in ways contrary to the objectives of the state.

<sup>2</sup>As noted in Chapter 1, in the years leading up to this study, Haiti consistently ranked in the bottom 1% of the World Bank Governance Indicators for Government Effectiveness <https://databank.worldbank.org/source/worldwide-governance-indicators/preview/on>

tax payment.

I find that taxpayers who receive this social exposure message respond by further reducing tax payments beyond the adverse tax collection effect found in Chapter 1. The combined effect is economically and statistically significant causing average tax payments to fall by 66% over the full 18 months. However, this negative response is concentrated in blocs randomly assigned to only receive the tax collection treatment. In blocs randomly assigned to also receive public goods, the effect is mitigated, and the evidence suggests it may even be reversed with average payments increasing by 34% though estimates are imprecise. These findings suggest that in the absence of public goods, antisocial punishment contributes to the stability of the low-tax equilibrium, and that the pro-social signaling mechanism previously identified in the literature may be motivated by receiving public goods.

I also indirectly test several other motivations for tax compliance by randomizing the framing of tax collection engagement. I find evidence that each of these appeals mitigates the adverse tax collection effect found in Chapter 1. I find further evidence that they are effective in preventing significant losses even in the absence of public goods. For instance, I make salient incremental financial penalties of non-compliance. My qualitative research suggests that though the state lacks the capacity to enforce more significant penalties, marginal fines are perceived to be more credible. My experimental results support this hypothesis providing evidence that such appeals may potentially increase net tax revenue in this setting by 19%. I test culturally-specific appeals to obligation and shared responsibility communicated through local proverbs. I find making obligations salient in this way has a net positive effect, increasing tax payments by more than 40%. I also find evidence that effects may be even larger when combined with public goods.

I discuss the cost effectiveness of implementing these interventions. I show that in this setting, attempting to increase tax revenue by increasing social exposure could instead lose the government nearly \$400,000 per year or nearly 3/4 of property tax revenue. In contrast, if the city were to scale-up the appeal to obligation through local proverbs, my results suggest the potential for \$210,000 in additional annual revenue. Finally, as I find that increasing public goods mitigates the antisocial punishment mechanism and may also promote more pro-social norms, my findings further support the conclusions in Chapter 1 regarding the cost effectiveness of increasing public goods to mobilize tax compliance.

In what follows, I first discuss relevant literature and my contributions in 2.2. Then in 2.3, I motivate intuition with a simple model of the tax evasion decision. In 2.4, I provide background on my study city, Carrefour, Haiti, and document evidence of relevant norms in this setting. The research and intervention designs are presented in 2.5, and results are in 2.6. A discussion of the cost effectiveness follows in 2.7, and I conclude in 2.8.

## 2.2 Literature Review

In 1.2 I provide an overview of the literature related to state formation, taxes, and public goods. While contributing to those literatures as well, in this Chapter I highlight research



that more directly intersects with the role of social norms in government policy. Recent research in economics has highlighted both the important role of perceived social norms in decision making as well as the ability to shift social norms under certain conditions ([Bursztyn et al., 2017](#)). Empirical work following a natural experiment in a high-compliance setting has shown that appealing to social norms by increasing social exposure of tax compliance – that is, making one’s tax compliance decision known to one’s neighbors – increases tax payments ([Slemrod et al., 2019](#)). With respect to low-capacity, low-compliance states, theoretical work and even some empirical findings in other disciplines suggest incentive structures different than that found in the relatively developed settings of most existing research ([Frey, 1997](#); [Besley, 2020](#)). The psychology literature has identified one mechanism in such settings called antisocial punishment that may be particularly relevant for early state formation. In settings of low social coordination, populations may punish compliers so to maintain the low-compliance equilibrium. So then, when individuals perceive the norm to be one of non-compliance, concerns of this informal sanction encourage continued non-compliance. [Herrmann et al. \(2008\)](#) conducted public goods games in 16 different cultural settings around the world. They document that “(antisocial) punishment can lead to very strong differences in cooperation levels among comparable social groups” and find that the response is inversely correlated with rule-of-law in their study settings.

Perhaps the best known work testing the effects of using framing or behavioral “nudges” via letter on tax compliance is that of [Pomeranz \(2015\)](#). This paper consists of two inter-related experiments. The first analyzed how the collection of value added taxes (VAT) from private firms in Chile was effected by the interaction between the paper trail produced by the VAT and the Government’s efforts to encourage compliance through a direct mail campaign. Specifically, the Government of Chile sent one of three letters: 1) Penalties: informing firms that they had been selected for analysis and that “if irregularities are detected, they could be subject to an audit”; 2) Social norm of compliance: emphasizing “Chile has one of the highest levels of tax compliance in the world”; 3) Placebo: highlighting changes to the tax authority’s website. In terms of direct effect from the various letters, the author estimates a statistically significant increase of 1.4% from the deterrence letter and no effect for the other two. The primary analysis of the paper focuses on identifying the differential response between those transactions that generate a paper trail in a VAT regime (between-firm sales) and those that do not (final sales to consumers). Here the author finds that the positive increase in VAT reporting is driven by changes in reporting on final sales to consumers, and in particular, final sales by smaller firms. The second experiment tested for spillover effects along the VAT chain by conducting a follow-up audit on a random subset of both firms that received the deterrence letter and control firms. The author finds that upstream suppliers increase their reported VAT contributions by 3.8% over predicted levels and no effect on downstream clients.

The paper whose theoretical framework I extend is that of [Shimeles et al. \(2017\)](#) in Ethiopia. The authors hand-deliver one of two letters to a stratified and randomly selected set of private businesses: 1) Penalties: informing the firm that they “could be audited in that particular fiscal year and warned businesses to provide accurate financial statements”;

2) Obligations and Benefits: appealing to patriotic duties and praising honesty while also informing the recipient of “various compliance based incentives and rewards . . . invit[ing] taxpayers to benefit from these programs”. The authors estimate the largest treatment effects I am aware of for letter-based interventions on tax compliance finding the deterrence letter increased tax reporting by 38% and the combined tax morale and incentives letter increased tax reporting by 32%.

A paper targeting a form of tax compliance closer to that of this Chapter is [Castro and Scartascini \(2015\)](#) in Junin, Argentina. In this case the local government sent tax bills to 23,000 private citizens who owed property taxes and the authors performed a stratified randomization to assign one of four messages to be included with the tax bill: 1) Penalties: proving taxpayers with a simplified example of how to calculate the fees related to noncompliance and making salient the potential for other legal penalties; 2) Benefits: enumerating the number of water and sewage connections installed by the local government in the previous six months; 3) Social norm of compliance: highlighting 70% tax compliance (specifically framed as “only 3 out of 10 did not pay their taxes”). 4) No additional message. Of particular note, the authors use the fourth type of tax bill, the one without any additional message, as their control group. Similar to [Pomeranz \(2015\)](#), the authors find that only the tax bills with the added message on penalties had a statistically significant effect on tax compliance, increasing payments by 12% over the tax bills with no additional message. The authors do find suggestive evidence that households receiving lower levels of public goods increase tax compliance in response to the benefits message, and that households who did not pay (paid) in the previous year, who the authors interpret as potentially having priors below (above) the revealed rate of compliance, increase (decrease) compliance in response to the message communicating social norms of compliance.

In this Chapter I contribute to this literature by studying a dramatically different context, introducing novel treatments and interactions, and the effects that I identify are larger and, in some cases, of a different sign than those found in the tax literature to date. In terms of context, Chile is a high-income country and Argentina is a middle-income country<sup>3</sup> with respective annual per capita GDP PPP<sup>4</sup> that are 14x and 11x greater than that of either Ethiopia or Haiti, both of which are low-income countries. Government capacity between that of Haiti and both Chile and Argentina are similarly divergent, the latter two ranking respectively in the 80<sup>th</sup> and 60<sup>th</sup> percentiles of government effectiveness<sup>5</sup> and even Ethiopia nearly reaching the 30<sup>th</sup> while Haiti is in the 1<sup>st</sup>. However, as treatment arms in two of the three studies make salient, the most notable distinction with regards to context is the pre-existing level of tax compliance. 70% of both observed firms in Chile and of property owners in Argentina were paying the taxes of interest. In Ethiopia, though not stated explicitly,

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<sup>3</sup>All income ranking in this chapter is per World Bank lending categories. Accessed 29 April 2018: <https://datahelpdesk.worldbank.org/knowledgebase/articles/906519-world-bank-country-and-lending-groups>

<sup>4</sup>All reports of Gross Domestic Product (GDP) Purchasing Power Parity (PPP) in this chapter are in 2018 US Dollars as calculated by the World Bank. Accessed 29 April 2018: <https://data.worldbank.org/indicator/NY.GDP.PCAP.PP.CD?locations=ET-US-CL-AR-HT-CD-IT>

<sup>5</sup>2016 Worldwide Governance. Accessed 29 April 2018: <http://databank.worldbank.org/data/reports.aspx?source=worldwide-governance-indicators>

the paper implies that all study participants had, at some point, paid their taxes prior to the experiment, and at least 75% paid again during the period of observation. In stark contrast, 10% of individuals listed in the tax registry in my study city in Haiti paid last year. Like (Castro and Scartascini, 2015), in this Chapter I study the decision of private citizens to pay (or evade entirely) property taxes on their own homes as opposed to the decision of firms to under-report – but still report something – to the government. In addition, I introduce novel treatments in this context. As will be discussed in 2.5, I design a treatment for and experimentally increase social exposure of tax compliance. Similarly, I design novel, appeals to cultural norms of obligation and more credible warnings of penalties that are more credible in my setting of low state credibility. I also conduct my research in a setting with experimentally varied levels of salient public goods and can thus estimate the interaction effects. Finally, as will be discussed in 2.6, I find evidence of adverse responses to some of my treatments causing tax payments to fall, these findings along with those of Chapter 1 represent findings not previously documented in the literature to date (Slemrod, 2019). Further, my statistically significant findings are generally an order of magnitude larger than what has been found in the literature.

## 2.3 Model

In this chapter, to motivate intuition, I build off of the model presented in 1.3 in which the taxpayer chooses a level of evasion,  $e \in [0, 1]$  to maximize the sum of income and public goods received. For convenience, I re-write Equation 1.1:

$$\max_e g + w \left( 1 - (1 - e) \left( t - \tau (g - wt) \right) \right) - \tau f e \quad (2.1)$$

here again  $g \geq 0$  are the public goods received,  $w$  is the taxpayer's private income, and  $t \in [0, 1]$  is the tax rate which is itself weighted by the tax collection effort of the government,  $\tau$ , multiplied by the fairness of the tax,  $g - wt$ . The last term is again the expected financial penalty for evading,  $\tau f e$ .

In this chapter, to further explore compliance in a low-capacity state, I add two additional non-pecuniary incentives consistent with survey responses and existing literature. First, building off of Reckers et al. (1994), I add a sense of obligation,  $\rho \in [0, 1]$  which effectively decreases the weight of the tax rate as the taxpayer feels compelled to make this payment out of duty. This sentiment increases in the level of evasion chosen,  $e$ , as she feels increasingly worse for contravening her sense of duty. Second, to account for the prominent role of social norms, I incorporate a social incentive building off of Shimeles et al. (2017). I first add a social incentive,  $\Sigma$ , which captures the taxpayer's desire to be seen as socially conforming with the behavior of her neighbors. It is multiplied by the probability that her evasion decision will become known to her neighbors,  $q$ , which I refer to in this chapter as social exposure.

As before, both of these additional motivations enter the model as affecting the perceived weight of the tax rate,  $t$ , and the model now becomes:

$$\max_e g + w \left( 1 - (1 - e) \left( t - (e\rho + \tau(g - wt)) \right) - q\Sigma \right) - \tau fe \quad (2.2)$$

The resulting first order condition (*FOC*) maximizing with respect to evasion,  $e$ , is:

$$FOC_e: \quad e^* = \frac{t - \tau(g - wt) - q\Sigma + \tau f}{2\rho} \quad (2.3)$$

## Model Predictions

I begin with the effect on increasing social exposure,  $q$ , on tax evasion,  $e$ . Note that in a low-tax compliance setting, unlike as is generally modeled in the literature (Shimeles et al., 2017), informed by my survey data, I assume the social incentive to be strictly negative  $\Sigma < 0$ . This is consistent with an antisocial punishment mechanism wherein when most are not following the rules, desire for social conformity will incentivize being perceived as a rule breaker (Herrmann et al., 2008).

$$\frac{\partial e}{\partial q} = -\frac{\Sigma}{2\rho} > 0 \quad (2.4)$$

which implies:

### **Increasing social exposure of tax compliance *increases* evasion.**

However, the presence of a mechanism like antisocial punishment in the model does not preclude norms of tax compliance also playing a role in decreasing evasion. Note that the sense of obligation,  $\rho$ , and perceived fairness,  $g - wt$ , when taken together provide a model of ‘civic virtue’ consistent with Frey (1997). Then in this model, in the absence of other external incentives ( $f = 0$ ) or tax collection effort ( $\tau = 0$ ), compliance is motivated through the sense of obligation,  $\rho$ . This is reinforced when the tax is perceived to be generous ( $wt - g < 0$ ) but counteracted when the level of taxation is perceived to be unfair ( $wt - g > 0$ ). As a result, even in the general case, we have the following:

$$\frac{\partial e}{\partial \rho} = -\frac{e}{\rho} < 0 \quad (2.5)$$

which implies:

**Increasing the salience of obligations *decreases* evasion.**

In Chapter 1, I make the simplifying assumption that in a low-compliance setting the level of perceived fine is zero,  $f = 0$ , due to the state's lack of capacity and credibility. However, the model also predicts that efforts to increase the perceived fine, even if starting from essentially zero, should decrease evasion. This can be seen in:

$$\frac{\partial e}{\partial f} = -\frac{\tau}{2\rho} < 0 \quad (2.6)$$

This lends itself to making salient the fine so to test the prediction:

**Increasing the salience of penalties *decreases* evasion.**

## 2.4 Context

In the previous Chapter in 1.4, I provide more general background on Haiti's [History and Institutions](#). I also introduce the [Study City](#), Carrefour, Haiti, in greater detail including more comprehensive summary statistics (Table 1.2). As a reminder, Carrefour is one of the largest cities in the country with an estimated population of between 500,000-1,000,000 people. Globally poor with median monthly per capita household expenditures of about \$65 USD or just over \$2 USD/day (Figure A3), the city is considered middle class in Haiti serving as a bedroom community for the larger Port-au-Prince area of which it is one of six (6) independently governed municipalities.

I observe the universe of properties in the tax registry, approximately 50,000 over 331 census blocs. Property taxes are based on the estimated annual rental value of the property, and though progressive in structure, 84% of properties fall into the second-lowest bracket and owe 7% annually (Table 1.1). At baseline, tax compliance rates for those in the tax registry were 10% (Table 1.2). Comparing compliers to non-compliers, though there is no statistical difference in value of the tax bill, the data suggests that the visibility of the property may play a role in compliance (Table 1.3).

To shed light on the relationship between society and the state in this context, I spent a considerably amount of time working with the Mayor's office and engaging with the community of Carrefour. I conducted an extensive series of focus group discussions followed by a baseline survey of a representative sample of households prior to the start of interventions (see A.4 for survey tools). My data collection supports the intuition introduced in Section 1.3 and further developed in Section 2.3 that incentives to pay taxes in this setting are quite different than has generally been modeled in the literature. Specifically, responses are consistent with the modeling assumptions I have made regarding the incentives arising out of state capacity, civic virtue, public goods, and social incentives for optimizing the evasion decision. Respondents repeatedly highlighted the absence of the state in their lives and their limited trust in any state institutions or officials. As an example, survey responses revealed the

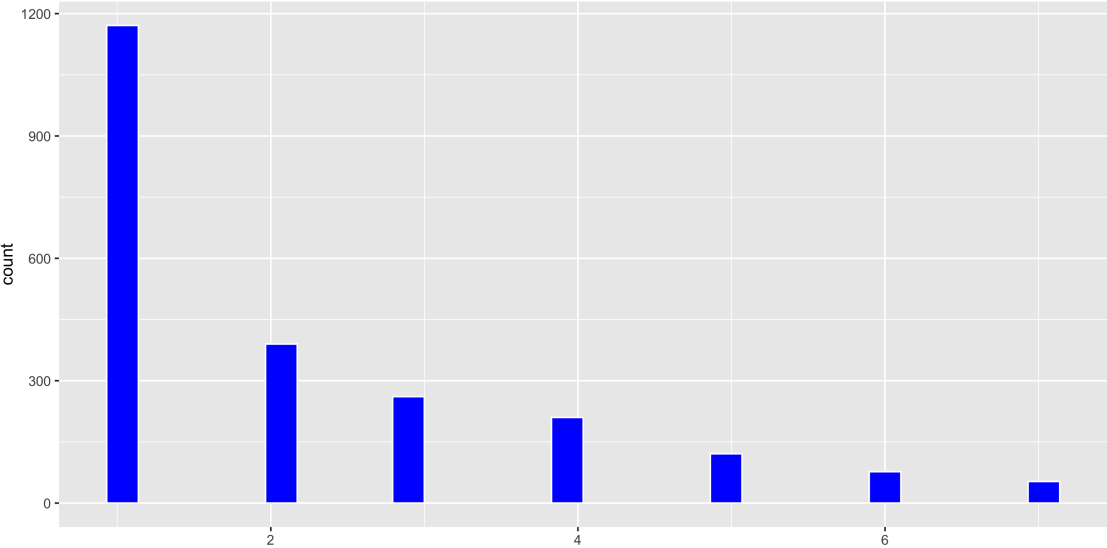


Figure 2.1: “I have confidence in the mayor.”

1 = Completely Disagree 7 = Completely Agree  
 Baseline Survey Responses from a representative sample of households. Respondents were asked to indicate how strongly they agreed with the statement “I have confidence in the mayor” on a 7-point scale with 1 indicating “Completely Disagree” and 7 corresponding to “Completely Agree”.

most trusted public official of those asked about to be the mayor, but still a large majority of people ‘completely disagree’ with the statement, ‘I have confidence in the mayor’ (Figure 2.1).

Focus groups and survey respondents consistently were able to identify the potential penalties for noncompliance - the most significant being property seizure. However, none reported having experienced or having heard of others experiencing this penalty. This is consistent with the Mayor’s own position at the start of this study that even if he had the capacity to enforce the prescribed penalties (which he indicated to not be the case), because so few people pay, any attempt to enforce the punishment would be perceived as arbitrary or as politically motivated retribution. Officials reported that the frequency of protests and concerns of civil unrest prevent the mayor’s office from taking any action in this regard. The dynamic of political protesting limiting the lawful action of the mayor’s office was directly observed on several occasions during this study, most notably in the repeated closure of the city dump due to protests by those living in close proximity to it. This is consistent with my modeling assumption of the perceived fine,  $f$ , being very small or even zero in this context.

At the same time, a large majority of survey respondents indicated a sense of civic obligation toward paying property taxes (Figure 2.2). The lack of external incentives and the strong internal motivation to pay are consistent with the set-up outlined by Frey (1997) and formalized in my model which anticipates a crowding-out effect from increased tax collection efforts.

With respect to public goods in my study setting, consistent with information from



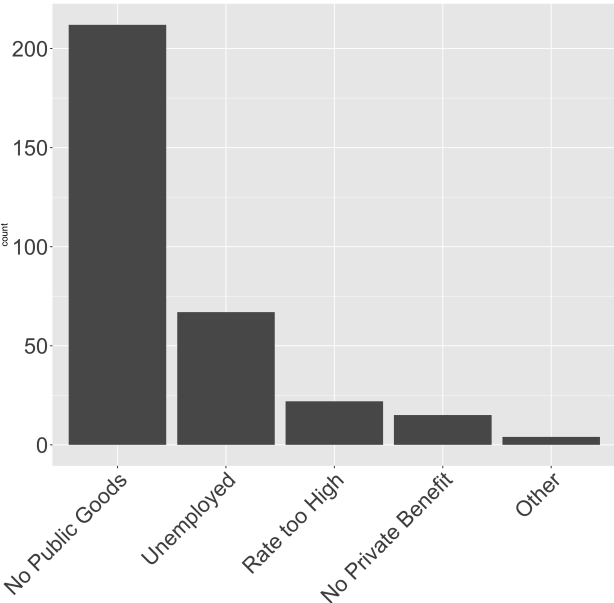


Figure 2.4: “Why do you believe your property taxes are NOT just?”

Baseline Survey Responses from a representative sample of households. Respondents responding “no” when asked, “Do you believe your property taxes are just?” were asked the open ended question, “Why do you believe your property taxes are not just?”

the mayor’s office and observed in the field, survey respondents indicated that there were essentially no services on offer, and they were not satisfied with this level of public goods (Figure 2.3). Furthermore, the lack of public goods provided is the most common reason given for the perceived lack of fairness of property taxes (Figure 2.4). As I highlighted in the previous Chapter, nearly all respondents referenced public goods in responding to the open ended question of “What would motivate you to pay property taxes?” (Figure 1.2). These responses are consistent with the level of public goods determining the perceived fairness of the tax, and tax evasion decreasing as public goods are increased.

Finally, focus group discussions indicated an aversion to discussing tax compliance with neighbors, particularly if one had paid one’s taxes. Survey responses were more polarized with most respondents divided between the extreme positions of “Completely Disagree” and “Completely Agree” in response to the statement “If I paid my taxes, I would want my neighbors to know” (Figure 2.5). Though this evidence does not fully support the modeling assumption of the social incentive depressing tax compliance, it does question the more common assumption that social exposure unambiguously encourages tax compliance.



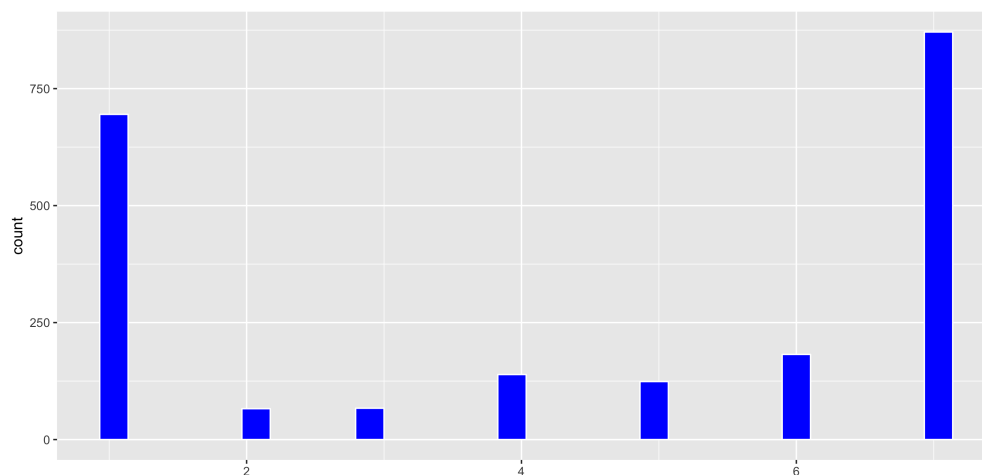


Figure 2.5: “If I paid my taxes, I would want my neighbors to know.”

1 = Completely Disagree

7 = Completely Agree

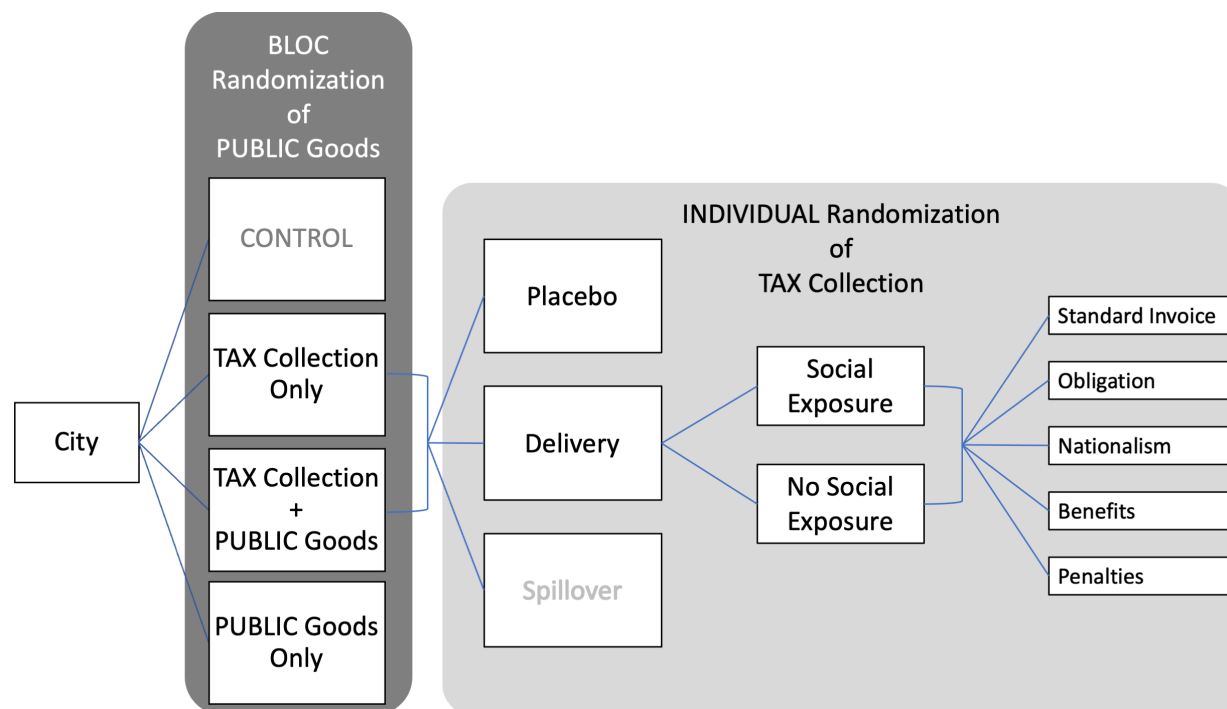
Baseline Survey Responses from a representative sample of households. Respondents were asked to indicate how strongly they agreed with the statement “If I paid my taxes, I would want my neighbors to know.” on a 7-point scale with 1 indicating “Completely Disagree” and 7 corresponding to “Completely Agree”.

## 2.5 Methodology

As discussed in greater detail in the previous Chapter in 1.5, the design is a stratified clustered randomization over the entire city (Figure 2.6). Census blocs were randomly assigned to one of four treatment arms: no change (“CONTROL”), receive only an increase in tax collection effort (“TAX Collection Only”), receive only an increase in public goods provision (“PUBLIC Goods Only”), or receive an increase in both tax collection effort and public goods provision (“TAX Collection + PUBLIC Goods”). Then, conditional on being randomly assigned to receive the tax collection treatment at the bloc-level, properties were further randomized at the property-level to either receive a visit from a tax agent who hand-delivered and discussed a personalized tax invoice for the property taxes due for the fiscal year 2019 (“Delivery”), a personalized visit from a representative of the local government who hand-delivered a personalized bank holiday calendar (“Placebo”), or no visit at all (“Spillover”).

Conditional on being randomly assigned to receive a visit from a tax agent (“Delivery”), properties were further randomized into various treatments to further test motivations in two stages. First, properties were randomized as to whether or not the invoice signaled “Social Exposure”. As discussed with respect to the existing literature in 2.2, modeled in 2.3, and consistent with my findings from focus groups and survey data (Figure 2.5), concerns about the possibility of your neighbor becoming aware of your tax compliance decision may be quite important in maintaining the stability of the low-tax equilibrium in low-compliance settings. To experimentally test my model’s prediction that increasing social exposure of tax compliance increases evasion, I introduce a novel treatment for increasing probability

Figure 2.6: Experimental Design Tree – Full



Bloc randomization in dark gray depicts the same design information as presented in the “Factorial Design for Treatments (2x2)” in Figure 1.3. The light gray area, as in Figure 1.4, depicts the additional individual-level randomization conditional on being assigned tax collection treatment (either alone or with public goods) at the bloc-level. “Placebo” indicates that a property is assigned to receive a bank holiday calendar. The findings for this treatment are found in 1.7. “Delivery” indicates that a property is assigned to receive an invoice. Conditional on being assigned to “Delivery”, properties are first randomized as to whether or not the invoice signals “Social Exposure” by committing, “If you pay your property taxes in the next 30 days, the Mayor will place a sign indicating payment on the exterior wall of your house facing the street.” Finally, properties are randomly assigned to receive no additional information as part of the visit (“Standard Invoice”) or one of the following information treatments making salient different motivations, “Obligation”, “Nationalism”, “Benefits”, or “Penalties”. A full summary of individual-level treatments are also summarized in A.3.

of social exposure of tax compliance (Figure 2.7). Treated households were notified “If you pay your property taxes in the next 30 days, the Mayor will place a sign indicating payment on the exterior wall of your house facing the street.” (2.10). Second, properties were randomly assigned either to receive no additional information as part of the visit (“Standard Invoice”) or to receive an additional information treatment or ‘nudge’ designed to make salient additional motivations. The full experimental design is summarized in Figure 2.6.

I spent a significant amount of time working with the Mayor’s office, National Tax Authority, and local community to inform and prepare for this experiment. Together we simplified the design of the tax invoices to make the information more accessible. As can be seen in Figure 2.8, the redesigned invoice included a general reminder of property tax obligations, a prominent clarification of the deadline at the top, and information for how to proceed with payment. It also included one’s name, address, estimated property value, and total tax

Figure 2.7: Stickers Indicating Tax Compliance



(a) Sticker design

The center is the seal of the city saying “Mayor of Carrefour” and the edges indicate “Paid” and the corresponding fiscal year “2018-2019”.



(b) Example in the field

See middle pane window just to the right of the red door.


Half of all properties in invoice treatment blocs are informed that if they pay their taxes in the 2018-2019 fiscal year, the mayor will affix a sign indicating payment on the outside of their house facing the road. I designed this treatment in partnership with the Mayor’s office. For the 2019-2020 fiscal year the design is the same but the color of the background is now red. This chapter only presents the effects of being informed of this treatment, in a subsequent companion paper I will present the effects of receiving these stickers as well as the effects of seeing your neighbors receive them on tax compliance.

bill. We then scripted and refined the engagement that the tax collection team used when approaching each property and used smart phones to both provide tailored scripts as well as to track the progress and accuracy of implementation (see [A.3](#)).

The redesign also made it possible to incorporate and reinforce the various messages I test in this chapter to make salient various motivations. As an example of how the invoices differed, in Figures [2.9](#) and [2.10](#) the standard invoice is contrasted with the invoice signaling social exposure. These messages were then included in the scripts tailored for each property in compliance with the study design. We spent a considerable amount of time training the agents to ensure they understood and would comply with the scripts as presented to them on their smart phones at each house. We also closely monitored their implementation throughout.


Figure 2.8: Standard Invoice

LIBERTÉ                      EGALITÉ                      FRATERNITÉ



**REPUBLIQUE D'HAÏTI**  
**MAIRIE DE CARREFOUR**

**BORDEREAU CFPB (IMPÔT LOCATIF) 2018-2019**  
**À PAYER À LA DGI AVANT LE : 30 Avril 2019**



IDENTIFICATION	CONTRIBUABLE	PROPRIETAIRE	IMMEUBLE	Année Fiscale
NOM ET PRÉNOM				2018-2019
ADRESSE			DATE D'EMISSION	1 Octobre 2018
			DATE D'ÉCHÉANCE	30 Avril 2019
TELEPHONE			DISTRICT :542	
MATRICULE			BLOC :202	

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VALEUR LOCATIVE APPLIQUÉE : 131,250.0      LOYER DÉCLARÉ 75,000.00      TAUX : 8.00%

**INFORMATION SUR LES CONTRIBUTIONS À PAYER**


CONTRIBUTIONS COMMUNALES		CONTRIBUTIONS TRESOR PUBLIC	
Montant principal	10,500.00	Taxe additionnelle (10%)	1,050.00
		DSAV (2/1000)	21.00
			0.00
<b>Total contribution communale</b>	<b>10,500.00</b>	<b>Total contribution Trésor Public</b>	<b>1,071.00</b>

**CONTRIBUTION TOTALE À PAYER EN GOURDES : 11,571.00**

**AUTRES EXERCICES A PAYER : 2010-2011, 2011-2012, 2012-2013, 2013-2014, 2014-2015, 2015-2016, 2016-2017, 2017-2018**

**IMPORTANT :** La mairie de Carrefour invite les contribuables de la commune de Carrefour à verser le montant mentionné ci dessus, sans addition des surtaxes de retard, à condition que le paiement se fasse avant le 30 Avril 2019. Passé ce délai, le centre des impôts de Carrefour (CDI) calculera le montant des surtaxes à raison de 5% par mois de retard et exigera leur paiement. Tel 22260726

Gagnez du temps et de l'effort! Avec bordereau en main, veuillez-vous présenter directement à la DGI pour faire le paiement. La DGI est située entre Thor 14 et Cote Plage 16 entre 9h00 AM et 4h00 PM du Lundi au Vendredi.

  
 Directeur des affaires Administratives et Financiere

TYPE :A



Informed by the literature discussed in 2.2 and the modeling from 2.3, I worked with focus groups and community organizations to developed several different locally-relevant frames aimed at evaluating the remaining predictions<sup>6</sup>.

To test the prediction that increasing the salience of obligations decreases evasion, I developed two different messages, one appealing to cultural norms of mutual support and the other appealing to national obligations for collective action. The first was an appeal to Kreyol proverbs, a culturally significant collection of folk knowledge that plays a substantial role in everyday communication. I identified two proverbs as particularly resonant with relevant stakeholders. The first, “Men anpil, chay pa lou” literally translates to “Many hands, load not heavy” which is analogous to the English proverb, “Many hands make light work.” The second, “Yon sel dwet pa manje kalalou,” translates to “You can’t eat ‘kalalou’ with just one finger.” Kalalu is a very thick and sticky soup made of green vegetable leaves that has been traditionally eaten by dipping all of one’s fingers tightly cupped into the bowl so to laddle the viscous meal into one’s mouth. Both proverbs emphasize the need for many people to come together to accomplish an important task. The second carries an additional emphasis of a unified identity emerging from all of the individuals coming together (the fingers together make up the hand), and that unity being necessary for survival (working together is necessary in order to eat). To appeal to similar norms of obligation rooted instead in national identity and the state, I found the opening lines of the Haitian National Anthem to be most resonant. It can be translated as, “For the country, For the ancestors / Let us march united. Let us march united. / Let there be no traitors in our ranks! Let us be masters of our soil. / Let us march united. Let us march united.” The National Anthem is widely sung and played at the start of many community events. The rest of the song continues to emphasize the need for everyone to work together for their mutual success.

To test the last prediction that increasing the salience of penalties decreases evasion, I worked to identify the most credible penalty in this setting of weak state capacity. Focus groups consistently indicated that the larger potential penalties of legal consequences including property forfeiture are not of concern. Even the mayor had never heard of any of these consequences being attempted. Instead, I found in focus groups that the more incremental penalties could be seen as potentially enforceable. As a result, I used the following: “Property taxes are due at the end of the month of APRIL! Know that for each month of delayed payment, your total tax bill increases by 5%.”

Finally, though not discussed in my modeling exercise explicitly, I also tested making even more explicit that the public goods being provided in this experiment are paid for by the taxes that this experiment sought to collect. As discussed in 1.5, trash collection was identified in my surveys as the top priority that the people wanted from their government (Figure 1.6). The mayor’s trash collection trucks then had the equivalent of “Property Taxes + Business Taxes = Services” painted on each side of each vehicle. In this setting, some were even further reminded of the benefits of taxes with the message “Remember that Property Taxes are the principal source of financing for public goods: garbage removal and street

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<sup>6</sup>A summary and examples of each invoice can be found in the Appendix A.3.

cleaning. Your contribution will make these services possible for you and your community.”

## 2.6 Results

To identify the effect of increasing social exposure, I estimate:

$$Y_{ib} = \beta TAX_{ib} + \delta PUBLIC_b + \kappa SOCIAL_{ib} + \gamma FRAME_{ib} + \mathbf{X}_{ib}\Theta + \mathbf{Z}_b\Psi + \phi_s + \epsilon_{ib} \quad (2.7)$$

As in Chapter 1,  $Y_{ib}$  is the outcome of interest for property  $i$  in bloc  $b$ .  $TAX_{ib}$  is an indicator variable equal to 1 if property  $i$  in bloc  $b$  is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise.  $PUBLIC_b$  is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise.

I add to this,  $SOCIAL_{ib}$ , an indicator variable for random assignment to be notified that the mayor would place a sign on their door. Then,  $FRAME_{ib}$ , is now the indicator variable for random assignment to also receive additional framing *other than* the social exposure treatment.

I continue to include individual covariates,  $\mathbf{X}_{ib}$ , including the valuation of the property, the total tax bill, and past tax payment history as well as bloc covariates,  $\mathbf{Z}_b$ , incorporating the population density, number of registered properties, and average level of access to services like roads, power, and water. Stratum fixed effects,  $\phi_s$ , are included in all specifications as standard errors,  $\epsilon_{ib}$ , clustered at the bloc-level to correspond to the primary level of randomization.

Again, in order to test if the effects of receiving both tax collection and public goods together are different than simply adding their individual effects, I also estimate the interactions:

$$Y_{ib} = \beta TAX_{ib} + \delta PUBLIC_b + \kappa SOCIAL_{ib} + \gamma FRAME_{ib} + \mu TAX_{ib} \times PUBLIC_b + \lambda PUBLIC_b \times SOCIAL_{ib} + \nu PUBLIC_b \times FRAME_{ib} + \mathbf{X}_{ib}\Theta + \mathbf{Z}_b\Psi + \phi_s + \epsilon_{ib} \quad (2.8)$$

Table 2.1, presents the results. Columns 1 and 3 indicate that the Social Exposure treatment does not have a statistically significant additional effect on tax payments either as an interaction or in a linear combination with tax collection. However, Columns 2 and 4 provide evidence that this is because the sign of the social exposure treatment is determined by the interaction with public goods. For properties that are only receiving tax collection, the additional Social Exposure treatment is economically and statistically significant causing

Table 2.1: Effects of Social Exposure of Tax Compliance on Amount of Taxes Paid by Property

	Amount of Tax Paid in USD			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-4.01* (2.31)	-5.16* (2.85)	-5.02 (3.76)	-5.11 (5.14)
Public Goods	3.57* (1.97)	0.47 (1.61)	3.18 (2.68)	1.34 (2.64)
Social Exposure	2.56 (3.23)	-2.68** (1.26)	-2.31 (4.39)	-9.23** (4.35)
PUBLIC X TAX		3.19 (4.47)		0.90 (7.40)
PUBLIC X SOCIAL		15.56 (9.77)		19.50* (11.57)
TAX + SOCIAL	-1.45 (3.98)	-7.84 (3.04)	-7.32 (5.05)	-14.34 (5.32)
Full Linear Combination		11.39 (9.48)		7.4 (10.47)
Mean	13.1	13.1	21.86	21.86
Observations	40,060	40,060	40,060	40,060
Adjusted R <sup>2</sup>	0.02	0.02	0.03	0.03

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. “TAX + SOCIAL” presents the estimated coefficient of a two-sided general linear hypotheses test of the linear combination of the coefficients on “Tax Collection”, and “Social Exposure” equaling zero, and the corresponding clustered standard errors are presented in parentheses. “Full Linear Combination” presents the estimated coefficient of a two-sided general linear hypotheses test of the linear combination of all coefficients shown equaling zero, and the corresponding clustered standard errors are presented in parentheses. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{it}$  indicator which control for additional randomly assigned variations in implementation of the “tax collection” treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$



an additional reduction in tax payments of \$2.68 or 20% in the first 12 months and \$9.23 or 42% over the full 18 months.

The Social Exposure treatment was delivered as part of the tax collection intervention and therefore we must test its linear combination with Tax Collection to observe the overall effect. To do this, I estimate coefficients of a two-sided general linear hypotheses test and find the effect is also statistically significant as indicated in the lower panels for both Columns 2 and 4. In the absence of public goods, attempting to collect taxes while signaling that one's tax compliance decision would be made known to one's neighbors resulted in a loss of \$7.84 or 60% in the first 12 months and \$14.34 over the observed period or a 66% loss in revenue. I interpret this as evidence of antisocial punishment and these findings are consistent with the prediction that increasing social exposure of tax compliance increases evasion.

Where public goods are also received, the Social Exposure effect changes signs and causes a large, and in the case of Column 4, statistically significant, increase in tax payments as an interaction effect. The \$19.50 increase over the full 18 months represents 89% of the average tax payment. The full linear combination however for both Columns 2 and 4, though positive and large, are imprecise. The \$7.40 net increase in tax payments over 18 months shown would correspond to a 34% increase in tax revenue. In comparing the point estimates between the interaction terms, these findings also suggest that a majority of the positive interaction effect observed between tax collection and public goods is from the addition of the signal of social exposure. These findings provide suggestive evidence that as public goods are introduced, people become more willing and perhaps even interested in having their neighbors know their compliance decision. Providing public goods thus mitigates the antisocial punishment mechanism and potentially leads toward the pro-social signaling previously documented in the literature.

To further test other motivations in this setting, I estimate the following:

$$Y_{ib} = \beta TAX_{ib} + \delta PUBLIC_b + \gamma_j \sum_1^J FRAME_{jib} + \mathbf{X}_{ib}\Theta + \mathbf{Z}_b\Psi + \phi_s + \epsilon_{ib} \quad (2.9)$$

$$Y_{ib} = \beta TAX_{ib} + \delta PUBLIC_b + \gamma_j \sum_1^J FRAME_{jib} + \mu TAX_{ib} \times PUBLIC_b + \nu_j PUBLIC_b \times \sum_1^J FRAME_{jib} + \mathbf{X}_{ib}\Theta + \mathbf{Z}_b\Psi + \phi_s + \epsilon_{ib} \quad (2.10)$$

Where  $FRAME_{jib}$  is now the full set of different indicator variables for random assignment to also receive additional framing or 'nudges' accompanying the property tax invoice. Each  $j$  representing a specific frame or 'nudge' and  $j = 1$  being the social exposure treatment from the previous section.

Table 2.2: Effects of Framing Tax Collection on Amount of Taxes Paid by Property Showing Results for all Frames

	Amount of Tax Paid in USD			
	FY19		FY19-20	
	(1)	(2)	(3)	(4)
Public Goods	3.6* (2.0)	0.5 (1.6)	3.3 (2.7)	1.3 (2.6)
Tax Collection	-3.9* (2.3)	-5.1* (2.8)	-4.3 (3.7)	-4.5 (5.0)
Benefit of Compliance	4.0 (3.2)	5.3 (3.3)	8.4* (5.0)	9.4 (6.0)
Penalties	4.0 (2.9)	6.4** (3.0)	6.0 (4.2)	8.7* (4.7)
Nationalism	1.3 (2.7)	6.0** (2.8)	10.3 (8.3)	19.3 (12.6)
Obligation	12.9 (8.2)	9.3* (4.8)	17.2* (8.8)	14.3** (7.1)
Public X TAX		3.4 (4.4)		1.1 (7.4)
Public X BENEFIT		-3.0 (6.9)		-2.5 (11.3)
Public X PENALTIES		-5.9 (5.8)		-6.4 (8.2)
Public X NATIONALISM		-12.8** (5.3)		-24.5* (13.9)
Public X OBLIGATION		10.3 (22.6)		8.2 (23.3)
Mean	13.1	13.1	21.86	21.86
Observations	40,060	40,060	40,060	40,060
Adjusted R <sup>2</sup>	0.02	0.02	0.03	0.03

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Results are presented in Table 2.2. In all columns, unlike the negative effect of tax collection seen in this setting, each of the motivations has a positive point estimate suggesting that people respond to each by increasing tax compliance. Column 3 shows that emphasizing the benefits to tax compliance (specifically the public goods received) as well as the social obligations to support one's community based on the Kreyol proverbs are statistically significant over the full period. Social obligations continue to be statistically significant in columns 2 and 4, controlling for the interactions with public goods, as are making salient penalties and the appeal to obligation based on nationalism. While noisy, the interaction estimates each are consistent with the effect of these motivations diminishing in the presence of public goods for all but appeals to social obligations which is consistently positive. Of note, even the interaction of public goods with the message emphasizing the direct relationship between those goods being received and the taxes being requested has a negative point estimate on its interaction term – though this is quite imprecise with standard errors up-to 4.5 times larger than the estimate itself.

As above, it is important to remember that each of these motivations were delivered as part of the tax collection intervention and therefore we must test the linear combination of Tax Collection with each to observe the overall effect. To do this, I again estimate coefficients of a two-sided general linear hypotheses test of the linear combinations of the relevant coefficients. I find that the impacts of appealing to benefits, penalties, and nationalism in linear combination with tax collection as well as their interactions with public goods are imprecisely estimated but are consistent with the findings above that these motivations effectively counteract the negative effect of tax collection effort in this setting. I do however find some statistically significant net effects from appealing to social obligations. I present these in Table 2.3. Most notably in the lower panel of Column 4 for areas not receiving public goods, framing tax collection by making salient cultural appeals to obligations caused an increase in tax payments of \$9.82 or about 40% more than the mean of \$21.86.

It is notable that even in this low-compliance setting, while the effect of attempting to collect taxes is negative as can be the attempt to leverage social norms (in the absence of public goods), these other motivations play a role consistent with what has been found in the rest of the literature. Moreover, even the negative effect of tax collection can be reversed with a sufficiently strong appeal to culturally-rooted obligations.

## 2.7 Discussion: Costs and Benefits

The cost effectiveness of implementing these interventions is also relevant for policy. As discussed in Chapter 1, each delivery of the standard tax invoice lost approximately \$8.29 in the first year. The Social Exposure treatment in areas not receiving public goods further compounded the losses to a total of \$9.84 per taxpayer - equivalent to losing about 3/4 of property tax revenue for the city or nearly \$400,000 if this least effective approach had been universally implemented.

One variation on the tax collection implementation was able to mitigate the negative

Table 2.3: Effects of Framing Tax Collection on Amount of Taxes Paid by Property

	Amount of Tax Paid in USD			
	FY19		FY19-20	
	(1)	(2)	(3)	(4)
Public Goods	3.6* (2.0)	0.5 (1.6)	3.3 (2.7)	1.3 (2.6)
Tax Collection	-3.9* (2.3)	-5.1* (2.8)	-4.3 (3.7)	-4.5 (5.0)
Penalties	4.0 (2.9)	6.4** (3.0)	6.0 (4.2)	8.7* (4.7)
Obligation	12.9 (8.2)	9.3* (4.8)	17.2* (8.8)	14.3** (7.1)
Public X TAX		3.4 (4.4)		1.1 (7.4)
Public X PENALTIES		-5.9 (5.8)		-6.4 (8.2)
Public X OBLIGATION		10.3 (22.6)		8.2 (23.3)
TAX + PENALTIES	0.01 (2.29)	1.3 (2.12)	1.65 (3.42)	4.21 (4.04)
PENALTIES Linear Combination		-0.77 (4.29)		0.21 (5.67)
TAX + OBLIGATION	8.93 (7.51)	4.12 (2.81)	12.85 (7.98)	9.82 (4.62)
OBLIGATION Linear Combination		18.25 (20.03)		20.42 (20.15)
Mean	13.1	13.1	21.86	21.86
Observations	40,060	40,060	40,060	40,060
Adjusted R <sup>2</sup>	0.02	0.02	0.03	0.03

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. “TAX + PENALTIES” presents the estimated coefficient of a two-sided general linear hypotheses test of the linear combination of the coefficients on “Tax Collection”, and “Penalties” equaling zero, and the corresponding clustered standard errors are presented in parentheses. “PENALTIES Linear Combination” presents the estimated coefficient of all coefficients contributing to the full interaction effect of PUBLIC, TAX and PENALTIES equaling zero, and the corresponding clustered standard errors are presented in parentheses. “TAX + OBLIGATION” presents the estimated coefficient of “Tax Collection”, and “Obligation” equaling zero, and the corresponding clustered standard errors are presented in parentheses. “OBLIGATION Linear Combination” presents the estimated coefficient of all coefficients contributing to the full interaction effect of PUBLIC, TAX and OBLIGATION equaling zero, and the corresponding clustered standard errors are presented in parentheses. All regressions also control for the remaining  $j - 2$  frames separately tested. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

effects and resulted in a gain even in the absence of public goods. Per Column 4 of Table 2.3 and taking into account the costs of implementation mentioned above, appealing to social obligations with culturally relevant proverbs resulted in an average net increase in revenue of \$7.82 per delivery over the first 18 months of data collected thus far. This is a 35% increase in revenue per taxpayer. If the city had implemented a well-framed tax collection intervention like this one, the results suggest the potential for an average of \$210,000 in additional revenue in the first year.

Finally, in Chapter 1, I find the public goods intervention to be cost effective, and this Chapter further reinforces that conclusion. Table 2.1 indicated that public goods interacted with Social Exposure of tax compliance to mitigate the antisocial punishment mechanism that likely is contributing to the stability of the low-tax equilibrium. Furthermore, suggestive evidence indicated that public goods may also shift the effect of Social Exposure to one of pro-social signaling which could potentially result in increased revenue over time.

## 2.8 Conclusion

Development, and specifically, state formation, requires navigating the at times competing interests of society and the state. While the tax literature has to this point generally supported conclusions that social norms are either neutral or supportive of state consolidation and capacity building, this research has almost exclusively been based on evidence from modern states and medium- and high-income countries. In this Chapter I provide evidence from one of the poorest and lowest capacity states in the world of how social norms can be leveraged to reinforce state formation, but also how the state can inadvertently undermine its own efforts when appealing to social norms.

I overcome prior limitations with respect to implementation and context to provide new evidence on the role of social norms in a setting with low rates of pro-social coordination. In a setting of near-zero tax compliance and very low state capacity, I partner with the national tax authority, the mayor of one of the largest cities in the country, and a local software firm over multiple years. Through extensive local engagement with relevant stakeholders including novel qualitative and quantitative data collection, I document evidence suggesting differences in relevant social norms from the economics literature. I use these findings to model the evasion decision in this context. Building on field work and local knowledge, I develop several novel treatments and combine my surveys with new administrative data to test the corresponding predictions. In the context of cross-randomizing tax collection effort and public goods provision at the census bloc-level over the entirety of my study city, I identify effects from further individual-level randomization of the approximately 50,000 properties.

I introduce to the tax literature the mechanism of antisocial punishment. Through a novel treatment I experimentally increase social exposure of tax compliance. I find that taxpayers respond by further reducing tax payments by 66% over the full 18 months. However, I find

this negative effect mitigated when randomly combined with receiving an increase in public services with suggestive evidence that in combination net tax payments increase.

I also indirectly test other motivations for tax compliance by randomizing the framing of tax collection engagement. I find evidence that each appeal tested reduced the adverse tax collection response even in the absence of public goods. I find that even in this setting where threats of enforcement from the state are not credible, making salient incremental financial penalties prevent the negative response to tax collection efforts in this setting and may even result in a net increase in revenues. Using local proverbs to make salient cultural norms of obligation and shared responsibility results in net tax payment increases of more than 40% with evidence that effects may be even larger when combined with public goods.

I discuss the cost effectiveness of these interventions to increase tax revenue. I find that appeals to social exposure could instead cost the government 3/4 of property tax revenue when operating costs are included, nearly \$400,000 a year in total. If instead the city were to leverage the cultural appeal to obligation, it may gain \$210,000 in additional annual revenue. Finally, as antisocial punishment can be mitigated by increasing public goods, and such services may further promote pro-social norms, these findings provide additional evidence of the cost effectiveness of increasing public goods to increase tax revenue.

These findings point to additional questions for future research. As a first step, this work suggests the need for a systematic examination of the norms tested thoroughly in more developed settings to understand which are operative in states of earlier stage development as well as under what other social conditions might different norms exist and emerge. Of particular concern is identifying under what conditions might a state need to anticipate antisocial punishment so to avoid inadvertently pursuing self-defeating public policies. Second, the evidence suggesting public goods provision can move antisocial punishment to more pro-social responses points to a need to further interrogate this dynamic to understand the inflection point as well as the durability of these changes over time. Finally, understanding the general equilibrium effects will be essential for ensuring effective long-term policy recommendations. The above will be quite helpful for low-income states pursuing their paths of development so that they might benefit from leveraging pro-social norms while avoiding the pitfalls of antisocial punishment – learning when to rely on society’s support as an ally and how to avoid society’s backlash as an adversary.

# Chapter 3

## Public–Private “Partnership”

### *Improving Welfare and Reducing Corruption in Haiti’s Universal Primary Education Program*

#### 3.1 Introduction

For Least Developed Countries (LDCs), the challenges of governance are acute. National budgets generally represent less than 100 USD per citizen per year even before purchasing power is eroded by bureaucratic barriers, clientelism, and graft (Thomas, 2009). Increasingly, multinational institutions and donor governments are promoting Public–Private Partnerships (PPPs) to leverage these limited funds as incentives for the private sector to provide more of the services traditionally identified with government. In general these sorts of arrangements change the responsibilities of government officials, introduce parties into the public sector who have different (and potentially misaligned) incentives, and create new opportunities for both socially beneficial collaboration as well as collusion and capture for private gain. For LDCs these impacts are likely more significant as they are often characterized by an understaffed public sector paid significantly less than private sector counterparts, *de facto* unregulated markets, and permissive institutional environments with regards to misappropriation and corruption.

In this chapter, I examine one such PPP in Haiti, the government’s universal primary education initiative. Beginning in 2011, this effort aimed to increase enrollment by directly subsidizing private schools to provide free primary education. Though initially launched with limited oversight, the government sought to improve accountability two–years into implementation by sending teams to visit each participating school. As a result, this initiative provides an opportunity to evaluate the impact of both a PPP on the extensive margin of service delivery as well as basic accountability efforts on the amount of corruption within the system. Specifically, this chapter seeks to answer the following: What was the impact of the government of Haiti’s subsidies for private schools on the rate of primary school

enrollment? What were the welfare effects of this program for households? What was the impact of the government of Haiti’s audits on the level of fraud within the system? To address these questions, I make use of household-level data from USAID’s Demographic and Health Surveys (DHS), exam scores from Haiti’s Ministry of Education and Professional Training (Ministry of Education), registration by and payments to persons operating private schools – who I refer to in this chapter as ‘education entrepreneurs’ – participating in the PPP from the National Bank of Haiti, and finally audit data from the office of the Prime Minister.

I begin by examining evidence of the PPP’s impact on school attendance and household welfare. The government provided sufficient transfers through the PPP to fund schooling for every child between the ages of 6 and 11 in the country (1.3 million). First I show that the initiative coincided with an increase in primary school enrollment from under 50% to more than 80%. I then use a cohort study to causally identify the impact of the program. I find that each PPP-funded school per 1,000 students in a given administrative region increases the probability by 1-1.3% that a student just old enough to start school at the launch of the PPP will be enrolled. Using the same specification, I also identify small but significant improvements in household welfare as measured in asset accumulation, savings, and consumption.

I then identify the impact of audits on the behavior of education entrepreneurs. I use a difference-in-difference model at the level of the school with time-by-commune<sup>1</sup> fixed effects. I find a 18.9% reduction of grant claims on the government corresponding to a reduction in reported enrollment in the program of 6.7 students per audited school. These reductions are interpreted as corresponding to fraudulent claims and thus the resulting difference in sample of 875,000 USD per year is seen as a savings resulting from this anti-corruption measure. As the expenditures for audits and related data management cost approximately 80,000 USD in total, the resulting savings indicates that 11 USD is the lower bound of the return on investment for every dollar spent on the audit program. The school audit is also associated with a 7.6% increase in schools exiting the program all together which is consistent with the predicted response by corrupt participants as modeled in this chapter. I estimate losses of 11 million USD associated with implementing this universal primary education initiative without adequate oversight from the outset. I do not find evidence of spillover effects on non-audited schools. Finally, I leverage the panel nature of the data to conduct an event study and find results which are consistent in sign and of even greater magnitude.

The rest of the chapter is structured as follows: Section 3.2 reviews the relevant literature. I provide additional background on the Context in Section 3.3 which I then use in Section 3.4 to develop a simple model to inform the research. In Section 3.5, I summarize the various data sources employed in this analysis, and I present my empirical approach and results in Section 3.6. I review the implications for policy in Section 3.7 and then conclude in Section 3.8.

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<sup>1</sup>The government of Haiti is administratively divided as follows: Admin1: 10 “departements”; Admin2: 42 “arrondissements”; Admin3: 145 ‘communes’; and Admin4: 571 “section communales” which I refer to as “sections” in this chapter.



## 3.2 Literature Review

As discussed in the introduction, this work engages with and contributes to several different literatures within political economy and development. It is rooted in and builds upon the work of Acemoglu and Robinson ([Acemoglu et al., 2012](#); [Acemoglu and Robinson, 2016, 2008](#)) as it engages with the lack of institutions as an impediment to development while evaluating PPPs, and in particular Haiti’s attempt to provide incentives to the private sector to increase education supply, as a potential hybrid response to the challenges facing these countries. Furthermore this work is informed by the principle–agent models developed by [Grossman and Hart \(1983\)](#) and multitasking model of [Holmstrom and Milgrom \(1991\)](#) as they examine a similar situation wherein a government must evaluate how best to provide incentives to (albeit publicly employed) teachers. The primary focus of this chapter, the response of education entrepreneurs to school audits, most directly engages the long literature on the efficiency losses from rent–seeking, corruption and other forms of government misappropriation by state actors – or in this case, actors on behalf of the state – as well as the more recent literature on how government actions can mitigate such abuses ([Krueger, 1974](#); [Becker, 1974](#); [Shleifer and Vishny, 1993](#); [Reinikka and Svensson, 2005](#); [Olken and Barron, 2009](#); [Ferraz et al., 2012](#)). As this work examines how citizens seek to defraud the government, it is also contributing to literature on tax avoidance and other means by which the citizenry takes advantage of the state ([Allingham and Sandmo, 1972b](#); [Slemrod, 2007](#); [Pomeranz, 2015](#)). Finally, as discussed in the modeling exercise, this chapter builds on and contributes to the literature of political connectedness ([Fisman, 2001](#); [Duchin and Sosyura, 2011](#); [Cruz et al., 2015](#); [Fafchamps and Labonne, 2016](#)).

## 3.3 Context

Well known as the poorest country in the Western Hemisphere (See [Section 1.4](#)), education and literacy rates in Haiti have also lagged behind with only about 50% of primary school–aged children enrolled in classes. Historic precedents, weak institutions and limited capacity on the part of government have resulted in 95% of education being provided by private institutions. For this reason, in 2011 when the government of Haiti launched its universal primary education initiative, “*Le programme de scolarisation universelle, gratuite et obligatoire*” (“The Universal, Free and Required Schooling Program”), it attempted to do so through private sector education providers. This PPP began as an open offer from the central government to all persons operating private schools in the country. Specifically the central government offered to pay 90 USD per pupil per year divided up into three payments of 30 USD per trimester to any entrepreneur that provided primary education without collecting tuition and fees directly from students’ families.

In the first years of the PPP, due to administrative complications and rushed implementation, roll–out was staggered across the country. Nevertheless, the program continued to expand. The ruling political party, Parti Haïtien Tèt Kale (PHTK) touted the program as

its greatest success citing 1.2 million additional children enrolled in school across national media and in political signs pasted across the country. While there have been mixed anecdotal reports regarding the efficacy of the program, the overall popularity of the PHTK was evident in their subsequent electoral victories.<sup>2</sup>

Two years after the initial roll-out, in an effort to strengthen accountability in the system, the then-Prime Minister decided to create an online portal to publicly disseminate information about all participating schools. This portal was to include photographs of the school administrators and education facilities as well as physical address, GPS coordinates, class lists and aggregate performance on national exams. In order to populate the site, he hired an IT firm to visit each of the more than 10,000 schools enrolled in the system. In the process, they began to create the first national map of schools while also serving as a check against rumors of “ghost schools” collecting payments but not educating students.

This chapter makes use of the natural experiment resulting from the quasi-random implementation of these visits<sup>3</sup>. Logistic, budgetary, and political constraints prevented the teams from setting their own travel schedule and ultimately caused the visits to be abruptly stopped. To overcome the constraints they faced, the teams were added to the unrelated “*Gouveneman Lakay*” (GOL – “Government at Home”) program whose schedule was determined by the Government Cabinet – a separate entity than that which oversaw the education initiative. The GOL schedule was reportedly erratic in both its timing and locations. Only a few trips were completed each month. Furthermore, none of the members of the school audit teams had a seat in the Government Cabinet meetings. Panel (d) in Figure 3.1 demonstrates which sections were visited by the team, and of particular note is that it is an assortment of different regions consisting of both urban centers and rural areas.

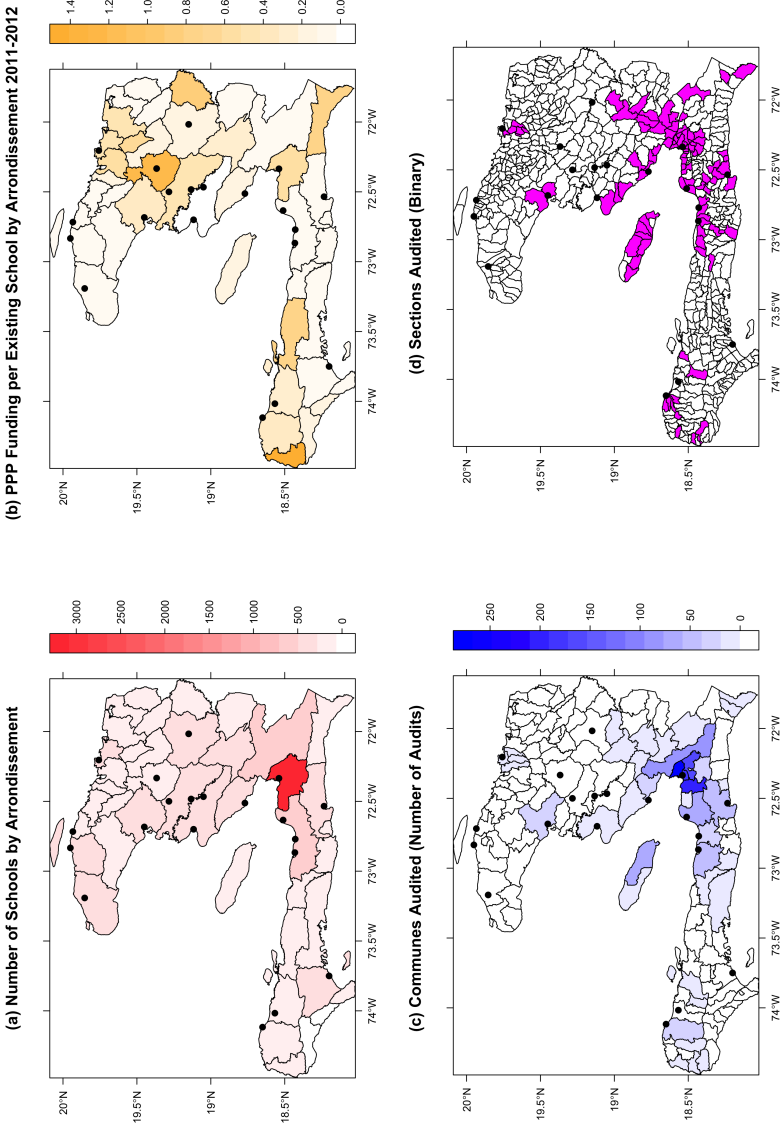
For each visit, the audit team was provided a list of confirmed participating schools from the National Bank prior to travel. This list was then divided among the team and upon arriving in a section, each person was assigned responsibility for locating a subset of the schools. This was all coordinated internally among the team and there were not outside attempts to influence their visits. Furthermore, as they had to move when the GOL program was completed with its work in the area, the team generally did not have sufficient time to attempt to visit all schools in a section. Furthermore, as the teams stayed entirely within the Commune of any one audit trip, for the primary specification of this chapter I limit my analysis to only those communes that had one or more section receive a audit. Panel (c) in Figure 3.1 maps the communes that received the team and the relative number of schools audited in each. Based on the above, I argue that conditional on commune, the

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<sup>2</sup>Though the PHTK did not exist as a political party prior to 2010, their candidate won the Presidency in 2011. Then in October of 2015, following the launch of the PPP, they made considerable gains in Parliament. At the end of November 2016, in addition to gains in the Senate, their new presidential nominee avoided a second-round runoff by receiving more than 55% of the vote in a first-round field of 27 candidates. While each of these elections were heavily scrutinized and widely criticized, results have not been overturned nor has evidence of irregularities or abuses reached the magnitudes sufficient to refute the trends presented here.

<sup>3</sup>Of note is that the teams that completed the visits were not trained as auditors and the intent was not to “audit” the schools in the traditional sense. Rather, as mentioned, they were IT specialists who were tasked with collecting the relevant data for the website they were building. Furthermore, as they were operating independently of the education initiative, they did not have systematic access to the sort of data that would be necessary for targeted auditing.

Figure 3.1: Maps of Schools, the Public-Private Partnership Payments, and Audits of Participating Education Entrepreneurs



For all maps, the 20 largest cities of Haiti - which are also the only cities with more than 100,000 inhabitants, are indicated with small black circles. The capital, Port-au-Prince, is seen most clearly in the brightest red region of map (a). Longitude and latitude are provided on the perimeters and the key to the right of each of the three choropleth maps indicates the units of relative density. (a) Number of schools by arrondissement (admin2) as estimated from the Ministry of Education's records for the 2013-2014 6th grade national exams. Of particular note is that schools are, with the exception in the capital, relatively uniformly spread out throughout the arrondissements. (b) Variation in the grant funding provided to education entrepreneurs through the Public-Private Partnership (PPP) by arrondissement as reported by the National Bank of Haiti and collected for the Office of the Prime Minister. The number of schools per arrondissement as reported in map (a) provides the denominator for each calculation. Of particular note is that the relative intensity of PPP is not correlated with population centers. (c) The relative number of school audit visits completed in each commune (admin3) from fall of 2013 to winter of 2014 as reported in the database constructed for the website for the Office of the Prime Minister. Of note is that the audit teams visits were not correlated with the distribution of schools (a), the distribution of PPP (b) nor population centers as seen by the cities indicated. (d) The specific sections (admin4) visited by the audit team. By contrasting maps (c) and (d) one can see the variation used to test for spillovers as reported in Table 3.7.

combination of the above created an as good as random assignment of audit treatment on schools participating in the PPP, and in my empirical analysis, I provide evidence to further support this claim.

Over the next year-and-a-half, through this quasi-random implementation, 1,586 of the 10,839 schools in the PPP were audited (14.6%), and the corresponding information for these schools was subsequently made available on-line. About 10% of those audited were not found to exist, and eventually steps were taken by Haiti’s *l’Unité de lutte contre la corruption* (ULCC – Anticorruption Unit) against some – though not all – of these “ghost schools”. While an abrupt change in governments in January of 2015 unrelated to the education program ended the school audits, the information gathered by the teams has remained on-line for those schools that were audited.

## 3.4 Model

### Before the Program

The education sector in Haiti is highly competitive and unregulated. As a result, I model those who run these schools, the education entrepreneurs, as price-taking firms solving the classic profit maximizing problem in a competitive market. While the fact that only about about half of all school-aged-children in Haiti are enrolled in classes could be interpreted as indicating that there has simply not been sufficient entry into this market to meet the demand, the additional stylized facts that households in Haiti reportedly spend as much as half of their income on education and that educational scores in Haiti are particularly poor relative to other countries, indicate an alternative explanation that the private market is saturated and education entrepreneurs have competed to the point that the marginal school is offering the lowest quality level of education that is still worth receiving. That is to say that there is a fixed minimum level of education below which the cost of tuition combined with the opportunity cost of losing the additional laborer to the school for the day are greater than the present discounted value of the education received. As a result, I model this market as having a horizontal supply curve at a tuition,  $t$ , that is the minimum necessary for a marginal firm to provide this minimum acceptable quality of education.

More formally, before the introduction of the PPP, the entrepreneur effectively chooses the number of students to educate ( $N$ ) based upon the available facilities, staffing and other related costs ( $C(N)$ ) so to maximize profits ( $\pi_{private}$ ). The production cost function  $C(N)$  is modeled as upward sloping and convex in  $N$  so that  $\frac{\partial C(N)}{\partial N} > 0$  and  $\frac{\partial^2 C(N)}{\partial^2 N} > 0$ .

$$\pi_{private}(N) = \underbrace{N \cdot t}_{\text{(tuition revenue)}} - \underbrace{C(N)}_{\text{(production cost function)}} \quad (3.1)$$

Solving the first order condition with respect to  $N$  we have  $\frac{\partial \pi_{private}}{\partial N} = t - C'(N) = 0$  which implies  $t = C'(N)$ . In other words, the education entrepreneur will continue to accept students until tuition collected equals the marginal cost of educating one more child.

## Introduction of PPP

With the introduction of PPP, the government of Haiti offered every qualifying school the opportunity to forgo tuition in exchange for receiving a fixed transfer ( $\bar{g}$ ) per student enrolled. Schools were not to mix enrollment, which is to say that they were faced with either continuing in the private market or entering fully into the PPP and no longer collecting tuition or related school fees. I define a new term,  $\tilde{N}$ , as the number of students reported by the education entrepreneur to the government such that if the education entrepreneur responds honestly by becoming a ‘partner’ with the government then  $\tilde{N} = N$ . Formally then  $\tilde{N} \cdot \bar{g}$  is the total amount earned by the entrepreneur from the grant. So long as  $t = 0$  (the entrepreneur is not violating the rules by also collecting tuition), then the profit function of an ‘partner’ education entrepreneur is as follows:

$$\pi_{partner}(\tilde{N}; \bar{g}) = \underbrace{\tilde{N} \cdot \bar{g}}_{\text{(PPP grant revenue)}} - \underbrace{C(\tilde{N})}_{\text{(production cost function)}} \quad (3.2)$$

and in solving the first order conditions with respect to  $\tilde{N}$ , we have  $\bar{g} = C'(\tilde{N})$  which is to say that the entrepreneur will switch from the perfectly competitive private market to become a partner of the government if  $\bar{g} \geq t$ . If this is the case for at least some of the education entrepreneurs, then we would anticipate an increase in demand as the remaining 50% of children that are not enrolled in school who previously could not afford school at  $t$  can now enter classrooms.<sup>4</sup>

Note however that the new program affords participating education entrepreneurs much more discretion. In particular, an education entrepreneur could report  $\tilde{N} = N + l$ . To do so would carry with it some risk – particularly for those entrepreneurs who lacked political connections – and so to avoid this risk, the entrepreneur will have to pay an additional cost. I call this the ‘cost of cover-up’ ( $\tau(l, \phi)$ ) and model it as an increasing function of  $l$  and a decreasing function of political capital ( $\phi$ ). The result is the profit function of a entrepreneur aiming to capture this government program.

$$\pi_{capture}(\tilde{N}, l; \bar{g}, \phi) = \underbrace{\tilde{N} \cdot \bar{g}}_{\text{(PPP grant revenue)}} - \underbrace{C(\tilde{N} - l)}_{\text{(production cost function)}} - \underbrace{\tau(l, \phi)}_{\text{(cost of cover-up)}} \quad (3.3)$$

Solving the first order conditions we have

$$\frac{\partial \pi_{capture}}{\partial \tilde{N}} = \bar{g} - C'(\tilde{N} - l) = 0 \quad \text{and} \quad \frac{\partial \pi_{capture}}{\partial l} = C'(\tilde{N} - l) - \tau_1(l, \phi) = 0 \quad (3.4)$$

So for an education entrepreneur solving the capture problem, the optimal response would be to choose to declare students up to the point that the per student grant was exactly equal to the marginal cost of coverup.

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<sup>4</sup>A future extension of this model not explored in this chapter is the general equilibrium result.

In summary, with the rollout of the PPP, education entrepreneurs were faced with solving the following problem:

$$\pi_{PPP}(N, \tilde{N}, l; \bar{g}, \phi) = \max\{\pi_{private}, \pi_{partner}, \pi_{capture}\} \quad (3.5)$$

A trivial result of this simple model is that so long as the grant is greater than tuition, entrepreneurs will join the program. Furthermore, entrepreneurs will only opt to be partners (as opposed to captors) if their respective marginal cost of capture exceeds the marginal benefit from the grant.

## Program Audit

The audit visit makes salient any dishonesty in reporting, namely the magnitude of  $l$ , and as such, I model the audit as increasing the impact of  $l$  on the cost of cover-up. For simplicity here, the audit increases the way that  $l$  enters the cost of cover-up function by a factor of  $\alpha$  with  $\alpha > 0$ . So after the audit, the cost of cover-up for an entrepreneur is  $\tau(\alpha l, \phi)$ .

As  $\tau$  does not enter into the partner problem, the model predicts that an audit would have no effect on schools where the education entrepreneur is a partner.

By contrast, at schools where the education entrepreneur is a captor, the model predicts a drop in  $l$ . Furthermore, there will be a heterogeneity in response based on which of the following points is reached first: the marginal cost of coverup increases until it reaches the marginal benefit of the grant ( $\tau_1(\alpha l', \phi) = \bar{g}$ ) or the difference between the reported and actual number of students enrolled at the institution falls to zero ( $l' = 0$ ). On the one hand, for those capture entrepreneurs with at least the minimum level of political capital sufficient to mitigate concern about being discovered, formally that have  $\phi_c \equiv \{\phi : \tau_1(\alpha l'_c, \phi) \leq \bar{g} \text{ for some } l'_c > 0\}$ , then model predicts having reduced  $l_c$  to the new optimum  $l'_c^* > 0$  that the entrepreneur will continue in the program solving the capture problem. On the other, those capture entrepreneurs who lack sufficient political capital to guard against being discovered following the audit, namely  $\phi_u \equiv \{\phi : \tau_1(\alpha l'_u, \phi) > \bar{g} \forall l'_u > 0\}$ , the difference between reported and actual enrolled students will fall to zero,  $l'_u^* = 0$ , and the model predicts that if  $\bar{g} \geq t$ , the entrepreneur will solve the partner problem, and if  $\bar{g} < t$ , the entrepreneur will exit the program entirely and return to solving the private competitive equilibrium problem.<sup>5</sup>

## 3.5 Data

For this research I combine multiple data sets including several novel sources not previously shared outside the government of Haiti. Here I discuss each. It is important to note that the data environment in Haiti is particularly sparse, and even given the shortcomings mentioned below, these data are particularly valuable and credible.

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<sup>5</sup>This assumption of permanent exit is consistent with what is observed in the data as I do not see any education entrepreneurs re-enter following an exit. However, over a longer period of observation, this assumption may not hold.

For the initial analysis of the impact of the PPP on total enrollment and household welfare, I make use of USAID’s Demographic and Health Surveys (DHS). This is a representative, household-level data set with sampling across the entire country reaching approximately 10,000 households per survey. For this chapter I make use of the surveys from 1995, 2000, 2006, 2012 and 2016. Though independent and credible, these data have several notable limitations for my analysis. First, the amount of time between rounds creates opportunities for confounding factors to influence results. To mitigate this concern, my preferred specification uses administrative fixed effects to ensure that comparisons are limited to variation within administrative units. Second, the data collected in 1995 does not allow for mapping respondent households to administrative units lower than the first administrative level, Département, which limits my ability to make use of lower-level fixed effects when examining parallel trends. Third, the sampling methodology for all rounds is designed to be nationally representative. Generally the DHS reports at Département-level results as well, but cautions lower level interpretations. Furthermore, the GPS points linked to household clusters for the 2000, 2006, 2012 and 2016 surveys all have an added randomness in their measure so to conceal the identity of participating households. This means that even though I have used GIS to map these survey clusters to all four administrative levels, I restrict analysis to the second level of Arrondissement as it is the lowest level for which all units have data for all years and it is sufficiently aggregated so to guard against locating survey clusters within the wrong lower-level administrative unit.

To determine geographic schooling density, I make use of the Ministry of Education’s 6th Grade Exam Results. The PPP audit was the first attempt to fully map the locations of all schools across the country, but as it was discontinued before completion, the government still lacks a complete map of all schools. About 12,600 schools had at least one student sit for the 6th grade national exam in 2014. The exam is conducted by the Ministry of Education each summer to determine which students are eligible to begin middle school. The data set that I use here reports exam results by school including the pass rate, total number of students that registered for the exam, total that sat for the exam, and the commune where the school is located. I use this data to estimate school density using schools with students sitting for the 6th grade exam as a proxy for total number of schools in each administrative unit. It is important to note that these data likely under-represents the density of schools in more rural and poorer areas that would be less likely to have a student sitting for the 6th grade exam in any given year. Panel (a) of Figure 3.1 demonstrates that primary school density is, unsurprisingly, strongly correlated with population density. The darkest commune represents Port-au-Prince, the nation’s capital, and each of the darker communes corresponds to one of the provincial population centers which are represented by black circles.

For the primary analysis of this chapter, I make use of the registration and payments information for the PPP itself. This data set includes observations of the 10,839 registered schools and reports of approximately 60,000 transfers from the government of Haiti to the participating education entrepreneurs across 11 tranches representing nearly 4 complete school years. These data were collected and compiled by the IT firm hired by the Office of the Prime Minister in preparation to populate the website described earlier. The descriptive

data for each school was collected and digitized from the files of the Ministry of Education, and the financial transactions were collected directly from the National Bank of Haiti.

Finally, I employ the audit results from the Prime Minister’s office. This data reports on the 1,586 schools that were visited by the IT firm tasked with launching the website representing 15% of all PPP participating schools.

While the validity of data collected by government officials in low-income, low-capacity countries like Haiti is often merited, several characteristics of the data I bring together for this chapter mitigate these concerns and bolster their credibility. A particular strength of the PPP data is that, as described above, it was collected by two different sources and then compiled and matched by a third party. This dramatically increases the costs of intentionally manipulating the data. In particular, the key indicators for my analysis are the financial transactions reported by the National Bank, and this particular account was heavily scrutinized by both other government offices and the international community given the particularly high-profile nature of the PPP initiative. Concerns that one or more people at the National Bank might have sent payments to ghost accounts are mitigated by the fact that there are no transactions that fail to link to reported schools from the Ministry of Education. There are a few schools (< 1%) reported as part of the program by the Ministry of Education for which there is no transaction data, but this is too small a share to significantly bias the overall results. Still, the estimation is more precisely interpreted as being for those schools for which I have data. It is possible that additional payments were made through channels other than the National Bank, but such payments would be illicit, and since I do not observe them, they would actually bias my estimate of observed fraud downward from the actual level. There is the possibility that return payments were made from individual education entrepreneurs to the government officials who were instrumental in their registration with the initiative. This would be (part of) the cost of cover-up as discussed in Section 3.4 but would not affect my estimations as the resources would still be used for purposes other than their intent to educate students. It is important to note that prior to launch of the PPP, education entrepreneurs had limited interaction with the Ministry of Education. Specifically, there was no systematic collection of student enrollment data and no known prior incentive to falsely report such information. Finally, with respect to the Prime Minister’s audit data, most of the information I leverage was collected by the digital devices directly including time and date stamps as well as GPS coordinates collected as the team input other information. This mitigates data input errors and potential for manipulation. Furthermore, there is an additional layer of public accountability for this data as it is available with the GPS data easily accessible online through the government’s website and interactive map.



## 3.6 Empirical Strategy and Results

### Public-Private Partnership Impact

To begin I examine the available data on the effect of the roll-out of the PPP on overall primary school enrollment across Haiti. I leverage the series of DHS household surveys conducted in 1995, 2000 and 2006 before the PPP and again 2012 and 2016 after the PPP to estimate the effect. Figure 3.2 graphs the average percentage of primary school-aged children enrolled in school as reported in these years.

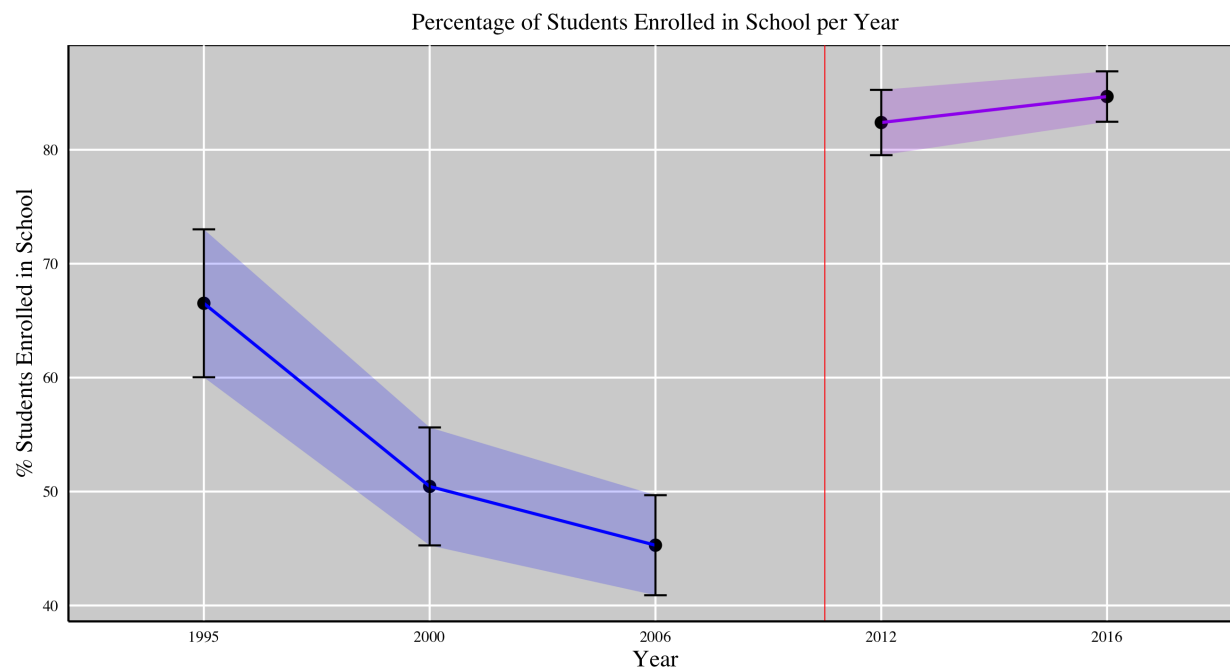
After falling more than 20 percentage points over the previous decade from 1995 to 2006, between 2006 and 2012 the enrollment rate increased more than 30 percentage points from 50% to 82%. Furthermore, the variance of enrollment across the departments strinks considerably with the standard errors of the final period (1.13) about 1/3 those of the first period (3.31).

While the period between 2006–2009 had been one of relative stability and economic growth for Haiti, a series of hurricanes and floods in 2009, the earthquake in 2010 and cholera epidemic in 2011 were collectively catastrophic for the country resulting in per capita income in 2012 falling below that of 2006. As a result, aggregate economic growth would not be a likely explanation for this change. Furthermore, (thankfully) it was the not case that so many children passed away during these disasters that those loses could explain such a sharp rise in enrollment percentages. The PPP was the only nation-wide program aiming to influence school enrollment during this period and one of the few positive national shocks in general during this period.

To provide more rigorous identification of the roll of PPP in this change, I follow [Duflo \(2001\)](#) and more recently [Ashraf et al. \(2016\)](#) in leveraging cohort differences to estimate impact. In particular, I rely on the fact that the level of exposure to the PPP is jointly determined by the administrative unit of residence and age of an individual child. As the maps in Figure 3.1 illustrate, (a) though the distribution of schools across the country does not vary considerably (with the notable exception of the national capital, Port-au-Prince), (b) there was significant variance in the intensity of treatment of the PPP across administrative units both in proportion to the number of schools as well as to the number of school-aged-children in an administrative unit. However, this variation was not widely publicized and evidence suggests that it was not known.

While being eligible to start primary school (i.e. being between the ages of 5-7) when the PPP was announced is predetermined, if families could strategically relocate in response to variation in program intensity, the administrative unit of residence would be endogenous. Such responses would have required both actionable information and freedom of movement, and neither could have been widely available. As already noted, the heterogeneity of implementation of the PPP was not widely known, if known at all. More importantly, payments for the first trimester were severely delayed so that families would have had to have responded several months prior to the transfers being made during a period of significant uncertainty especially for the education entrepreneurs. There was also a time constraint with respect to

Figure 3.2: Primary School Enrollment Rates



Enrollment rates calculated by departement (admin1) from the DHS surveys from 1995, 2000, 2006, 2012, and 2016. Note the downward trend in the first three periods followed by the dramatic increase in the fourth period with narrowing confidence intervals shown at the 95% level, and the improvements further increase and the distribution further narrows in the final period. The PPP (red line) began about 6 months before the 2012 DHS survey was collected meaning that its data includes the effects of the first two semesters of the PPP households while the 2016 data, collected primarily in 2015, includes the full program period.

freedom of movement as families would have had only about a month to relocate (or to send their child away to another administrative area) as the program was announced immediately prior to the start of the school year. Such relocation would have required financial means likely both beyond the reach of most families as well as in-excess of the expected financial return of such an action. Migration is in general quite constrained in Haiti due to the complexities and competing interests within its existing land tenure system which were further exacerbated in the aftermath of the 2010 earthquake. Finally, if there were strategic relocation, we would expect to see migration changes correlated with relative treatment intensity which we do not see between 2006 and 2012.

The identifying assumption of my analysis is that age and administrative unit of residence are predetermined and that there are neither time nor administrative unit-varying effects correlated with the PPP intensity which have been omitted. Under these assumptions, I use the following model to identify the causal impact on primary school enrollment arising from differences in relative PPP intensity. In particular, I compare the treatment, students just old enough to start primary school at the outset of the PPP (aged 5-7), against a control, students who are just beyond the threshold to have potentially benefited from the program

(aged 16-18)<sup>6</sup>, by running the following model:

$$Y_{ick} = \beta PPP_c * T_i + \delta \mathbf{X}_c * T_i + \gamma_c + \gamma_k + \epsilon_{ick} \quad (3.6)$$

Here the dependent variable is an indicator equal to unity if a primary school-aged individual  $i$  from administrative unit<sup>7</sup>  $c$  of age  $k$  is attending primary school as reported by the DHS surveys in 2012.  $PPP_c$  indicates the relative intensity of the PPP in administrative unit  $c$ , specifically in units of 1 school’s worth of funding per 1,000 students<sup>8</sup>. This is interacted with  $T_i$ , a cohort indicator that equals unity for those children in the survey who are 5-7 years old at the time of the survey. In some specifications I include various controls represented by the vector  $\mathbf{X}_c$  which are also interacted with the cohort indicator. These include both the primary school-aged population in the administrative unit in 2006 as well as the administrative unit’s 2006 primary school enrollment rate to control for any targeting effects from the program. Finally I include fixed effects for both the age of the individual and administrative unit of residence. The coefficient,  $\beta$ , is then the outcome of interest for this regression, and it can be interpreted, under the assumptions already discussed, as the causal effect of funding for one additional school per 1,000 students on the rate of primary school enrollment.

Table 3.1 reports the results, and even with all of the controls (columns 4 and 8 respectively by arrondissement and commune) there is a statistically significant increase in enrollment. Furthermore, across all specifications, both the point estimates and explanatory power of the regression are relatively consistent suggesting limited potential for significant bias from the unobservables. We can interpret from column 4 that each additional PPP-funded school per 1,000 students in a given area increased the probability of enrollment for a student just old enough to attend by about 1.1%.

The results at the commune level are about 1/3 as large and less statistically significant. This is likely due to the sampling methodology of the DHS which, as mentioned, samples for national representativeness. As a result, only about half of the total number of communes are represented and the sample itself at that level may not be representative.

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<sup>6</sup>DHS defines primary-school-aged children as those between 6-12 years of age. However, it is clear from the data that 6 is not a sharp threshold for starting school. Furthermore, given its history of low levels of enrollment and poor instruction, it is not uncommon for young teenagers to be several years behind where they would be if they were advancing at grade-level. 16 is generally accepted as the age at which a child is too old to sit for the 6th grade national exam. For these reasons, I chose the wider cohort for starting school of 5-7 years of age and use 16-18 as the control. Even those these cohorts are themselves not sharp, so long as being within one increases probability of the observed behavior, my model still holds.

<sup>7</sup>The level is specified in the regression tables, either arrondissement or commune for this cohort study.

<sup>8</sup>The conversion from grant payments in Haitian Gourdes to one PPP-funded school per 1,000 students is calculated from the total PPP grant dollars sent to a given administrative unit per trimester before the 2012 survey was conducted divided by 40 to convert from HTG to USD (the approximate exchange rate at the time) then divided by 30 to convert to a full student scholarship for one trimester, then divided by 95 which is the mean number of students per school per period conditional on the school being part of the program in a given period. This then is divided by the estimated total population per administrative unit to calculate schools per student and finally multiplied by 1,000 to calculate schools per 1,000 students. I choose this metric as it is in keeping with Duflo (2001) and Ashraf et al. (2016) though their dependent variable is total years of schooling for which Duflo (2001) finds that 1 additional school per 1,000 students leads to a 12-19% increase in years of education and a 1.5-2.7% increase in wages several decades after a school construction intervention in Indonesia.

Table 3.1: Public Private Partnership Impact on Primary School Enrollment

	<i>Dependent variable:</i>							
	Primary School Enrollment Rate By Arrondissement (Admin3)				Primary School Enrollment Rate By Commune (Admin4)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cohort x PPP Arron.	0.0131*** (0.0041)	0.0099** (0.0050)	0.0120*** (0.0039)	0.0108** (0.0049)				
Cohort x PPP Commune					0.0027 (0.0016)	0.0029* (0.0017)	0.0029 (0.0018)	0.0030* (0.0017)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adm. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort X '06 Adm. Prim. Pop.	No	Yes	No	Yes	No	Yes	No	Yes
Cohort X '06 Adm. Enroll.	No	No	Yes	Yes	No	No	Yes	Yes
Clustered SE Adm.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,805	8,805	8,805	8,805	8,805	8,173	8,173	8,173
R <sup>2</sup>	0.2934	0.2937	0.2938	0.2938	0.3093	0.3070	0.3066	0.3072
Adjusted R <sup>2</sup>	0.2897	0.2898	0.2899	0.2898	0.2994	0.2977	0.2973	0.2977

Estimation of Equation 3.6 with 2012 DHS data. The specifications are first run at the level of the arrondissement (admin2) and then the commune (admin3). The dependent variable is an indicator which equals unity if the individual is enrolled in primary school per the standard definitions and guidance of the DHS. Each estimation includes the respective administrative level fixed effects as well as age fixed effects and has a standard error clustered at the respective administrative level. In addition, some specifications control for the primary school enrollment rate in 2006 by administrative level as reported in the DHS survey of that year ('06 Adm. Enroll.). There are also controls for the 2006 population of primary-school-aged (6-12 years old) children by administrative level also as reported by DHS (Adm. Prim. Pop.). Both are interacted with a cohort indicator variable which equals unity if a household member was the age to begin primary school (5-7 years old) in the initial school year of the PPP. This indicator variable is also interacted with a continuous variable of the intensity of the PPP program per administrative unit. This is calculated by taking the total payments to education entrepreneurs by administrative level in the first two trimesters of the program and dividing it by the total population of primary-school-aged children in a given administrative area. The coefficient of interest is the term on this final interaction. The test compares the difference-in-differences between the 5-7 year olds who receive the PPP treatment and the 16-18 year olds who are just past the normal threshold of attending primary school and thus do not directly benefit from the intervention. \*p< 0.1; \*\*p< 0.05; \*\*\*p< 0.01

To test whether or not there were pre-existing trends which are being spuriously captured in the model, I run a placebo test on equation Equation 3.6. Specifically, I use household members 16-18 years of age as the placebo treatment against a new control of household members who are 20-22 years old. Table 3.2 reports the results. The coefficients and explanatory power are much reduced however the standard errors remain roughly the same magnitude meaning that none of the estimations are statistically significant. This is evidence that supports interpreting the results in Table 3.1 as the impact of the cohort effect during the PPP roll-out.

In addition to the increase in enrollment, as it is a form of indirect transfer, the PPP also has the potential to affect welfare for households even in the short-run. For households that had been previously paying tuition that now are able to send their children for free, the additional savings made would likely increase consumption and other welfare metrics. Similarly for households that were not able to send their children and were further burdened with the need to care for and watch truant children, the program would likely save on these (opportunity) costs. However, it may be the case that some families that previously had not been sending their children to school relied upon their children to help provide for the family for instance through work outside the home, as a substitute for adults on domestic work so to free them to pursue work, or potentially as a source of labor for household or on-farm income generating activities.

To test the potential impact on welfare, I again run Equation 3.6, this time with various welfare outcomes as the dependent variable. Given the brief nature of exposure and the relatively small amount of transfer available,<sup>9</sup> if there were welfare effects we would expect them to be relatively modest and concentrated in areas with relatively small units of consumption. The results provide evidence for this pattern of behavior with no changes in more durable household assets as evidenced by indicators of mobile phone ownership, household flooring material, roofing type, hectares of land owned, or ownership of large livestock like cattle and horses.<sup>10</sup> However, there are statistically significant changes in ownership of smaller household items like chickens. Table 3.3 presents some additional positive results, all with the full set of controls included. Column 1 reports the wealth index of the households, and though the results are statistically significant for both administrative levels in the less constrained specifications (not shown), even with the full specification there is a clear positive point estimate that remains, significant for the commune at the 0.10 level. Additional gains are seen in whether or not the household has a bank account, has gone to bed hungry in the past four weeks and whether or not the household had food on hand at the time of the interview. For each measure, positive coefficients indicate welfare improvements, and the interpretation continues to be the impact of the equivalent of funding one additional school through PPP per 1,000 school-aged-children in the administrative area. These findings show that even in

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<sup>9</sup>The upper bound on the direct value of PPP for a given family by the time of the 2012 DHS survey is 10 USD per month. This level would be achieved only if a family were to be directly benefiting from the program with a student in school, the government grant perfectly matched what they had been previously paying in tuition and the entirety of this value were passed through the education entrepreneur to the family.)

<sup>10</sup>These results are not presented here in the interest of brevity but are available upon request.

Table 3.2: Public Private Partnership Placebo Test

	<i>Dependent variable:</i>							
	By Arrondissement (Admin3)				By Commune (Admin4)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cohort x PPP Arron.	-0.0013 (0.0042)	-0.000005 (0.0060)	0.0024 (0.0051)	-0.0029 (0.0052)				
Cohort x PPP Commune					0.0011 (0.0012)	0.0012 (0.0012)	0.0011 (0.0012)	0.0011 (0.0012)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adm. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort X '06 Adm. Prim. Pop.	No	Yes	No	Yes	No	Yes	No	Yes
Cohort X '06 Adm. Enroll.	No	No	Yes	Yes	No	No	Yes	Yes
Clustered SE Adm.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,831	7,831	7,831	7,831	7,831	7,295	7,295	7,295
R <sup>2</sup>	0.1508	0.1508	0.1522	0.1528	0.1685	0.1625	0.1629	0.1630
Adjusted R <sup>2</sup>	0.1458	0.1456	0.1470	0.1474	0.1551	0.1499	0.1504	0.1502

Estimation of a placebo effect of Equation 3.6 with 2012 DHS data. The specifications are first run at the level of the arrondissement (admin2) and then the commune (admin3). The dependent variable is an indicator which equals unity if the individual is enrolled in primary school per the definitions of the DHS. Each estimation includes the respective administrative level fixed effects as well as age fixed effects and has a standard error clustered at the respective administrative level. In addition, some specifications control for ('06 Adm. Enroll.) the primary school enrollment rate in 2006 by administrative level. There are also controls for the 2006 population of primary- school-aged (6-12 years old) children by administrative level. Both are interacted with a cohort indicator equaling unity if a household member is just past the age for primary school (16-18 yrs old) in the initial school year of the PPP. This indicator variable is also interacted with a continuous variable of the intensity of the PPP program per administrative unit calculated from the total payments to education entrepreneurs by administrative level in the first two trimesters of the program and dividing by the total population of primary- school-aged children in a given administrative area. The coefficient of interest is the term on this final interaction. The placebo test compares the difference-in-differences between the 16-18 year olds and a placebo control of the 20-22 year olds in the sample. Neither of whom should have received direct benefit from the PPP and thus the anticipated effect is zero if our the results shown in Table 3.1 are driven by variation in the PPP. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 3.3: Public Private Partnership Impact on Welfare

	<i>Dependent variable:</i>							
	Wealth Index (1)	Has a Bank Account (2)	Has a Bank Account (3)	Go to Bed Account (4)	Go to Bed Hungry (5)	No Food in Home (6)	No Food in Home (7)	No Food in Home (8)
Cohort x PPP Arron.	0.0900 (0.0749)		0.0082*** (0.0028)	0.0020** (0.0009)	0.0090** (0.0040)	0.0020* (0.0012)	0.0099* (0.0055)	0.0029*** (0.0011)
Cohort x PPP Commune		0.0236* (0.0131)						
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adm. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort X '06 Adm. Prim. Pop.	No	Yes	No	Yes	No	Yes	No	Yes
Cohort X '06 Adm. Enroll.	No	No	Yes	Yes	No	No	Yes	Yes
Clustered SE Adm.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,173	8,173	8,173	8,173	8,173	8,173	8,173	8,173
R <sup>2</sup>	0.3669	0.4211	0.0786	0.0960	0.0301	0.0575	0.0339	0.0600
Adjusted R <sup>2</sup>	0.3630	0.4132	0.0729	0.0837	0.0241	0.0446	0.0279	0.0472

Estimation of Equation 3.6 with 2012 DHS data. For each dependent variable the specification is run at the level of the arrondissement (admin2) and then the commune (admin3). The dependent variables are as follows: ‘Wealth Index’: continuous scale from 1-5. ‘Has a Bank Account’: indicator that equals unity when a household reports having bank account. ‘Go to Bed Hungry’ and ‘No Food in Home’: discrete variables with 1 = Often, 2 = Sometimes, and 3 = never. So positive coefficients for all dependent variables indicate welfare improvements. Each estimation includes the respective administrative level fixed effects as well as age fixed effects and has a standard error clustered at the respective administrative level. Each specification controls for ‘06 Adm. Enroll.’ the primary school enrollment rate in 2006 by administrative level and the 2006 population of primary-school-aged (6-12 yrs old) children by administrative level. Both are interacted with a cohort indicator which equals unity if a household member was the age to begin primary school (5-7 yrs old) in the initial school year of the PPP. This indicator variable is also interacted with a continuous variable of the intensity of the PPP program per administrative unit. This is calculated by taking the total payments to education entrepreneurs by administrative level in the first two trimesters of the program and dividing it by the total population of primary-school-aged children in a given administrative area. The coefficient of interest is the term on this final interaction. The test compares the difference-in-differences between the 5-7 yr olds who receive the PPP treatment and the 16-18 yr olds who are just past the normal threshold of attending primary school. \*p< 0.1; \*\*p< 0.05; \*\*\*p< 0.01

just the first six months of the initiative, in addition to the increase in school enrollment, the PPP also had positive impacts on household welfare.

## Audit Impact: Difference-in-Difference

The positive impact for households resulting from PPP mitigates concern of complete capture by education entrepreneurs. However, as the program lacked mechanisms for accountability at the time of launch, the potential for significant abuse to occur was high. The method by which the audits were implemented provide a natural experiment to shed light on the level of abuse in the system.

To to estimate the impact of the audits on abuse, I restrict observations to only those schools within communes that the audit team visited. Specifically, out of 136 communes, the team visited at least one section in 36 of them. The resulting sample includes 5,740 schools in 183 sections. The treatment schools are those 1,586 that the team attempted to visit and the primary control group is then the 4,154 schools within the audited communes that were not audited.

A key assumption for this identification strategy is that the control schools do not differ systematically from the treatment schools and therefore their behavior in the absence of audit provides an unbiased counter-factual for how the audited entrepreneurs would have behaved had they not been audited. Because the audit was not fully randomized, this is assumption is nontrivial. To provide supporting evidence to this claim, I test for parallel trends in the trimesters prior to audit. That is to say, I test whether the two groups vary with respect to my outcomes of interest in any systematic or significant way prior to the audit by running:

$$Y_{ict} = \beta_{ct}AUDIT_i * \gamma_{ct} + \epsilon_{ict} \quad (3.7)$$

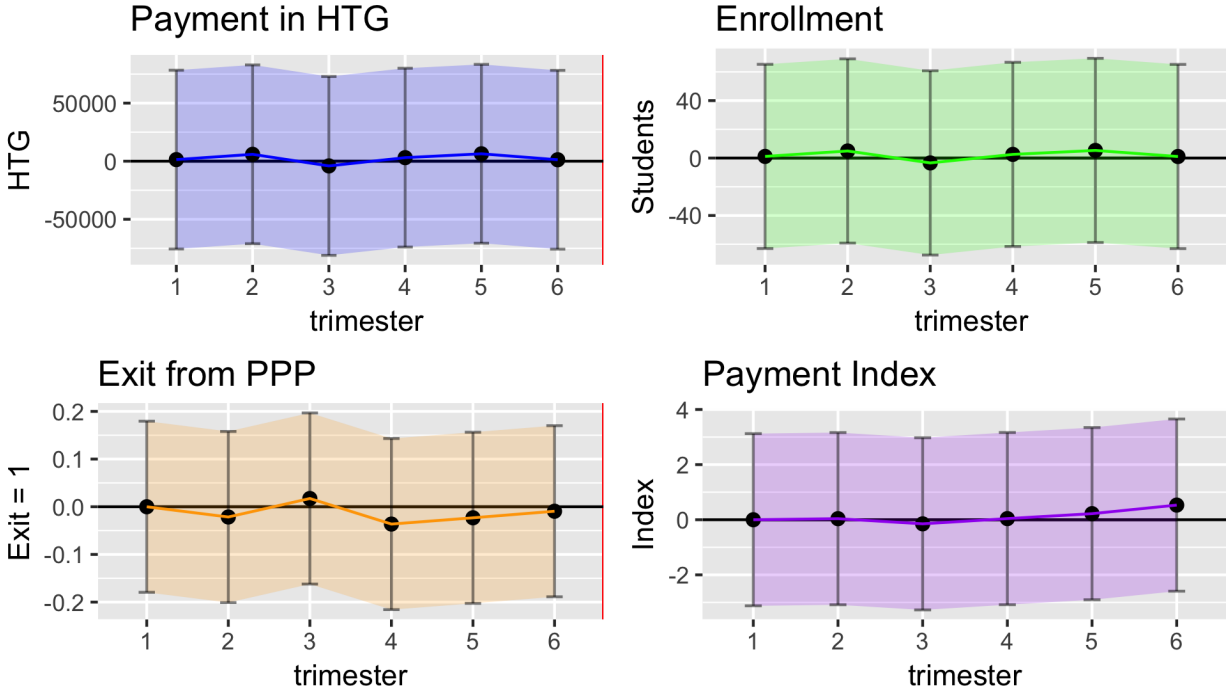
Where  $i$  indicates individual schools (or the behavior of an individual entrepreneur),  $c$  indicates the administrative unit within which the school falls (the level of administrative unit is either commune or section depending on the specification), and  $t$  is the indicator of time, in this case the first six trimesters of the education initiative before the audit began. The dependent variables are alternatively the amount, in Haitian Gourdes<sup>11</sup>, of the grant payment made by the government of Haiti to the School, the number of reported students enrolled in the program in the school, a binary variable indicating whether or not the school exited the program in a given trimester, or a Payment Index calculated using the payment a school received in the first trimester as the denominator and the payment in trimester  $t$  as the numerator<sup>12</sup>.  $AUDIT_i$  is a binary variable equal to 1 for those schools that are audited,  $\gamma_{ct}$  is a trimester-by-commune fixed effect, and  $\epsilon_{ct}$  is error term clustered at the school. This tests against the null hypothesis that during the period prior to the audit, those schools that

<sup>11</sup>At the time of this study the exchange rate was approximately 40 HTG to 1 USD.

<sup>12</sup>When the Payment Index is the outcome of interest, I thus further restrict analysis to those 4,305 schools (82% of the total sample) that were part of the program from the beginning.



Figure 3.3: Testing for Parallel Trends Before School Audits Begin by Commune



The above is a graphical summary of the results from running Equation 3.7 at the level of the Commune and then taking the mean of the estimated coefficients and standard errors. Grant payments to schools participating in the Public-Private Partnership (PPP) are reported in Haitian Gourdes (approx. 40 HTG/USD). Enrollment is the number students funded by the grant at a participating school. Exit is an indicator variable that is equal to 1 if a school exits the program in the period following the audit. The Payment Index uses the payment received in the first trimester as the denominator and as such only includes schools that were part of the program from the beginning. The standard errors for all estimations are clustered at the level of the school and 95% confidence intervals are shaded. The individual commune level estimates consistently demonstrate similar patterns of point estimates near zero throughout the pre-audit period providing evidence of consistent parallel trends between the control and treatment groups.

would eventually be audited were not different in their participation in the PPP from those in the same commune who would not be audited during the full period of observation.

The results are graphically summarized in Figure 3.3 by taking the mean of the point estimates and standard error terms over the whole of those estimated for each commune in each trimester. I find that in each trimester the point estimate is nearly zero and that zero always within the 95% confidence intervals. Therefore I cannot reject the null.<sup>13</sup> While it is the case that the confidence intervals are much tighter for most of the individual commune estimations, it remains important to note that this is not a precisely estimated zero. This is one of the reasons that in the following section I also conduct an event study.

Turning to my primary specification, I test the difference-in-difference model:

<sup>13</sup>I have run this test estimated at all four administrative levels by trimester and also with the trimesters aggregated to complete school years. All tests have similar results and though the variance further increases with the level of aggregation, they continue to indicate that one cannot reject the null hypothesis. Regression tables for the above as well as both tables and graphs for those mentioned here are available upon request.

$$Y_{ict} = \beta AUDIT_{it} + \gamma_{ct} + \eta_i + \epsilon_{ict} \quad (3.8)$$

With  $i$ ,  $c$ ,  $t$ , and  $Y_{ict}$  as before,  $AUDIT_{it}$  as the school-by-trimester indicator variable set to unity starting in the trimester in which a given school is audited until the end.  $\gamma_{ct}$  is again a trimester-by-commune fixed effect,  $\eta_i$  is the school specific fixed effect and  $\epsilon_{ict}$  is the error term clustered at the school level.

The results of this estimation are reported in Table 3.4. For each of the dependent variables of interest, the model is estimated from variation within commune-by-trimester as well as at the even more restricted level of section-by-trimester. Across all outcomes of interest and both specifications, we see entrepreneurs changing their behavior following the audit in the direction expected. Columns 1 and 3 estimated with commune-by-trimester fixed effects report the most sizable change by way of a reduction in claimed enrolled students and subsequent grant payments of nearly 9,000 HTG or 675 USD per school per year. These results are significant at the 0.01 level.

Even taking the more conservative estimates in columns 2 and 4 from within section variation, we still see that audited schools respond by claiming fewer students, 6.7 fewer on average, and as result receive 8,011 HTG (200 USD) less per trimester or 600 USD less per school year than those that are not audited. This result is significant at the 0.05 level and also represents an economically significant loss of income of 19% for the schools as can be seen in column 8. Furthermore I observe an increase of about 7.6% in number of schools that exit the initiative following the audit. These results are all in keeping with the intuition provided by the model in Section 3.4.

A potential threat to the validity of these results would be if they are driven by a few particularly large schools whose responses happen to be correlated with the audit visits thus inducing spurious effects. The significant reductions across all outcomes of interest at the section level suggests that the impacts are not likely driven by outliers. To further investigate heterogeneity across school size, I divide the sample into quintiles based on the grant payment from the first trimester of the program. The calculated thresholds are at 55,350; 103,320; 143,910; and 166,050 HTG respectively. These thresholds represent approximately 1,344; 2,583; 3,598; 4,151 USD or schools ranging in size from less than 45 students in the first quintile to those with more than 140 in the largest. For comparison, the mean per-trimester enrollment across all schools conditional on being part of the program during a given trimester is 95 with a median of 77 students.

Heterogeneity analysis rejects concerns that results are being driven by large outliers. Two examples can be found in Tables 3.5 and 3.6 which respectively report the results from columns 6 and 8 of Table 3.4 disaggregated by quintiles<sup>14</sup>. Table 3.5 shows that audited schools across the distribution increased exit at statistically significant rates. The middle cohort demonstrates the maximum relative increase at 11 percentage points with smaller differences in the upper end of the distribution and the fourth cohort being the least precisely

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<sup>14</sup>I have run all of the columns from Table 3.4 in this way and report these two as illustrative. The whole set is available upon request

Table 3.4: Audit Effects on PPP Schools

	<i>Dependent variable:</i>							
	Grant Payments HTG		Enrollment		Exit = 1		Payment Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Audit Treatment	-8,954*** (2,236)	-8,011** (3,213)	-7.5*** (1.9)	-6.7** (2.7)	0.068*** (0.007)	0.076*** (0.010)	-0.039 (0.131)	-0.189** (0.082)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Commune X Tri. FE	Yes	No	Yes	No	Yes	No	Yes	No
Section X Tri. FE	No	Yes	No	Yes	No	Yes	No	Yes
Custered SE School	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	57,145	57,145	57,145	57,145	57,145	57,145	47,355	47,355
R <sup>2</sup>	0.510	0.524	0.510	0.524	0.200	0.229	0.431	0.494
Adjusted R <sup>2</sup>	0.457	0.458	0.457	0.458	0.114	0.121	0.369	0.421

Estimation of Equation 3.8. For each pair of estimations per dependent variable, the left is with trimester-by-commune (at admin level 3) fixed effects and the right estimation is with trimester-by-section (admin level 4) fixed effects. There are 11 trimesters in the data set, and audits begin in the seventh and continue to the final trimester. All estimations include school fixed effects and an indicator variable that equals unity from the trimester the school is audited. Grant payments to schools participating in the Public Private Parntership (PPP) are reported in Haitian Gourdes (approx. 40 HTG/USD). Enrollment is the number students funded by the grant at a participating school. Exit is an indicator variable that is equal to 1 if a school exits the program in the period following the audit. The Payment Index uses the payment received in the first trimester as the denominator and as such only includes schools that were part of the program from the beginning. The standard errors for all estimations are clustered at the level of the school. \*p < 0, 1; \*\*p < 0, 05; \*\*\*p < 0, 01

Table 3.5: Audit Effects on PPP School Exit by Quintile

	<i>Dependent variable:</i>				
	Exit = 1 by Quintiles				
	(1)	(2)	(3)	(4)	(5)
Audit Treatment	0.098*** (0.029)	0.087*** (0.025)	0.110*** (0.025)	0.066* (0.034)	0.047*** (0.017)
School FE	Yes	Yes	Yes	Yes	Yes
Section X Tri. FE	Yes	Yes	Yes	Yes	Yes
Custered SE School	Yes	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.307	0.303	0.310	0.370	0.265
Adjusted R <sup>2</sup>	0.143	0.141	0.165	0.167	0.129

Estimation of Equation 3.8 by quintiles. Each estimation includes trimester-by-section (admin level 4) fixed effects for the 11 trimesters in the data set (audits begin in the seventh and continue to the final trimester), school fixed effects and an indicator variable that equals unity from the trimester the school is audited. Each column represents a quintile of the distribution of schools based on the payment received in the first trimester of the program with column 1 estimating the lowest quintile and increasing successively to column five with the highest quintile. Thresholds are at 55,350; 103,320; 143,910; and 166,050 Haitian Gourdes (approx. 40 HTG/USD). Note that these estimations only include schools that were part of the program from the beginning. Exit is an indicator variable that is equal to 1 if a school in the program in the period following the audit. The standard errors are clustered at the level of the school.

\*p < 0.1; \*\*p < 0.05; \*\*\*p < 0.01

estimated but still significant at the 10% level. The results in Table 3.6 provide further evidence that results are being driven by responses from the lower and middle portions of the distribution as only the first three quintiles respond significantly at the 10% level or better while the fourth cohort has a positive though noisy point estimate. The largest relative change in terms of point estimate is in the first quintile with a 57 percentage point reduction in price index. However its standard errors are sufficiently large that the I cannot reject that the middle cohort may well have a larger true effect. Taken together these tables provide additional evidence that the estimated effects from Table 3.5 are not driven by orthogonal responses by a few large outliers. They are instead consistent with results driven by responses across the distribution, and in particular by significant changes in the behavior of education entrepreneurs in the middle-to-lower portions of the distribution.

It may also be the case that there are positive spillovers from these sorts of interventions that would multiply the effectiveness of a given audit. To test this I again estimate equation

Table 3.6: Audit Effects on PPP School Payment Index by Quintile

	<i>Dependent variable:</i>				
	Payment Index by Quintiles				
	(1)	(2)	(3)	(4)	(5)
Audit Treatment	-0.569*	-0.274*	-0.290**	0.105	-0.004
	(0.340)	(0.142)	(0.137)	(0.183)	(0.084)
School FE	Yes	Yes	Yes	Yes	Yes
Section X Tri. FE	Yes	Yes	Yes	Yes	Yes
Custered SE School	Yes	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.625	0.564	0.588	0.561	0.582
Adjusted R <sup>2</sup>	0.516	0.463	0.501	0.421	0.504

Estimation of Equation 3.8 by quintiles. Each estimation includes trimester-by-section (admin level 4) fixed effects for the 11 trimesters in the data set (audits begin in the seventh and continue to the final trimester), school fixed effects and an indicator variable that equals unity from the trimester the school is audited. Each column represents a quintile of the distribution of schools based on the payment received in the first trimester of the program with column 1 estimating the lowest quintile and increasing successively to column five with the highest quintile. Thresholds are at 55,350; 103,320; 143,910; and 166,050 Haitian Gourdes (approx. 40 HTG/USD). The Payment Index also uses the payment received in the first trimester as the denominator and as such only includes schools that were part of the program from the beginning. The standard errors are clustered at the level of the school. \*p < 0.1; \*\*p < 0.05; \*\*\*p < 0.01

3.8, but this time use the section as the unit of treatment and the schools in the other sections that are not audited within the same commune as the control group by changing the fixed effects to commune-by-trimester and replacing the school fixed effect with one for section. If there are positive spillovers, they would be expected among those schools within close proximity to the audited schools. However, as the results reported in Table 3.7 indicate, we instead find evidence against spillover effects. All point estimates are of the opposite sign than before and the standard errors are such that we cannot rule out a zero effect in all cases. Of note, for Enrollment and Grant Payments, I cannot rule out a negative result of the same magnitude as the point estimate of my preferred specification.

### Audit Impact: Event Study

As mentioned in the previous section when discussing the test for parallel trends, the results of that test were not precisely estimated zeros. Given that the audit was not perfectly

Table 3.7: Audit Effects on Administrative Sections

	<i>Dependent variable:</i>			
	Grant Payments HTG	Enrollment	Exit = 1	Payment Index
	(1)	(2)	(3)	(4)
Audit Treatment	1,430 (3,654)	1.2 (3.0)	-0.005 (0.008)	0.071 (0.078)
Section FE	Yes	Yes	Yes	Yes
Commune X Tri. FE	Yes	Yes	Yes	Yes
Clustered SE Section	Yes	Yes	Yes	Yes
Observations	57,145	57,145	57,145	47,355
R <sup>2</sup>	0.221	0.221	0.175	0.126
Adjusted R <sup>2</sup>	0.214	0.214	0.167	0.117

Estimation of Equation 3.8. Each estimation includes trimester-by-commune (at admin level 3) fixed effects, section (admin level 4) fixed effects, and an indicator variable that equals unity from the trimester the first school is audited within the section. There are 11 trimesters in the data set, and audits begin in the seventh and continue to the final trimester. Grant payments to schools participating in the Public Private Partnership (PPP) are reported in Haitian Gourdes (approx. 40 HTG/USD). Enrollment is the number students funded by the grant at a participating school. Exit is an indicator variable that is equal to 1 if a school exits the program in the period following the audit. The Payment Index uses the payment received in the first trimester as the denominator and as such only includes schools that were part of the program from the beginning. The standard errors for all estimations are clustered at the level of the section.

\*p < 0.1; \*\*p < 0.05; \*\*\*p < 0.01

random, there may be selection issues that are biasing the results so I also run an alternative test for the impact of audits on the behavior of education entrepreneurs with an event study. With this specification instead of testing audited schools against a subset of schools that never receive an audit, I leverage the panel data and variation in audit dates to use the schools not yet audited that I know from the data will be audited in later periods as a control for those that are audited in the current period. Therefore I further restrict my sample to only those 1,586 schools which are audited at some point during the audit process and estimate:

$$Y_{ict} = \sum_{j=-8}^5 \beta_j * \mathbf{I}(TrimestersSinceAudit_{it}) + \gamma_{ct} + \eta_i + \epsilon_{ict} \quad (3.9)$$

Where  $Y_{ict}$ ,  $\gamma_{ct}$ ,  $\eta_i$ , and  $\epsilon_{ict}$  are as before. In addition I calculate  $I(\cdot)$ , an indicator function, and  $TrimestersSinceAudit_{it}$  which counts the trimesters,  $t$ , since school  $i$  was audited with negative values for trimesters prior to audit, zero during the audit itself and positive after the audit. When estimating this equation, I set the trimester of audit as the reference factor to be excluded so to avoid perfect co-linearity.

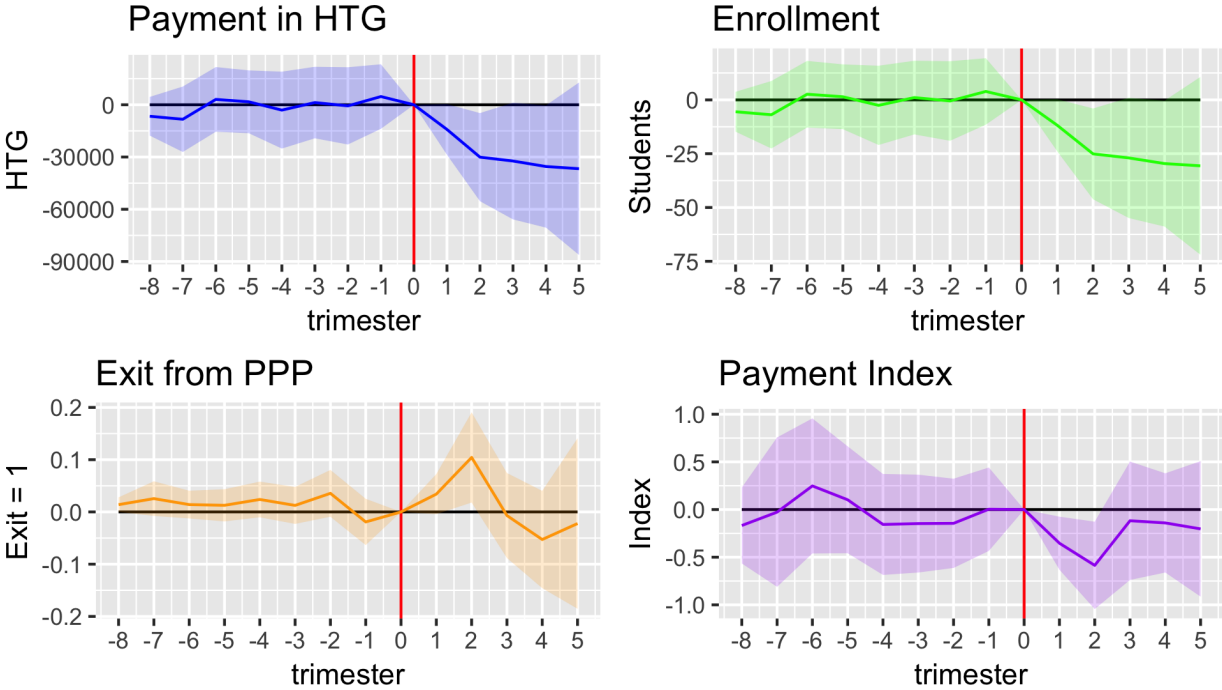
I provide the complete regression results in Table 3.8 and the graphical depictions in Figure 3.4. All four dependent variables trend with varying degrees of imprecision around zero for the trimesters leading up to audit. This is evident from the graphs which show relatively horizontal lines with confidence intervals overlapping zero throughout the pre-

Table 3.8: Event Study of Audit Impact on PPP Schools

	<i>Dependent variable:</i>			
	Grant Payments HTG (1)	Enrollment (2)	Exit = 1 (3)	Payment Index (4)
-8 Trimesters	(0.000)	(0.000)	(0.000)	(0.000)
-7 Trimesters	-6,598 (4,312)	-5.5 (3.6)	0.014** (0.005)	-0.168 (0.155)
-6 Trimesters	-8,322 (7,295)	-6.9 (6.1)	0.025** (0.013)	-0.027 (0.304)
-5 Trimester	3,086 (7,203)	2.6 (6.0)	0.014 (0.010)	0.248 (0.275)
-4 Trimesters	1,699 (7,006)	1.4 (5.8)	0.013 (0.012)	0.102 (0.218)
-3 Trimesters	-3,072 (8,573)	-2.5 (7.1)	0.024* (0.013)	-0.156 (0.206)
-2 Trimesters	1,277 (7,970)	1.1 (6.6)	0.012 (0.014)	-0.147 (0.199)
-1 Trimester	-636 (8,610)	-0.5 (7.2)	0.036** (0.017)	-0.145 (0.182)
Audit Trimester	4,721 (7,192)	3.9 (6.0)	-0.019 (0.017)	0.004 (0.170)
+1 Trimester	-14,168** (5,646)	-11.8** (4.7)	0.034** (0.015)	-0.353*** (0.108)
+2 Trimesters	-30,073*** (9,824)	-25.1*** (8.2)	0.104*** (0.034)	-0.586*** (0.178)
+3 Trimesters	-32,297** (13,013)	-27.0** (10.8)	-0.007 (0.032)	-0.118 (0.241)
+4 Trimesters	-35,454*** (13,631)	-29.6*** (11.4)	-0.053 (0.036)	-0.140 (0.202)
+5 Trimesters	-36,713* (19,229)	-30.6* (16.0)	-0.022 (0.063)	-0.203 (0.276)
School FE	Yes	Yes	Yes	Yes
Section X Tri. FE	Yes	Yes	Yes	Yes
Custered SE School	Yes	Yes	Yes	Yes
Observations	11,451	11,451	11,451	10,087
R <sup>2</sup>	0.551	0.551	0.342	0.537
Adjusted R <sup>2</sup>	0.465	0.465	0.215	0.446

Estimation of Equation 3.9. Each estimation includes trimester-by-section (at admin level 4) fixed effects, school fixed effects, and the set of event study indicator variables indicating trimesters since the audit (is negative until the trimester of audit). Grant payments to schools participating in the Public Private Partnership (PPP) are reported in Haitian Gourdes (approx. 40 HTG/USD). Enrollment is the number students funded by the grant at a participating school. Exit is an indicator variable that is equal to 1 if a school exits the program in the period following the audit. The Payment Index uses the payment received in the first trimester as the denominator and as such only includes schools that were part of the program from the beginning. The standard errors for all estimations are clustered at the level of the school. \*p< 0.1; \*\*p< 0.05; \*\*\*p< 0.01

Figure 3.4: Event Study of Audit Impact on Schools



The above is a graphical summary of the results from running Equation 3.9 with zero as the excluded reference period also representing the trimester when a given school was audited. Grant payments to schools participating in the Public-Private Partnership (PPP) are reported in Haitian Gourdes (approx. 40 HTG/USD). Enrollment is the number students funded by the grant at a participating school. Exit is an indicator variable that is equal to 1 if a school exits the program in the period following the audit. The Payment Index uses the payment received in the first trimester as the denominator and as such only includes schools that were part of the program from the beginning. The standard errors for all estimations are clustered at the level of the school and 95% confidence intervals are shaded. All four dependent variables demonstrate trends near zero prior to the trimester of audit, and all show statistically significant diversion in the predicted directions following the audit for at least two period with Payment and Enrollment demonstrating a persistent level change as well. Full regression results provided in Table 3.8.

period and in the table with point estimates alternating on either side of zero with standard errors that are often larger than the estimate.

Following the trimester of audit, there is a sharp change as Payments, Enrollment and Payment Index all drop and Exit increases – all statistically significantly so for the first two observed trimesters. For Payments and Enrollment, the magnitude of change for the first period after audit is nearly double that reported in Table 3.4 and eventually reaches more than three times the estimated change from Table 3.4. Furthermore, these decreases persistent through the end of the period of observation and though the precision of the estimates decreases as well these findings suggest that the impact may result in a level



change. For Exit after the second period where difference observed reaches 10 percentage points which is about a quarter again greater than the 7.6 percentage points from Table 3.4. This affect appears to end as would be expected from the model in Section 3.4. Regarding the payment index, after the second period where the reduction peaks at nearly 60 percentage points, the impact subsides, but this may be due in part to the increased exit among the middle and lower portions of the distribution as observed in Table 3.6. As noted, statistical power attenuates as the data approaches the end of the period of observation which partially explains the broadening confidence intervals for all measures.

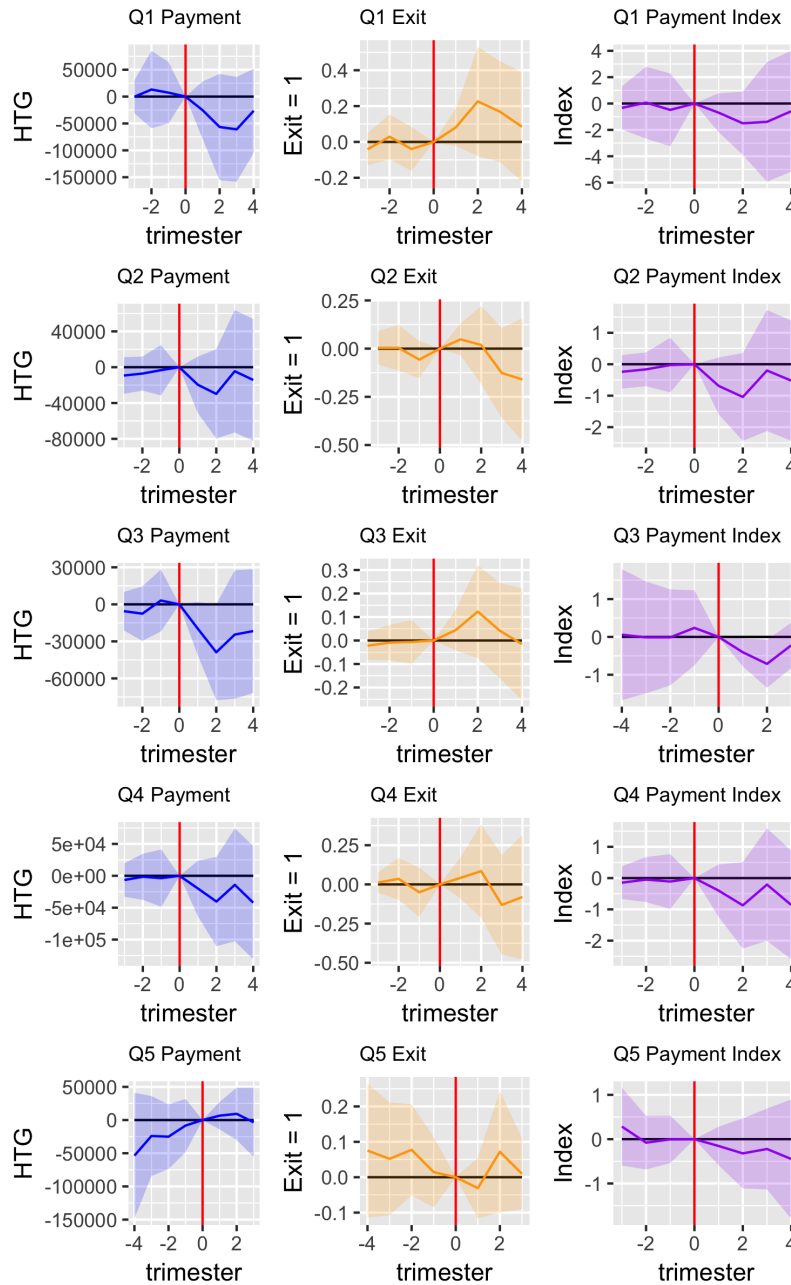
To provide further evidence of the effect of audits, I again divide the schools by quintiles following the same methodology as before and estimate Equation 3.9 for each subset. In the interest of brevity, I report here only the graphical results which can be found in Figure 3.5. Again all four dependent variables demonstrate trends near zero prior to the trimester of audit. The impact is again most significant in the first three quintiles and the point estimates for nearly all panels are in the direction of the aggregate results reported in Figure 3.9. However, these estimates are even less precisely estimated with only the middle quintile exhibiting any results that can be distinguished from zero with 95% confidence - specifically the Payment and more clearly the Payment Index. These results further mitigate concerns that aggregate results are driven by outliers on the high end of the distribution as the fifth quintile has a negative point estimate for exit indicating that the audit may actually cause the entrepreneurs of the largest schools to increase their probability of staying in the program. However, as the standard errors are quite wide, the evidence for such an interpretation is only suggestive.

## 3.7 Discussion: Costs and Benefits

Taken together, these results suggest that education entrepreneurs respond in a way consistent with the intuition of the model in section 3.4 by reducing fraudulent claims and exiting when partnership or capture under reduced fraud is no longer the profit maximizing solution. The estimated savings of 600 USD per school per year when multiplied across just the 1,586 and accounting for a 8% reduction in participating schools, represents an observed annual savings of 875,472 USD. The total expenditures for the full audit were less than US\$80,000 much of which was the one-time fixed costs of survey tool development, procurement, and web design. Therefore the lower bound for the return on investment of the audit effort is the annual savings divided by audit costs or about 10.94 USD return for every dollar spent on the audit. This assumes the level changes are not persistent, there are no economies of scale or savings from the existing investment, and it ignores any value that was generated by setting up the website and other related services provided by the IT team.

Assuming our audited sample is representative of all education entrepreneurs in the PPP, this implies auditing the entire population, even without spillovers, would conservatively

Figure 3.5: Event Study of Audit Impact on Schools by Quintiles



The above is a graphical summary of the results by quintiles from running Equation 3.9 with zero as the excluded reference period also representing the trimester when a given school was audited. Quintiles are calculated from the distribution payment received in the first trimester and as such estimates only include schools that were part of the program from the beginning. Thresholds are at 55,350; 103,320; 143,910; and 166,050 Haitian Gourdes (approx. 40 HTG/USD). Grant payments to schools participating in the Public-Private Partnership (PPP) are also reported in Haitian Gourdes. Exit is an indicator variable that is equal to 1 if a school exits the program in the period following the audit. The Payment Index uses the payment received in the first trimester as the denominator. The standard errors for all estimations are clustered at the level of the school and 95% confidence intervals are shaded.

result in a net savings of 5.4 million USD<sup>15</sup>. If the changes are persistent, this means that the government of Haiti lost about 11.4 million USD<sup>16</sup> in funds to capture over the first two years of PPP without audit or about 14% of transfers during that period.

### 3.8 Conclusion

Governance in the severely constrained settings faced in least developed countries is particularly challenging and requires different approaches to what are otherwise standard challenges. One particular solution increasingly pursued is the hybrid institution of the Public-Private Partnership (PPP). This analysis of Haiti’s attempt to provide universal primary education through a PPP with education entrepreneurs finds that positive results are possible. This initiative coincided with a 30 percentage point increase in primary school enrollment rates. Through a cohort study I identify causal evidence that each additional PPP-funded school per 1,000 students in a given area leads to an increase in the probability of enrollment for students just old enough to benefit from the first year of the program by 1-1.3%. I also identify further evidence that even within the first year of the program there are observable welfare benefits to families in terms of improved consumption and small household asset accumulation.

Management of PPP’s is itself a significant challenge for already-stretched governments in poor countries. This chapter identifies the impact of a low-cost accountability mechanism introduced by the government of Haiti that resulted in significant savings for the education initiative as a whole. Specifically, audits by IT consultants hired by the Prime Minister’s office to generate a web-based clearinghouse of education information led to an economically and statistically significant drop in reported student enrollment in the program and corresponding grant payments to the education entrepreneurs as well as increased exit from the program entirely. These changes are interpreted as reductions in fraudulent claims consistent with a model of education entrepreneurs re-optimizing their profit-maximizing responses.

Within the available data, I observe the government of Haiti spending more than US 170 million USD on the PPP. At the time, this represented nearly 5% of the total government budget. Given the 875,000 USD in direct savings per year observed from this sample, the government of Haiti may have been able to save 11 million USD over the first two years if an accountability mechanism like this effort had been rolled out in parallel with the launch of the PPP. With a return on investment of at least 11 USD for every dollar spent, the evidence suggests that a simple audit regime is a significantly cost-effective measure and it is likely that relatively simple extensions combined with more explicit consequences could increase the return and potentially induce positive spillovers.

$$^{15} \left( \underbrace{\left[ 10,839 \text{ schools} \times (1 - .08) \right] \times 600 \text{ USD}}_{\text{Gross Savings (per year)}} \right) - \left( \underbrace{10,839 \text{ schools} \times \frac{80,000 \text{ USD for audit}}{1,586 \text{ audited schools}}}_{\text{Audit Costs (potentially one-time)}} \right)$$

$$^{16} (2 \times \text{Gross Savings}) - \text{Audit Cost}$$

While the literature has shown that conditional cash transfer programs in emerging countries like Brazil and Mexico can provide substantial benefits to the population, such initiatives require a level of administrative sophistication and complexity that may be beyond the capacity of some LDCs. As such, different solutions may be required to enable governments in these countries to leverage their limited resources to best support their people. I have presented evidence that Haiti's PPP approach to education is a candidate for such a program. The PPP provided similar educational and welfare benefits as more established conditional cash transfer programs in other settings while being implementable within the constraints facing the government of Haiti. Furthermore, I have shown that additional accountability measures can help guard against capture of such efforts. The next step for research in this area is to more precisely measure the relative benefits of this form of indirect transfer and compare it to other more direct social transfer programs.

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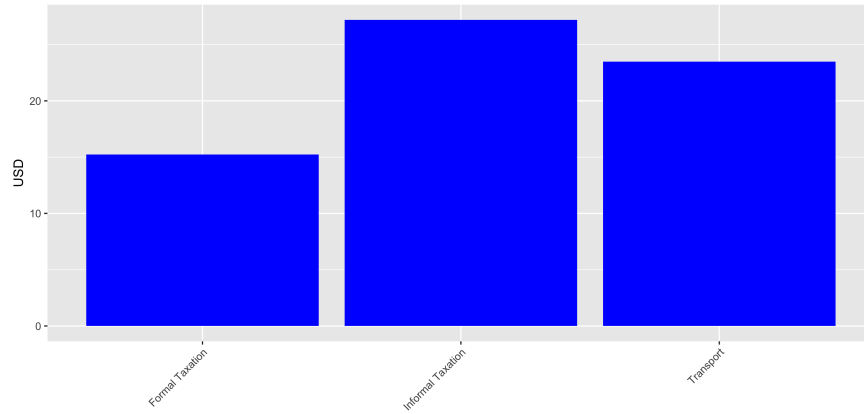


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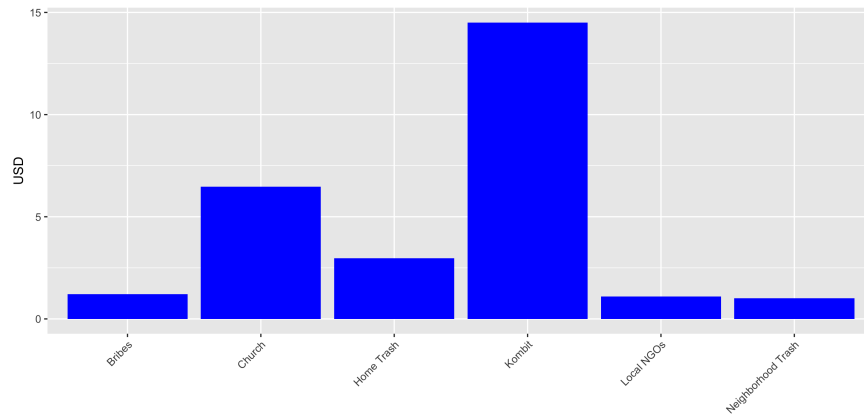
## A.1 Additional Figures

Figure A1: Formal vs Informal Taxation



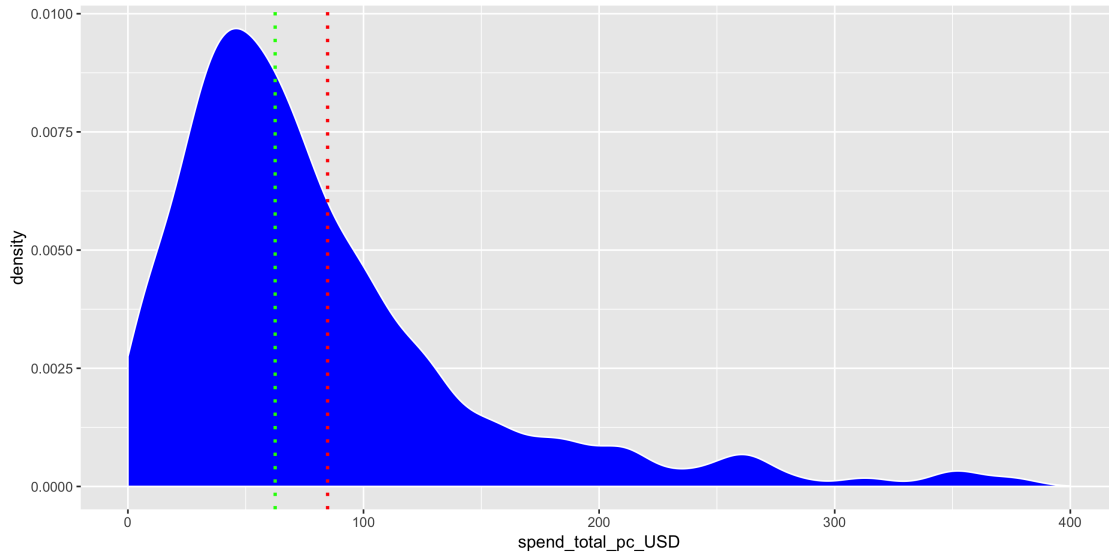
Contributions at baseline to formal and informal taxation with transport expenditures also provided as a benchmark. Baseline self-reported measures from 1,500 representative households. All amounts in USD per month.

Figure A2: Composition of Informal Taxation



Composition of contributions to informal taxation. “Kombit” is the combination of all community-based collective action activities. Value of in-kind contributions estimated by survey respondents. Value of labor contributions calculated by multiplying time worked by  $\frac{1}{2}$  national minimum wage. Baseline self-reported measures from 1,500 representative households. All amounts in USD per month.

Figure A3: Monthly per capita Expenditures



Self-reported household monthly per capita expenditures in USD as reported by the head of household in aggregate and then divided by the number of household members. Green dotted line is the median and the red dotted line is the mean.

Figure A4: Universe of Properties by Bloc

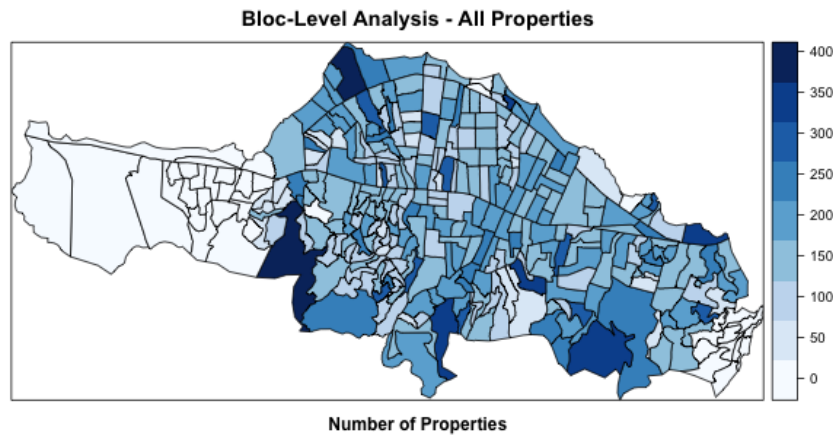
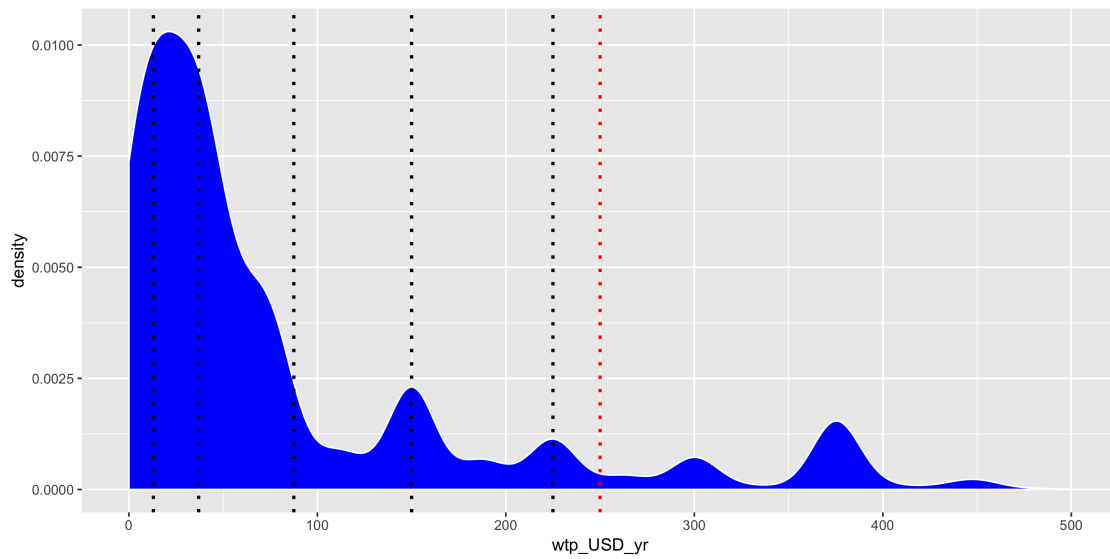


Figure A5: Willingness to Pay for Garbage Removal Services



The results of an incentive compatible Becker–DeGroot–Marschak willingness to pay solicitation for garbage removal services. X-axis indicates values in terms of USD. Dotted lines indicate the equivalent amounts for each of the various steps in the property tax schedule.

## A.2 Supplemental Tables

Table A.1: Effects of Tax Collection, Public Goods, and their Interaction on  $\log(\text{Total Paid} + 1)$  and Percent of Tax Bill in FY 2019

	Tax Payments Various Measures FY19			
	$\log(\text{Total Paid} + 1)$		Percent of Tax Bill	
	(1)	(2)	(3)	(4)
Tax Collection	-0.089** (0.043)	-0.070 (0.052)	-0.014 (0.010)	-0.008 (0.012)
Public Goods	0.001 (0.032)	0.047 (0.047)	0.006 (0.007)	0.007 (0.009)
PUBLIC X TAX		-0.051 (0.086)		-0.013 (0.020)
Observations	40,060	40,060	39,124	39,124
Adjusted R <sup>2</sup>	0.265	0.265	0.125	0.125

“ $\log(\text{Total Paid} + 1)$ ” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG), then multiplying by an exchange rate of 65HTG:1USD, adding one (1) and then taking the  $\log()$  of the result. “Percent of Tax Bill Paid” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then dividing it by the total tax bill. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the “tax collection” treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.2: Effects of Tax Collection and Public Goods on Indicator and Count Measures for FY 2019 and FY 2019-20

	Tax Payments Various Measures			
	Indicator FY19 (1)	Count FY19 (2)	Indicator FY19-20 (3)	Count FY19-20 (4)
Tax Collection	-0.013*** (0.004)	-0.016*** (0.005)	-0.015*** (0.004)	-0.024*** (0.007)
Public Goods	-0.001 (0.004)	0.002 (0.005)	0.0003 (0.004)	0.004 (0.007)
Mean	0.12	0.14	0.14	0.21
TAX coef / Mean	-0.11	-0.12	-0.11	-0.12
PUBLIC coef / Mean	0	0.02	0	0.02
Observations	40,060	40,060	40,060	40,060
Adjusted R <sup>2</sup>	0.269	0.208	0.297	0.264

“Indicator” equals 1 if the property paid any amount of taxes in the period indicated and 0 otherwise. “Count” sums the total number of unique times the property owner is recorded as making a payment toward the total tax bill in the specified year. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \*p< 0.1; \*\*p< 0.05; \*\*\*p< 0.01

Table A.3: Effects of Tax Collection, Public Goods, and Placebo on Amount of Taxes Paid

	Amount of Tax Paid in USD		
	FY19	FY19-FY20	FY19-FY20
	(1)	(2)	(3)
Tax Collection Bloc	3.53* (2.09)	5.80 (3.88)	1.90 (10.38)
Public Goods	1.65 (1.27)	2.60 (2.29)	2.65 (2.24)
Placebo			10.15 (21.26)
Mean	13.1	21.86	21.86
TAX coef / Mean	0.27	0.27	0.09
PUBLIC coef / Mean	0.13	0.12	0.12
Drop Treated with Tax Collection	Yes	Yes	Yes
Observations	22,214	22,214	22,214
Adjusted R <sup>2</sup>	0.04	0.05	0.05

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. “Placebo” is an indicator variable equal to 1 if a property was randomly assigned to receive the placebo tax collection treatment (see A.3). All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$



Table A.4: Effects of Tax Collection, Public Goods, and their Interaction on Various Measures of Tax Compliance

	Tax Payments			
	Total Paid FY19 (1)	log(Total Paid + 1) FY19 (2)	Total Paid FY19-20 (3)	log(Total Paid + 1) FY19-20 (4)
Tax Collection	-8.09* (4.68)	-0.03 (0.03)	-9.21 (7.71)	-0.01 (0.04)
Public Goods	1.69 (1.53)	0.02 (0.02)	3.30 (2.64)	0.02 (0.03)
Tax Collection X Public Goods	3.93 (5.72)	-0.05 (0.05)	-0.42 (10.13)	-0.08 (0.06)
Mean	13.1	0.51	21.86	0.64
$\beta$ / Mean	-0.62	-0.05	-0.42	-0.01
$\delta$ / Mean	0.13	0.05	0.15	0.04
$\mu$ / Mean	0.06	0.01	0.06	0.01
Individual Covariates	Yes	Yes	Yes	Yes
Bloc Covariates	Yes	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes
Clustered SE Bloc	Yes	Yes	Yes	Yes
Observations	40,060	40,060	40,060	40,060
Adjusted R <sup>2</sup>	0.02	0.26	0.03	0.29

“Total Paid” is a continuous variable calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG), then multiplying by an exchange rate of 65HTG:1USD. “log(Total Paid + 1)” is calculated by adding one (1) to “Total Paid” and then taking the log() of the result. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.5: Effects of Tax Collection, Public Goods, and their Interaction on Making any Tax Payments and Number of Tax Payments

	Tax Payments			
	Indicator FY19	Count FY19	Indicator FY19-20	Count FY19-20
	(1)	(2)	(3)	(4)
Tax Collection	0.0003 (0.006)	-0.002 (0.009)	0.004 (0.008)	0.010 (0.013)
Public Goods	0.004 (0.005)	0.005 (0.006)	0.004 (0.006)	0.010 (0.010)
Tax Collection X Public Goods	-0.014 (0.010)	-0.010 (0.014)	-0.019* (0.011)	-0.032 (0.020)
Mean	0.12	0.14	0.14	0.21
$\beta$ / Mean	0	-0.01	0.03	0.05
$\delta$ / Mean	0.04	0.04	0.03	0.05
$\mu$ / Mean	0.01	0.01	0.01	0.01
Individual Covariates	Yes	Yes	Yes	Yes
Bloc Covariates	Yes	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes
Clustered SE Bloc	Yes	Yes	Yes	Yes
Observations	40,060	40,060	40,060	40,060
Adjusted R <sup>2</sup>	0.269	0.208	0.297	0.264

“Indicator” equals 1 if the property paid any amount of taxes in the period indicated and 0 otherwise. “Count” sums the total number of unique times the property owner is recorded as making a payment toward the total tax bill in the specified year. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.6: Testing for Spillovers of Public Goods on Amount of Taxes Paid by Property by Dropping Border Properties

	Amount of Tax Paid in USD			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-5.99* (3.32)	-8.87* (4.87)	-9.31* (5.27)	-11.14 (7.97)
Public Goods	3.83** (1.80)	1.05 (1.54)	4.00 (2.51)	2.42 (2.49)
PUBLIC X TAX		6.51 (5.91)		4.12 (10.34)
Mean	12.94	12.94	21.53	21.53
TAX coef / Mean	-0.46	-0.69	-0.43	-0.52
PUBLIC coef / Mean	0.3	0.08	0.19	0.11
INTERACTION coef / Mean		0.5		0.19
F-stat		0.49		0.24
Drop 'Contaminated'	Yes	Yes	Yes	Yes
Observations	37,779	37,779	37,779	37,779
Adjusted R <sup>2</sup>	0.02	0.02	0.03	0.03

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.7: Testing for Spillovers of Public Goods on Amount of Taxes Paid by Property by Dropping ‘Contaminated’ Properties”

	Amount of Tax Paid in USD			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-3.91 (3.26)	-6.16 (4.23)	-4.07 (4.52)	-6.69 (6.33)
Public Goods	5.32** (2.53)	2.03 (1.88)	5.21* (3.16)	3.25 (2.73)
PUBLIC X TAX		5.55 (4.47)		6.10 (6.84)
Mean	12.82	12.82	21.22	21.22
Drop all boundary properties	Yes	Yes	Yes	Yes
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.03	0.03	0.05	0.05

“Amount of Tax Paid in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.8: Effects of Tax Collection, Public Goods, and their Interaction on  $\log(1 + \text{Amount of Tax Paid in USD by Property})$ 

	$\log(1 + \text{Amount of Tax Paid in USD})$			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-0.085*** (0.027)	-0.066* (0.036)	-0.083*** (0.032)	-0.061 (0.045)
Public Goods	0.020 (0.020)	0.056* (0.029)	0.026 (0.025)	0.061* (0.036)
PUBLIC X TAX		-0.048 (0.055)		-0.056 (0.064)
Linear Combination		-0.058 (0.037)		-0.056 (0.04)
Drop all boundary properties	Yes	Yes	Yes	Yes
F-Stat	268.5	216.37	297.04	245.63
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.257	0.258	0.293	0.293

“ $\log(1 + \text{Amount of Tax Paid in USD})$ ” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG), then multiplying by an exchange rate of 65HTG:1USD, adding one (1) and then taking the  $\log()$  of the result. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{it}$  indicator which control for additional randomly assigned variations in implementation of the “tax collection” treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.9: Effects of Tax Collection, Public Goods, and their Interaction on Change in Amount of Tax Paid from FY 2018 in USD by Property

	Change in Amount of Tax Paid from FY 2018 in USD			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-3.49 (5.46)	-2.92 (7.42)	-7.41 (7.33)	-9.09 (9.20)
Public Goods	6.77** (2.89)	2.48 (1.89)	12.10** (5.15)	4.52 (3.54)
PUBLIC X TAX		-0.40 (6.10)		5.16 (8.24)
Linear Combination		-0.83 (4.17)		0.59 (6.82)
Drop all boundary properties	Yes	Yes	Yes	Yes
Mean	1.2	1.2	14.02	14.02
F-Stat	15.88	16.31	6.52	5.61
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.03	0.03	0.003	0.003

“Change in Amount of Tax Paid from FY 2018 in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then subtracting from that the sum of all property tax payments for a given property made in FY2018, and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.10: Effects of Tax Collection, Public Goods, and their Interaction on  $\log(1 + \text{Change in Amount of Tax Paid from FY 2018 by Property})$ 

	$\log(1 + \text{Change in Amount of Tax Paid from FY 2018})$			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-0.085*** (0.024)	-0.068** (0.032)	-0.090*** (0.029)	-0.078** (0.040)
Public Goods	0.015 (0.017)	0.036 (0.026)	0.018 (0.022)	0.051 (0.031)
PUBLIC X TAX		-0.041 (0.048)		-0.032 (0.058)
Linear Combination		-0.07 (0.03)		-0.06 (0.04)
Drop all boundary properties	Yes	Yes	Yes	Yes
F-Stat	100.882	78.894	451.28	364.44
Observations	26,099	26,099	26,339	26,339
Adjusted R <sup>2</sup>	0.074	0.074	0.322	0.322

“ $\log(1 + \text{Change in Amount of Tax Paid from FY 2018})$ ” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then subtracting from that the sum of all property tax payments for a given property made in FY2018, adding one (1), and then taking the  $\log()$  of the result. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.11: Effects of Tax Collection, Public Goods, and their Interaction on Change in Amount of Tax Paid from Average of Previous Three Years in USD by Property

	Change in Amount of Tax Paid from Average of Previous Three Years			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-6.79 (6.97)	-8.35 (8.69)	-10.70 (8.73)	-14.51 (10.84)
Public Goods	7.56** (3.28)	2.05 (2.00)	12.89** (5.40)	4.09 (3.57)
PUBLIC X TAX		4.49 (5.49)		10.05 (8.40)
Linear Combination		-1.81 (5.22)		-0.38 (7.44)
Drop all boundary properties	Yes	Yes	Yes	Yes
Mean	1.9	1.9	14.71	14.71
F-Stat	7.32	7.98	7.89	6.5
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.05	0.05	0.01	0.01

“Change in Amount of Tax Paid from Average of Previous Three Years in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then subtracting from that quantity of the sum of all property tax payments for a given property made for each of the previous three fiscal years prior to the start of the project (FY2016, FY2017 and FY2018) divided by 3, and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$



Table A.12: Effects of Tax Collection, Public Goods, and their Interaction on  $\log(1 + \text{Change in Amount of Tax Paid from Average of Previous Three Years in USD by Property})$ 

	$\log(1 + \text{Change in Amount of Tax Paid from Average of Previous Three Years})$			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-0.078*** (0.024)	-0.048 (0.032)	-0.094*** (0.026)	-0.075** (0.036)
Public Goods	0.021 (0.017)	0.045* (0.026)	0.015 (0.019)	0.034 (0.028)
PUBLIC X TAX		-0.070 (0.048)		-0.046 (0.051)
Linear Combination		-0.073 (0.034)		-0.087 (0.037)
Drop all boundary properties	Yes	Yes	Yes	Yes
F-Stat	897.64	700.59	4446.7	3479.24
Observations	24,443	24,443	25,311	25,311
Adjusted R <sup>2</sup>	0.358	0.358	0.545	0.545

“Change in Amount of Tax Paid from Average of Previous Three Years in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then subtracting from that quantity of the sum of all property tax payments for a given property made for each of the previous three fiscal years prior to the start of the project (FY2016, FY2017 and FY2018) divided by 3, adding one (1), and then taking the  $\log()$  of the result. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.13: Effects of Tax Collection, Public Goods, and their Interaction on Paying Tax Bill in Full

	Binary Indicator Paid All of the Tax Bill			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-0.014* (0.007)	-0.005 (0.009)	-0.009 (0.006)	-0.002 (0.007)
Public Goods	0.004 (0.004)	0.013* (0.007)	0.004 (0.003)	0.008* (0.005)
PUBLIC X TAX		-0.023 (0.014)		-0.016 (0.012)
Linear Combination		-0.014 (0.011)		-0.01 (0.01)
Drop all boundary properties	Yes	Yes	Yes	Yes
Mean	0.09	0.09	0.05	0.05
F-Stat	173.31	135.48	104.73	82.56
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.175	0.175	0.122	0.122

“Binary Indicator Paid All of the Tax Bill” is a binary indicator calculated by first summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then subtracting the total tax bill. The indicator variable equals 1 if the difference is greater than or equal to zero and otherwise the indicator equals to zero. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{it}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.14: Effects of Tax Collection, Public Goods, and their Interaction on Percent of Tax Bill Paid

	Percent of Tax Bill Paid			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-0.027** (0.012)	-0.012 (0.013)	-0.014 (0.009)	-0.001 (0.011)
Public Goods	0.012 (0.008)	0.019 (0.012)	0.007 (0.007)	0.013 (0.009)
PUBLIC X TAX		-0.034 (0.023)		-0.030* (0.017)
Linear Combination		-0.027 (0.017)		-0.017 (0.013)
Drop all boundary properties	Yes	Yes	Yes	Yes
Mean	0.16	0.16	0.13	0.13
F-Stat	185.67	149.65	199.06	163.61
Observations	27,090	27,090	27,216	27,216
Adjusted R <sup>2</sup>	0.124	0.124	0.180	0.180

“Percent of Tax Bill Paid” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG) and then dividing it by the total tax bill. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the “tax collection” treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.15: Effects of Tax Collection, Public Goods, and their Interaction on Number of Tax Installments Made by Property

	Number of Tax Installments Made			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-0.015* (0.008)	-0.008 (0.010)	-0.016 (0.012)	-0.005 (0.015)
Public Goods	0.005 (0.005)	0.013 (0.008)	0.006 (0.008)	0.016 (0.012)
PUBLIC X TAX		-0.017 (0.016)		-0.026 (0.023)
Linear Combination		-0.012 (0.011)		-0.015 (0.016)
Drop all boundary properties	Yes	Yes	Yes	Yes
Mean	0.13	0.13	0.2	0.2
F-Stat	272.96	218.13	261.7	208.41
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.207	0.208	0.262	0.262

“Number of Tax Installments Made” is a count variable. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.16: Effects of Tax Collection, Public Goods, and their Interaction on Average Amount Per Tax Installment in USD by Property

	Average Amount Per Tax Installment in USD			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-5.27** (2.37)	-7.52** (3.83)	-4.92* (2.69)	-7.25 (4.42)
Public Goods	2.55** (1.23)	1.27 (1.24)	2.17 (1.42)	1.79 (1.44)
PUBLIC X TAX		5.16 (4.08)		5.13 (4.73)
Linear Combination		-1.09 (1.79)		-0.33 (2.02)
Drop all boundary properties	Yes	Yes	Yes	Yes
Mean	10.71	10.71	13.48	13.48
F-Stat	47.07	37.29	48.19	41.92
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.05	0.05	0.06	0.06

“Average Amount Per Tax Installment in USD” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG), then dividing it by the number of installments made during that period, and then multiplying by an exchange rate of 65HTG:1USD. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the “tax collection” treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.17: Effects of Tax Collection, Public Goods, and their Interaction on  $\log(1 + \text{Average Amount Per Tax Installment in USD by Property})$ 

	$\log(1 + \text{Average Amount Per Tax Installment in USD})$			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-0.084*** (0.026)	-0.067* (0.036)	-0.080*** (0.030)	-0.061 (0.042)
Public Goods	0.019 (0.020)	0.055* (0.028)	0.025 (0.023)	0.058* (0.033)
PUBLIC X TAX		-0.045 (0.053)		-0.050 (0.060)
Linear Combination		-0.057 (0.036)		-0.052 (0.036)
Drop all boundary properties	Yes	Yes	Yes	Yes
F-Stat	273.05	220.49	309.82	257.97
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.263	0.264	0.290	0.290

“ $\log(1 + \text{Average Amount Per Tax Installment in USD})$ ” is calculated by summing all property tax payments for a given property made within the specified payment period in Haitian Gourdes (HTG), then dividing it by the number of installments made during that period, then multiplying by an exchange rate of 65HTG:1USD, adding one (1) and then taking the  $\log()$  of the result. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Table A.18: Effect of Tax Collection, Public Goods, and their Interaction on Binary Indicator for Any Tax Payment by Property

	Binary Indicator for Any Taxes Paid			
	FY19		FY19-FY20	
	(1)	(2)	(3)	(4)
Tax Collection	-0.0133** (0.0060)	-0.0081 (0.0079)	-0.0129* (0.0066)	-0.0069 (0.0090)
Public Goods	0.0018 (0.0044)	0.0109* (0.0065)	0.0035 (0.0050)	0.0115 (0.0076)
PUBLIC X TAX		-0.0131 (0.0118)		-0.0147 (0.0130)
Linear Combination		-0.0103 (0.0081)		-0.01 (0.0083)
Drop all boundary properties	Yes	Yes	Yes	Yes
Mean	0.12	0.12	0.14	0.14
F-Stat	339.83	274.29	389.24	318.48
Observations	27,773	27,773	27,773	27,773
Adjusted R <sup>2</sup>	0.2709	0.2710	0.2969	0.2970

“Binary Indicator for Any Taxes Paid” equals 1 if the property paid any amount of taxes in the period indicated and 0 otherwise. “FY19” is the first full fiscal year of the experiment running from October 1, 2018 - September 30, 2019. “FY19-20” includes the 18 months of administrative data collected during the period October 1, 2018 - March 30, 2020. “Tax Collection” is an indicator variable equal to 1 if a property is randomly assigned to have a tax agent from the mayor’s office hand-deliver an invoice for property taxes and equal to zero otherwise. “Public Goods” is an indicator variable equal to 1 if a property is located in a census bloc randomly assigned to receive garbage removal services and equal to zero otherwise. Analysis is limited to only properties within census blocs that were eligible to receive garbage removal services and thus were part of the bloc-level randomization. All regressions include the  $FRAME_{ib}$  indicator which control for additional randomly assigned variations in implementation of the ‘tax collection’ treatment beyond the simple delivery and explanation. All regressions also include individual and bloc controls, strata fixed effects, and standard errors clustered at the bloc-level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

## A.3 Treatment Invoices


### Summary of Tax Invoice Frames

- Pro-social Incentives:
  - **Obligation:** (commonly referenced proverbs) *Remember, “Many hands make light work,” and, “You can’t eat soup with just one finger.”*
  - **Nationalism:** (opening lines of the national anthem) *For the country, For the ancestors / Let us march united. Let us march united. / Let there be no traitors in our ranks! Let us be masters of our soil. / Let us march united. Let us march united.*
- Individualistic Incentives:
  - **Benefits:** *Remember that Property Taxes are the principal source of financing for public goods: garbage removal and street cleaning. Your contribution will make these services possible for you and your community.*
  - **Penalties:** *Property taxes are due at the end of the month of APRIL! Know that for each month of delayed payment, your total tax bill increases by 5%.*
- Anti-social Punishment
  - **Social Exposure:** *If you pay your property taxes in the next 30 days, the Mayor will place a sign indicating payment on the exterior wall of your house facing the street.*
- Informational
  - **Government Capacity (Placebo):** *Modeled off of the tax invoice, this letter carries as much personal information about the property and taxpayer as possible without explicitly referencing taxes. The actual substance of the letter then is merely a reminder of bank holidays in the city.*
  - **Spillover:** *No government official was sent to these households.*




## Invoice

LIBERTÉ                      EGALITÉ                      FRATERNITÉ



**REPUBLIQUE D'HAÏTI**  
**Mairie de CARREFOUR**

BORDEREAU CFPB (IMPÔT LOCATIF) 2018-2019  
À PAYER À LA DGI AVANT LE : 30 Avril 2019



IDENTIFICATION	CONTRIBUABLE	PROPRIETAIRE	IMMEUBLE	Année Fiscale
NOM ET PRÉNOM	[REDACTED]	[REDACTED]	[REDACTED]	2018-2019
ADRESSE	[REDACTED]		DATE D'EMISSION	1 Octobre 2018
			DATE D'ÉCHÉANCE	30 Avril 2019
TELEPHONE	[REDACTED]	[REDACTED]	DISTRICT :542	
MATRICULE	[REDACTED]	[REDACTED]	BLOC :202	

VALEUR LOCATIVE APPLIQUÉE : 131,250.0      LOYER DÉCLARÉ 75,000.00      TAUX : 8.00%

## INFORMATION SUR LES CONTRIBUTIONS À PAYER

CONTRIBUTIONS COMMUNALES		CONTRIBUTIONS TRESOR PUBLIC	
Montant principal	10,500.00	Taxe additionnelle (10%)	1,050.00
		DSAV (2/1000)	21.00
			0.00
<b>Total contribution communale</b>	<b>10,500.00</b>	<b>Total contribution Trésor Public</b>	<b>1,071.00</b>

CONTRIBUTION TOTALE À PAYER EN GOURDES : 11,571.00

AUTRES EXERCICES A PAYER : 2010-2011, 2011-2012, 2012-2013, 2013-2014, 2014-2015, 2015-2016, 2016-2017, 2017-2018

IMPORTANT : La mairie de Carrefour invite les contribuables de la commune de Carrefour à verser le montant mentionné ci dessus, sans addition des surtaxes de retard, à condition que le paiement se fasse avant le 30 Avril 2019. Passé ce délai, le centre des impôts de Carrefour (CDI) calculera le montant des surtaxes à raison de 5% par mois de retard et exigera leur paiement. Tel 22260726

Gagnez du temps et de l'effort! Avec bordereau en main, veuillez-vous présenter directement à la DGI pour faire le paiement. La DGI est située entre Thor 14 et Cote Plage 16 entre 9h00 AM et 4h00 PM du Lundi au Vendredi.



\_\_\_\_\_  
Directeur des affaires Administratives et Financiere

TYPE :A







## Government Capacity (Placebo)



IDENTIFICATION	CONTRIBUABLE	PROPRIETAIRE
NOM ET PRÉNOM	[REDACTED]	[REDACTED]
ADRESSE	[REDACTED]	
TELEPHONE	[REDACTED]	[REDACTED]
MATRICULE	[REDACTED]	[REDACTED]

Jours Fériés	2019	2020
Jour de l'Indépendance / Nouvel An	Lundi 1er janvier	Mecredi 1er janvier
Jour des Aïeux	Mercredi 2 janvier	Jeudi 2 janvier
Mardi Gras	Mardi 5 mars	Mardi 25 fevrier
Vendredi Saint	Vendredi 19 avril	Vendredi 10 avril
Pâques	Dimanche 21 avril	Dimanche 12 avril
Fête du travail	Mercredi 1 mai	Vendredi 1 mai
Fête du drapeau	Samdi 18 mai	Lundi 18 mai
Fête Dieu	Vendredi 31 mai	Jeudi 11 juin
Assomption	Jeudi 15 aout	Samedi 15 aout
Mort de Dessalines	Jeudi 17 octobre	Samedi 17 octobre
La Toussaint	Vendredi 1 novembre	Dimanche 1 novembre
Fête des Morts	Samdi 2 novembre	Lundi 2 novembre
Bataille de Vertière	Lundi 18 novembre	Mercredi 18 novembre
Veille de Noel	Mardi 24 décembre	Jeudi 24 décembre
Noel	Mercredi 25 décembre	Vendredi 25 décembre
Veille Nouvel An	Mardi 31 décembre	Jeudi 31 décembre



TYPE :

Directeur des affaires Administratives et Financiere

BM-

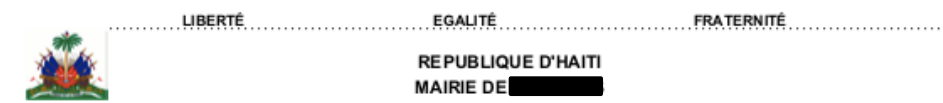
Imprimé par : Admin

Imprimé le :

30/3/2019 8:33:44AM

This letter was designed to be as close to the tax invoice as possible without making any mention of taxes or the tax obligation. Importantly, it includes all of the same personal information identifying the residents and owners of the property. The content of the message however is simply a calendar of upcoming bank holidays.

## Invoice Delivery Script - KREYOL

**INSTRUCTIONS POUR LIVRAISON DU BORDEREAU**

- I. Chercher et trouver l'adresse sur le bordereau et avant de procéder VERIFIER que c'est le MEME.
  - I.A. Ex 15, imp Siméon, Rue charpentier, Mahotièrè 79
- II. Frapper à la porte du contribuable et présentez-vous :
  - II.A. *Toc-toc*
  - II.B. « **Onè pa gen moun ? Bonjour/ bonsoir ! Non mwen se <VOTRE NOM>.**  
**Mwen se yon ajan fiskal Meri XXX. Mwen vin nan kay la pou m vin pote bòdro kay la.**
    - II.B.1. [Avec la première personne que vous trouvez à la maison]
 

**Silvoupè eske se nan kay Mr/M <NON NAN BORDO A> mwen ye la?»**

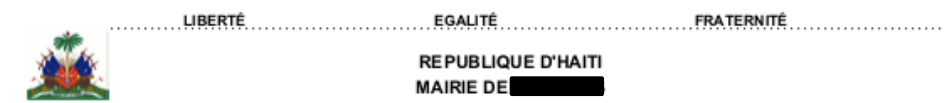
      - II.B.1.a. [Si Wi] Cliquez sur « CONFIRMER » sur votre tablette dans l'espace: « CONFIRMER LE NOM DU PROPRIETAIRE» et vous dites :
 

**« Eske mwen kapab pale avèk Mr/M <NON NAN BORDO A>?»**

        - II.B.1.a.i. [Si wi] Attendez le propriétaire et présentez-vous à nouveau avant de continuer. (Aller à II.B.)
        - II.B.1.a.ii. [Si non] « **Èske m'kapab pale ak yon moun ki responsab kay la? »**
          - [Si wi] Attendez le responsable et présentez-vous à nouveau avant de continuer. (Aller à II.B.)
          - [Si non] « **Tanpri pèmèt mwen prezante ou bòdro kay la. Ou pral responsab pou pataje li ak <NON NAN BORDO A>.** » (Aller à III.)
      - II.B.1.b. [Si non] « **Eske ou kapab verifye pou mwen se nan kay sa a <ADRESSE NAN BORDO A> mwen ye la ?»**
        - II.B.1.b.i. [Si wi] « **Ki non pwopriyetè kay la ? »**
          - Notez le nom et le prénom du propriétaire dans leurs champs respectifs sur votre tablette dans l'espace: « NOUVEAU PROPRIETAIRE DECLARE :» et vous dites :
 

**« Èske m'kapab pale ak pwopriyetè kay la? »**

            - ◇ [Si wi] Attendez le propriétaire et présentez-vous à nouveau avant de continuer. (Aller à II.B.)
            - ◇ [Si non] (Aller à II.B.1.a.ii)
        - II.B.1.b.ii. [Si non] « **Nan ki adrès mwen ye la? »**
          - Vérifiez si vous avez une bodereau pour l'adresse donnée.
            - ◇ [Si wi] « **Mwen regrèt, sa a se bòdro la pou kay sa a.** » (Aller à II.B.1)
            - ◇ [Si non] « **Èske ou ta kapab ed'm jwenn <ADRESSE NAN BORDO A> ? Mèsì anpil pou tan ou. Nou espere ou gen yon bon jou! »**
  - II.B.2. [Lorsque vous répétez l'introduction avec le propriétaire / responsable de la maison]



**Mwen ta vle konfime, eske ou se Mr/M <NON NAN BORDO A ?>**

II.B.2.a. [Si wi] (Aller à III.)

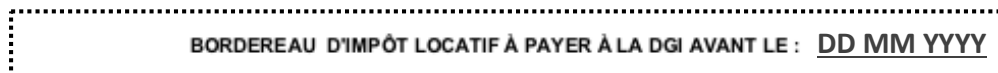
II.B.2.b. [Si non] (Aller à II.B.1.b.)

III. Faire la lecture du bordereau

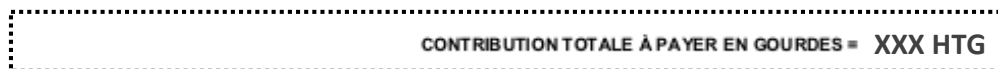
III.A. « **Tankou ou kapab we isi (endike an tet bòdro a), sa sot biwo meri XXX an. »**



III.B. « **Tankou ou kapab we isi (endike an nan bòdro a), ou dwe peye nan biwo DGI la avan <dat endike nan bordo a>»**



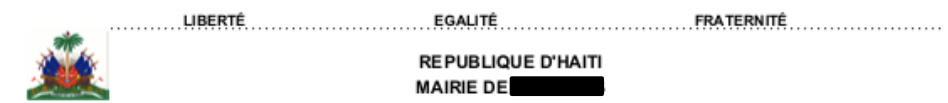
III.C. « **Isi ou ka we montan total ou dwe peye an goud se <kantite kob endike nan bordo a>?»**



III.D. « **Finalman, bordo ou vini avek mesaj sa de biwo meri:**

Li SELMAN sa ki nan bwat nan bordo sa.

**Cher < NON CONTRIBUABLE>**



**ATTENTION : ne pas lire ni communiquer ou même partager les informations des autres bordereaux avec ce contribuable.**

- IV. Si ce n'est pas le nom ou la personne qui est sur le bordereau, demander
- IV.A. « Silvouplè, konfime pou mwen nimewo telefon <NON CONTRIBUTUABLE>. »**  
 Taper numero sur votre tablette dans l'espace: « PREMIER NUMERO DE TELEPHONE DU PROPRIETAIRE »
- IV.B. « Eske <NON CONTRIBUTUABLE> gen yon lot nimewo telefon ? »**  
 Taper numero sur votre tablette dans l'espace: « DEUXIEME NUMERO DE TELEPHONE DU PROPRIETAIRE »
- IV.C. « Ki relasyon ou gen avek <NON CONTRIBUTUABLE>? »**  
 Taper sur votre tablette dans l'espace: « RELATION AVEC CONTRIBUTUABLE »
- IV.D. « Ki jan ou rele? »**  
 Notez le nom et le prénom du répondant dans leurs champs respectifs sur votre tablette dans l'espace: « REpondant QUI N'EST PAS LE PROPRIETAIRE: »
- IV.E. « Ki sa se nimewo telefon pa'w ? »**  
 Taper numero sur votre tablette dans l'espace: « PREMIER NUMERO DE TELEPHONE DU REpondant »
- IV.F. « Eske ou gen yon lot nimewo telefon ? »**  
 Taper numero sur votre tablette dans l'espace: « DEUXIEME NUMERO DE TELEPHONE DU REpondant »
- V. Faire une prise de photo de la maison
- v.A. « Pou m eksplike w, map pran foto kay la, yon fason pou li pi fasil pou nou ka retounen anko »**
- V.B. Klap (fè foto kay la)
- VI. Prendre un point GPS
- vi.A. « Pou m eksplike w, map pran pwèn GPS kay la, yon fason pou li pi fasil pou nou ka retounen anko »**
- VI.B. Pran pwèn GPS la
- VII. Pour terminer, l'agent doit dire :
- vii.A. « Mwen swete w yon bon fen jounen. »**



## A.4 Survey Tools

### Qualitative Questionnaire - KREYOL

1. Ki mo oswa slogan nou souvan itilize pou nou rele yon zanmi oubyen vwazen ke nou respekte anpil?
  - 1.a. Ki pi bon aksyon ou ka fè, pou montre lòt moun ke nan zòn nan yo respektew anpil \_\_\_\_\_?
  
2. Ki mo ou ta itilize pou dekri yon moun ki (yon) \_\_\_\_\_ ?
  - 2.a.i. bon vwazen
  - 2.a.ii. bon sitwayen
  - 2.a.iii. bon lidè
  - 2.a.iv. bon ASEC
  - 2.a.v. bon CASEC
  - 2.a.vi. bon polis
  - 2.a.vii. bon majistra
  - 2.a.viii. Notab
  - 2.a.ix. entelijan
  - 2.a.x. moun onèt
  - 2.a.xi. Bon bagay
  - 2.b. Ki bagay ki pi enpòtan ou ka fè pou montre lòt moun ou se (yon) \_\_\_\_\_?
  
3. Pami zanmi, fanmi, ak vwazen nou yo, ki mo nou souvan itilize pou dekri yon moun ke nou pa gen respè pou li?
  - 3.a. Ki kalite aksyon nou ka pran kòm egzanzp pou montre ke moun sa, moun nan zòn nan pa renmenl \_\_\_\_\_?
  
4. Ki mo ou ta itilize pou dekri yon moun ki yon \_\_\_\_\_ ?move vwazen
  - 4.a.i. move sitwayen
  - 4.a.ii. move ASEC
  - 4.a.iii. move CASEC
  - 4.a.iv. move polis

- 4.a.v. move majistra
  - 4.a.vi. pa jenere
  - 4.a.vii. inkredul
  - 4.a.viii. nayif
  - 4.a.ix. bet
  - 4.a.x. koronpi
  - 4.b. Ki bagay oswa aksyon ki pi enpòtan ou ka fè pou montre lòt moun ou se (yon) \_\_\_\_\_?
5. Konbyen legliz ak lye kote moun priye ki genyen nan zòn nan?
- 5.a. Konbyen manm ki genyen konsa?
  - 5.b. Ki kalite travay legliz \_\_\_\_\_ fè?
  - 5.c. Ki jan manm yo sipòte ak kontribye nan travay \_\_\_\_\_ la? (lajan, materyèl, travay, lòt ?)
  - 5.d. Ki jan lòt moun (non manm) sipòte ak kontribye nan travay \_\_\_\_\_ la?
6. Ki non òganizasyon, asosyasyon, ak komite yo ki aktif nan katye sa a?
- 6.a. Konbyen manm yo \_\_\_\_\_ genyen?
  - 6.b. Ki kalite travay yo \_\_\_\_\_ fè?
  - 6.c. Ki jan manm yo sipòte ak kontribye nan travay ki fèt yo \_\_\_\_\_ ? (lajan, materyèl, travay, lòt ?)
  - 6.d. Ki jan lòt moun (non manm) sipòte ak kontribye nan travay \_\_\_\_\_ la?
7. Èske katye sa a gen lòt gwoup aprè sa nou sòt site la yo ? kòman nou rele yo \_\_\_\_\_?
- 7.a.i. Kombit
  - 7.a.ii. Brigad
  - 7.a.iii. Kovee
  - 7.a.iv. Ladian
  - 7.a.v. Douvanjou

- 7.a.vi. Dine-manchette
- 7.a.vii. Colonne
- 7.a.viii. Ronde
- 7.a.ix. Ramponneau
- 7.b. Si wi :
- 7.b.i. Ki kalite travay \_\_\_\_\_ konn fè nan katye a?
- 7.b.ii. Kouman yo pran desizyon lè yon travay ap fèt nan zòn nan \_\_\_\_\_?
- 7.b.iii. Eske se moun ki bay pi bon lide a kip ase ? \_\_\_\_\_ ki estriktire yo genyen \_\_\_\_\_?
- 7.b.iv. Nan ki gwoup nou te deja mansyone yo nou ka pran \_\_\_\_\_?
- 7.b.v. Ki non lòt gwoup oubyen òganizasyon katye ke nou poko mansyone ki egziste, kòman nou rele \_\_\_\_\_?
- 7.b.v.1. Konbyen manm yo genyen konsa \_\_\_\_\_?
- 7.b.v.2. Ki kalite travay yo fè \_\_\_\_\_?
- 7.b.v.3. Ki jan manm yo sipòte ak kontribye nan travay kap fèt yo \_\_\_\_\_ ?  
(lajan, materyèl, travay, lòt ?)
- 7.b.v.4. Ki jan lòt moun ki pa manm yo sipòte ak kontribye nan travay \_\_\_\_\_ kap fèt yo \_\_\_\_\_?
8. Nan katye sa a, èske gen yon lòt gwoup ke nou poko mansyone kote moun pataje travay oswa travay ansanm pou objektif komen yo?
- 8.a. Si wi :
- 8.a.i. Kòman nou rele gwoup moun sa yo an general \_\_\_\_\_?
- 8.a.ii. Ki non endividyèl gen chak gwoup diferan nou dekri tankou \_\_\_\_\_ an jeneral?
- 8.a.iii. Konbyen manm \_\_\_\_\_ genyen?
- 8.a.iv. Ki kalite travay yo \_\_\_\_\_ fè?
- 8.a.v. Ki jan manm yo sipòte ak kontribye nan travay \_\_\_\_\_ la? (lajan, materyèl, travay, lòt ?)

- 8.a.vi. Ki jan lòt moun (ki pa manm) sipòte ak kontribye nan travay \_\_\_\_\_ la?
9. Ki sèvis ou enfrastrikti ou pataje avèk lòt vwazen?
- 9.a. Kilès oswa ki gwoup ki te konstri yo/etabli \_\_\_\_\_?
- 9.b. Kòman ou menm oswa vwazen ou yo te kontribye pou konstri yo / etabli \_\_\_\_\_?
- 9.c. Kilès oswa ki gwoup ki pa dekouraje \_\_\_\_\_?
- 9.d. Kòman ou menm oswa vwazen ou yo fè pou kontinye \_\_\_\_\_?
10. Ki sèvis **piblik** ou enfrastrikti **piblik** ou pataje avèk lòt vwazen?
- 10.a. Kilès oswa ki gwoup ki te konstri li/etabli \_\_\_\_\_?
- 10.b. Kòman ou menm oswa vwazen ou yo te kontribye nan konstriksyon an/ etabli \_\_\_\_\_?
- 10.c. Kilès oswa ki gwoup ki kontinye (ki bay antretyen) \_\_\_\_\_?
- 10.d. Kòman ou menm oswa vwazen ou yo kontribye pou kontinye (bay antretyen pou) \_\_\_\_\_?
11. Kòman nou bay kontribisyon pan ou (eske se lajan, materyèl, travay, lòt bagay ?) ou dwe bay kòm moun kap viv nan kominote a?
- 11.a. Èske se menm kantite kontribisyon tout moun kap viv nan kominote a bay?
- 11.b. Ki kantite moun nan vwazinaj la ki respekte pwomès yo te fè pou kontribye nan travay kap fèt la?
- 11.c. Ki moun ki ranfòse oswa ankouraje moun yo ranpli obligasyon sa a?
- 11.d. Ki konsekans sa ap genyen si yon moun pa respekte obligasyon sa a?
12. Ki kantite taks ke ou dwe peye kòm yon moun kap viv nan kominote sa a?
- 12.a. Èske menm kantite se menm kantite kontribisyon an ke tout vwazen yo bay?
- 12.b. Konbyen nan moun yo respekte obligasyon sa a?
- 12.c. Ki moun ki ranfòse oswa ankouraje moun yo ranpli obligasyon sa a?
- 12.d. Ki sanksyon yap pran kont moun ki pa akonpli obligasyon sa a?

13. Si nou ta chwazi 10 nan vwazen ou yo o aza (sa vle di, nan yon fason aléatoire), konbyen moun (ant 0-10) ou kwè fè bagay sa yo:
  - 13.a. Empo lokatif la (cfpb)
    - 13.a.i. Peye empo lokatif la (cfpb) ane pase la
    - 13.a.ii. Peye empo lokatif la (cfpb) omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
    - 13.a.iii. Ta di ou ke yo peye empo lokatif la (cfpb) ane pase la
    - 13.a.iv. Ta di ou ke yo peye empo lokatif la (cfpb) omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
  - 13.b. Patante
    - 13.b.i. Peye patante ane pase la
    - 13.b.ii. Peye patante omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
    - 13.b.iii. Ta di ou ke yo peye patante ane pase la
    - 13.b.iv. Ta di ou ke yo peye patante omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
  - 13.c. lòt taks
    - 13.c.i. Peye lòt taks ane pase la
    - 13.c.ii. Peye lòt taks omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
    - 13.c.iii. Ta di ou ke yo peye lòt taks ane pase la
    - 13.c.iv. Ta di ou ke yo peye lòt taks omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
  - 13.d. kontribisyon nan legliz li oswa òganizasyon relijye'l
    - 13.d.i. Peye kontribisyon nan legliz li oswa òganizasyon relijye'l ane pase la
    - 13.d.ii. Peye kontribisyon nan legliz li oswa òganizasyon relijye'l omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
    - 13.d.iii. Ta di ou ke yo peye kontribisyon nan legliz li oswa òganizasyon relijye'l ane pase la

- 13.d.iv. Ta di ou ke yo peye kontribisyon nan legliz li oswa òganizasyon relijye' l omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
- 13.e. kontribisyon nan òganizasyon lokal yo
- 13.e.i. Peye kontribisyon nan òganizasyon lokal yo ane pase la
- 13.e.ii. Peye kontribisyon nan òganizasyon lokal yo omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
- 13.e.iii. Ta di ou ke yo peye kontribisyon nan òganizasyon lokal yo ane pase la
- 13.e.iv. Ta di ou ke yo peye kontribisyon nan òganizasyon lokal yo omwen yon fwa nan senk ane ki sot pase yo (2013-2018)
14. *Mwen ta renmen pale yon ti jan plis sou taks. Ann itilize empo lokatif la (cfpb) tankou yon egzanp. Mwen konprann ke pa tout moun isit la bezwen peye sa a, men se pou nou imagine pou egzèsis sa a ke li se yon bagay ki kay ou ta dwe peye:*
- Poukisa se konsa kèk (oswa anpil) moun peye sa a?
- 14.a. Ki jan sa afekte volonte ou pou peye ?
- 14.b. Si ou te jwenn ke yon pousantaj pi **wo** nan moun yo (sa vle di, ke **plis moun yo**) peye pase sa ou kwè, ki jan sa ta afekte volonte ou pou peye?
- 14.c. Si ou te jwenn ke yon pousantaj pi **ba** nan moun yo (sa vle di, ke **mwens moun yo**) peye pase sa ou kwè, ki jan sa ta afekte volonte ou pou peye?
- 14.d. Ki se to empo lokatif aktyèl la (cfpb)?
- 14.e. Ki se etap yo pou peye empo lokatif la (cfpb) (sa vle di, ki kote ou bezwen ale, ki biwo ou bezwen visite, ki moun ou bezwen wè, ak ki jan ou peye taks sa a)?
- 14.f. Ki sa ki ta ogmante volonte ou pou peye empo lokatif la (cfpb)?
- 14.g. Èske gen yon moun nan katye a ke si ou te konnen li te peye empo lokatif la (cfpb), sa a ta ogmante volonte ou pou peye?
- 14.h. Ki ankourajman ki ta ogmante volonte ou pou peye?
- 14.i. Ki konsekans legal yo gen, si genyen, si ou pa peye?
- 14.i.i. Èske w te janm konnen nenpòt ki moun ki te resevwa konsekans sa yo?

- 14.j. Ki konsekans sosyal yo gen, si genyen, si ou pa peye?
- 14.j.i. Èske w te janm konnen nenpòt ki moun ki te resevwa konsekans sa yo?
- 14.k. Ki konsekans moral yo gen pou ou pèsònèlman, si genyen, si ou pa peye ?
- 14.l. Ki karakteristik ki dwe yon pwopriyete gen pou mèt pwopriyete a dwe empo lokatif la (cfpb)?
- 14.m. Èske to aktyèl yo pou empo lokatif la (cfpb) jis?
- 14.n. Si non, ki sa ki ta yon to empo lokatif la (cfpb) jis?
- 14.o. Ki sa ou panse nan yon moun ki peye empo lokatif la (cfpb)?
- 14.p. Èske ou ta vle vwazen ou yo konnen si ou peye empo lokatif la (cfpb)?
- 14.p.i. Pou ki sa ?
15. Sèvis ke otorite lokal la bay
- 15.a. Ki sèvis ki pi enpòtan majistra-a bay?
- 15.b. Ki sèvis ki pi enpòtan ke ou ta renmen resevwa nan men majistra a men katye sa a swa pa resevwa yo oswa pa resevwa ase nan yo?
- 15.c. Kisa kominote sa a fè lè majistra-a pa bay ase netwayaj lari oswa retire fatra?
- 15.d. Èske gen nenpòt moun ki te isit la ki te travay kòm yon "Cantonnier" pou majistra-a oswa ki te anboche nan pwogram travay ke biwo majistra-a bay?
- 15.e. Èske nenpòt nan ou gen manm fanmi ki te travay kòm "Cantonnier" pou majistra-a oswa te anboche nan pwogram travay ke biwo majistra-a bay?
16. Ki sa ki « demokrasi »?
- 16.a. Ki jan enpòtan se demokrasi pou Ayiti?
- 16.b. Ki jan enpòtan se demokrasi pou ou pèsònèlman?
- 16.c. Si ou pa t 'kontan ak gouvènman an oswa otorite lokal yo oswa ou te vle bay opinyon ou sou yon politik sèten oswa lalwa :
- 16.c.i. ki sa ki ta pi efikas pou ou pou w fè opinyon ou tande a?
- 16.c.i.1. Èske w te janm fè sa?
- 16.c.i.2. Si wi, pou ki règleman oswa pwoblèm?

- 16.c.ii. ki sa ki ta pi pwobableman fason pou ou fè opinyon ou tande a?
  - 16.c.ii.1. Èske w te janm fè sa?
  - 16.c.ii.2. Si wi, pou ki règleman oswa pwoblèm?
- 16.d. Ki moun ki isit la te patisipe nan yon reyinyon nan biwo majistra a oswa bay fidbak nan kèk fason? (Si wi, leve men ou)
- 16.e. Ki moun ki isit la te vote nan eleksyon ki sot pase a ? (Si wi, leve men ou)
  - 16.e.i. Pou moun ki pa vote:
    - 16.e.i.1. poukisa ou pa te vote?
    - 16.e.i.2. Èske ou planifye pou vote pwochen fwa?
    - 16.e.i.3. Ki sa ki ta ogmante pwobabilite ou pou vote pwochen fwa?
  - 16.e.ii. Pou moun ki te vote :
    - 16.e.ii.1. Poukisa ou te vote?
    - 16.e.ii.2. Èske ou planifye pou vote pwochen fwa?
    - 16.e.ii.3. Ki sa ki ta ogmante pwobabilite ou pou vote pwochen fwa?
    - 16.e.ii.4.



## Quantitative Questionnaire - ENGLISH

Hour and minute that the survey starts: \_\_\_\_: \_\_\_\_

FECHA. Date of the survey Day: \_\_ Month: \_\_\_\_\_ Year: 2018

[Note for surveyor : Before each question, there is a code in bold letters, for example: "MUNICIPIO", "HAISEKSYON", or "SD2bk1". Do not read this code. It is simply a tool for data entry. Also, do not the texts written between brackets [...]]

**Opening:** Hello, my name is \_\_\_\_\_, and I'm calling you to ask a few questions about community organizations, social services and citizenship in Haiti.

If you complete all of the questions, I will send you phone credit worth 50 HTG as an expression of gratitude for your time.

The survey will take approximately 20 minutes. You can pass on any question and you can end the survey at any time.

Do you agree to proceed?

**[If No] :** Thank you for your time. We wish you a good day.

**[Wait until the other person ends the call before you hang.]**

**[If Yes]**

To give you a bit of additional information, I'm calling on behalf of Benjamin KRAUSE, a researcher at the University of California BERKELEY in the United States. We will work to assure your confidentiality. All responses will be saved in an anonymized database protected by a password and only Mr. Krause will have access to the information. As with all research there remains the possibility that your confidentiality could be compromised, however we are taking precautions to minimize this risk.

There is no direct benefit to you from taking part in this study. However, we hope that the information gained from the study will help inform future efforts to improve government services and responsiveness.

Do you consent to participate in this study? That is to say, are you OK to continue with this survey? Please respond yes or no.

**[If no] :** Thank you for your time. Who sincerely wish you good day.

**[Stay on the line until the persn on the other end hangs up before going to your next call.]**

[If yes] : Excellent. Thank you.

**To begin, allow me to ask you some general information:**

**MUNICIPIO.** In which city/municipality do you?

**[Do NOT read the responses, select only one response.]**

- (1) Carrefour
- (2) Port-au-Prince
- (3) Cite Soleil
- (4) Delmas
- (5) Gressier
- (6) Kenscoff
- (7) Petionville
- (8) Tabarre
- (9) Other \_\_\_\_\_
- (10) (888888) Do NOT know **[DO NOT READ THIS ANSWER]**
- (11) (988888) No response **[DO NOT READ THIS ANSWER]**

**HAISEKSYON.** In which Communal Section do you live ?

**[Do NOT read these answers, select only one response]**

- (1) Morne Chandelle
- (2) Platon Dufréné
- (3) Taïfer
- (4) Procy
- (5) Coupeau
- (6) Bouvier
- (7) Lavalle
- (8) Berly
- (9) Bizoton
- (10) Thor
- (11) Rivière Froide
- (12) Malanga
- (13) Corail Thor
- (14) Other in Carrefour \_\_\_\_\_
- (15) Other outside Carrefour \_\_\_\_\_

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**HAISEC.** In which neighborhood do you live ?

**[Do NOT read these answers, select only one response]**

[1] "Arcachon/Mon-repos"	[22] "Grand Caye/Marre Bizoton"	[43] "Morne-Tesseract"
[2] "Bas Bizoton"	[23] "Grand Ravine"	[44] "Nan Citron - Village Michel"
[3] "Bas Thorland"	[24] "Haut Chaud-Eau"	[45] "Paloma/Canne-à-Sucre"
[4] "Bertin"	[25] "Haut La Grenade"	[46] "Pandjamen/Sous-Rails"
[5] "Bizoton 51"	[26] "Haut Mariani"	[47] "Petite Saline - Chiwawa"
[6] "Bizoton/Bergamot"	[27] "Haut Rivière-Froide"	[48] "Phare Lamentin"
[7] "Bizoton/Mont-Carmel-Diquini"	[28] "Haut Thor"	[49] "Ravine Grand-Fond"
[8] "Bois Dioute - Ti Kajou"	[29] "Immaculée Conception"	[50] "Ravine Ti-Source"
[9] "Brochette"	[30] "Jérusalem"	[51] "Rivière Froide"
[10] "Calebacher"	[31] "La Belair"	[52] "Rivière Froide (Kay Frè)"
[11] "Campus Diquini"	[32] "La Grenade"	[53] "Sapotille"
[12] "Centre Sportif de Carrefour"	[33] "Lajoie"	[54] "Source Corrossol"
[13] "Changement"	[34] "Laramé"	[55] "St-Charles - Lamentin"
[14] "Chaud-Eau"	[35] "Le Lambi"	[56] "Terrain Ti Carne"
[15] "Cocover/Bas Rails"	[36] "Littoral Est /Gressier"	[57] "Ti Source"
[16] "Côte-Plage"	[37] "Mahotièrè"	[58] "Village Petit Paradis"
[17] "Degand"	[38] "Malcom"	[59] "Waney"
[18] "Do Karyann"	[39] "Mariani Est"	[60] Other in Carrefour _____
[19] "Domaine Idéal"	[40] "Mariani Ouest"	[61] Other outside Carrefour _____
[20] "Fontamara Est"	[41] "Marine Haitienne"	(888888) Do NOT know <b>[DO NOT READ THIS ANSWER]</b>
[21] "Gaston-Magron"	[42] "Morne Madan Garnot"	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>

**UR.** Do you live in an area that is (1) urban or (2) rural

**Q1. [Ask only if you are not certain]** Sex: (1) Male (2) Female

**Q2.** How old are you?

# \_\_\_\_\_ Ane **[MUST BE OLDER THAN 18 YEARS OLD]**

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**QBK2.** Including yourself, how many people live in your home?

# \_\_\_\_\_ people

For the rest of the survey, when I asked about « *you and all of the members of your household together* » I am referring to these people that you just counted together.

**LS3.** Are you satisfied with your life? Would you say that you are ...**[Read the possible answers]**

- (1) Very satisfied  
 (2) A little satisfied  
 (3) A little unsatisfied  
 (4) Not at all satisfied  
 888888=Do NOT know [DO NOT READ THIS ANSWER]  
 988888=No response [DO NOT READ THIS ANSWER]

**Let's talk about your city or municipality:**

**HAIMUNI8.** Could you please tell me what are the the **three most important priorities** for local government in terms of improving your community.

[DO NOT READ THE LIST OF OPTIONS, indicate one in each column]

	First Response HAIMUNI8A	Second Response HAIMUNI8B	Third Response HAIMUNI8C
Cleaning the street and public places	66	66	66
Picking up garbage	67	67	67
Building schools	12	12	12
Security in the neighborhood	2	2	2
Creating employment	3	3	3
Building roads	4	4	4
Potable water	5	5	5
Electricity and renewable energy	6	6	6
Sanitation	7	7	7
Housing	8	8	8
Restoring/protecting the natural environment	9	9	9
Strengthening the capacity of central government	13	13	13
Strengthening the capacity of local government or of the mayor	11	11	11
Other priority	77	77	77
Do NOT know [DO NOT READ THIS ANSWER]	888888	888888	888888
No response [DO NOT READ THIS ANSWER]	988888	988888	988888

**A4. In your opinion, what is are the three biggest problems in the city/?****[Do NOT read these answers, select only one response]**

Abduction, kidnapping	31	Population growth	20
Unemployment/There isn't work	3	No power (Electricity from the national electric company, EDH)	24
Deforestation/Environmental problems	10	No housing	55
Forced displacement	32	No financial services	9
Foreign debt	26	No schools, poor quality of schooling	21
Discrimination	25	No security	27
Drugs	11	No land for planting	7
Impunity	61	No water	19
Gangs	14	Politicians	59
Corruption	13	Poverty	4
Crime, insecurity, violence	5	Taking care of people injured by the earthquake	63
War	30	Economic problems, economic crisis	1
War on Terrorism	17	Problem with transport	60
Cost of living, inflation	2	Protests (stikes)	6
Social inequality	58	Rebuilding the country after the earthquake	62
Malnutrition	23	Drug trafficking	1
Lack of sanitation services	22	Human rights violations	56
Migration	16	Terrorist acts	33
Poor governance, weak state	15	Cleaning the streets and public places	66
Bad roads	18		
Trash removal	67		
		Other	70
No response <b>[DO NOT READ THIS ANSWER]</b>	888888	Do NOT know <b>[DO NOT READ THIS ANSWER]</b>	988888

**CP.** Now I will give you a list of groups and organizations. Please, tell me for each how often you attending their organizational meetings: once a week, one or two times a month, one or two times a year, or you do not attend meetings of this group. [Repeat “once a week”, “one or two times per month”, “one or two times per year”, or “I do not attend meetings for this group” to help the respondent in answering the questions]

	once a week	one or two times per month	one or two times per year	I do not attend meetings for this group	Do NOT know [DO NOT READ THIS ANSWER]	No response [DO NOT READ THIS ANSWER]	NOT applicable [DO NOT READ THIS ANSWER]
<b>CP6.</b> Church or meetings of your religious organization? Do you attend...	1	2	3	4	888888	9888888	999999
<b>CP8.</b> Neighborhood improvement committee (Neighborhood Committee or Community Association)? Do you attend...	1	2	3	4	888888	9888888	999999
<b>CPbk1.</b> Other local organization, foundation, association, local NGO, committee, group, team or other form of “working group”? Do you attend ...	1	2	3	4	888888	9888888	999999
<b>CPI3.</b> Meetings of a political party or political movement? Do you attend ...	1	2	3	4	888888	9888888	999999
<b>CP20.</b> [Only for women] Meetings for women’s groups or home makers clubs? Do you attend ...	1	2	3	4	888888	9888888	999999

**NP1.** Have you participated in a Commune Assembly meeting or a meeting at the mayor’s office in the past 12 months?

- (1) Yes
- (2) No
- (888888) Do NOT know [DO NOT READ THIS ANSWER]
- (988888) No response [DO NOT READ THIS ANSWER]

**SD2bk0.** In your neighborhood, are you completely satisfied, satisfied, not satisfied or not at all satisfied with the work of the mayor’s office and the services they provide?

- (1) Completely satisfied
- (2) Satisfè
- (3) Not satisfied
- (4) Not at all satisfied
- 888888=Do NOT know [DO NOT READ THIS ANSWER]
- 988888=No response [DO NOT READ THIS ANSWER]
- (999999) NOT applicable (Does NOT use or access services) [DO NOT READ THIS ANSWER]

**SD2bk1.** In your neighborhood, are you completely satisfied, satisfè, not satisfied or Not at all satisfied with trash

removal?

- (1) Completely satisfied
- (2) Satisfied
- (3) Not satisfied
- (4) Not at all satisfied

888888=Do NOT know [DO NOT READ THIS ANSWER]

988888=No response [DO NOT READ THIS ANSWER]

(999999) NOT applicable (Does NOT use or access services) [DO NOT READ THIS ANSWER]

**SD2bk2.** In your neighborhood, are you completely satisfied, satisfè, Not satisfied or Not at all satisfied with the cleaning of streets and public spaces?

- (1) Very satisfied
- (2) A little satisfied
- (3) A little unsatisfied
- (4) Not at all satisfied

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

**SD2bk3.** In the past month, how many times has the mayor's trucks passed through your street to collect trash?

# \_\_\_\_\_ times

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

**SD2bk4a.** Think of all of the ways that neighbors in your community come together to realize a common goal whether you call these efforts « kombit », « kove », « douvanjou », « kolon », « tet ansanm », or another, in the past month, how many times has your community come together in this way

# \_\_\_\_\_ times

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

**SD2bk4b. [If the response is greater than zero]** When you think of those times over the past month, how many combined hours have you and the other members of your household spent in total in such efforts?

# \_\_\_\_\_ hours

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

**SD2bk5.** When you think of those times over the past month, how much money have you and the other members of your household contributed in total ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [**DO NOT READ THIS ANSWER**]

**SD2bk6.** When you think of those times over the past month, if your household contributed materials or food to support the work, what was the total value of your contribution ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [**DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.**]

(888888) Do NOT know [**DO NOT READ THIS ANSWER**]

(988888) No response [**DO NOT READ THIS ANSWER**]

**CPbk7.** How many times you or the other members of your household te patisipe nan yon manifestasyon nan dènye month?

# \_\_\_\_\_ times

(888888) Do NOT know [**DO NOT READ THIS ANSWER**]

(988888) No response [**DO NOT READ THIS ANSWER**]

**CPbk8.** How many days all together have you or the other members of your household worked as a "Street Cleaner" for the mayor's office or were included in a work program sponsored by the mayor's office in the past month?

# \_\_\_\_\_ days

(888888) Do NOT know [**DO NOT READ THIS ANSWER**]

(988888) No response [**DO NOT READ THIS ANSWER**]

### **Let's talk about your household's revenue and expenditures:**

**REVbkALL.** Concerning household revenue :

When we talk about all of the ways and sources of money together including:

- Salary or wages,
- Money earned in a small business,
- Money received as a gift,
- Money received as a loan,
- Money won in the lottery or through other forms of gambling,
- All other ways of getting money ;

**In the past month**, how much money did all of the members of your household together receive, that is say, in the past month, what is the total revenue of your household? \_\_\_\_\_ \$/HTG/Haitian Dollars

[**DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.**]

On household expenses:

**DEPbk1.** In the past month, how much money has all of the members of your household together spent on food?

\_\_\_\_\_ \$/HTG/Haitian Dollars [**DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.**]

(888888) Do NOT know [**DO NOT READ THIS ANSWER**]

(988888) No response [**DO NOT READ THIS ANSWER**]



**DEPbk2.** In the past month, how much money has all of the members of your household together spent on transport?

\_\_\_\_\_ \$/HTG/Haitian Dollars [DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

**DEPbk3.** In the past month, how much money has all of the members of your household together spent on telephone credit?

\_\_\_\_\_ \$/HTG/Haitian Dollars [DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

**DEPbk4.** In the past month, how much money has all of the members of your household together spent to have trash removed from the house?

\_\_\_\_\_ \$/HTG/Haitian Dollars [DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

**DEPbk5.** In the past month, how much money has all of the members of your household together spent to have garbage removed from the neighborhood?

\_\_\_\_\_ \$/HTG/Haitian Dollars [DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

In the past month, how much money has all of the members of your household together spent for tithes?

\_\_\_\_\_ \$/HTG/Haitian Dollars [DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

**DEPbk6.** In the past month, how much money has all of the members of your household together spent at other religious centers?

\_\_\_\_\_ \$/HTG/Haitian Dollars [DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]

(888888) Do NOT know [DO NOT READ THIS ANSWER]

(988888) No response [DO NOT READ THIS ANSWER]

**DEPbk7.** In the past month, how much money has all of the members of your household together spent to support

local organizations, foundations, associations, local NGOs, committees, groups, teams, or other forms of “team work”?

\_\_\_\_\_ \$/HTG/Haitian Dollars **[DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]**

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPbk8.** In the past month, if you or one of the members of your household sought services at the police station or had another interaction with the police and as a result had to pay some money on the side, how much money was given? That is to say, in the past month, how much money have all of the members of your household together spent on bribes to police?

\_\_\_\_\_ \$/HTG/Haitian Dollars **[DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]**

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPbk9.** In the past month, if you or one of the members of your household sought services at the mayor’s office or had another interaction with members of the mayor’s office and as a result had to pay some money on the side, how much money was given? That is to say, in the past month, how much money have all of the members of your household together spent on bribes to local government officials?

\_\_\_\_\_ \$/HTG/Haitian Dollars **[DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]**

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPbk10.** In the past month, if you or one of the members of your household sought services at one of the offices of the national government or had another interaction with officials working for the national government and as a result had to pay some money on the side, how much money was given? That is to say, in the past month, how much money have all of the members of your household together spent on bribes to national government officials?

\_\_\_\_\_ \$/HTG/Haitian Dollars **[DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]**

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPbkALL.** Finally, when we consider all of the different items you spend your money on including:

- Everything we have just mentioned
  - o Food
  - o Transport
  - o Telephone
  - o Removing trash from your house
  - o Garbage pick-up from the neighborhood
  - o Tithes
  - o Other religious centers
  - o Support for local organizations
  - o Bribes to the police and other government officials
- And all of the other items you spend money on including
  - o clothes

- electricity
- health
- school
- lottery and gambling
- small business expenses
- leisure

in the past month, how much money has all of the members of your household together spent, in other words, in the past month, what is the total expenditures of your household

\_\_\_\_\_ \$/HTG/Haitian Dollars **[DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]**

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPAbk1a.** In the past year, have you paid for housing?

(1) Yes

(2) No

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPAbk1b. [If yes]** In which month did you pay for housing ?

\_\_\_\_\_ month

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPAbk1c. [If yes]** How much money did you spend on your housing?

\_\_\_\_\_ \$/HTG/Haitian Dollars **[DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]**

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPAbk2a.** In the past year, have you paid property taxes?

(1) Yes

(2) No

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPAbk2b. [If yes]** In which month did you pay property taxes ?

# \_\_\_\_\_ month

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPAbk2c. [If yes]** How much money did you spend on property taxes?

\_\_\_\_\_ \$/HTG/Haitian Dollars **[DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]**

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**DEPAbk3a.** In the past year, did you pay the business license tax?

- (1) Yes  
 (2) No  
 (888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**DEPAbk3b. [If yes]** In which month did you pay the business license tax ?

- # \_\_\_\_\_ month  
 (888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**DEPAbk3c. [If yes]** How much money did you spend on the business license tax ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]

- (888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**DEPAbk4a.** Do you have other annual expenses?

- (1) Yes  
 (2) No  
 (888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**DEPAbk4b. [If yes]** In the past year, money much in total have you spent on all the other annual expenses not yet mentioned ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]

- (888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

### **Now let's talk about property taxes:**

**ELbk1.** At what point (in construction) does a house owe property taxes ? (A house must be/have . . .  
 [Do NOT read these answers. Do not accept "finished". If the respondent says "finished", respond with the question, "What does 'finished' mean?"]

1. Painted
  2. People living inside
  3. Built with bloc
  4. Framed with wood
  5. Roof
  6. You do not need to have a house, if you have land, you must pay property taxes:
- (888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**ELbk2.** What is the current property tax rate for a house like yours ?

\_\_\_\_\_ %

(888888) Do NOT know [DO NOT READ THIS ANSWER]  
(988888) No response [DO NOT READ THIS ANSWER]

**ELbk3a.** Do you believe that the property tax rate is just?

(1) Yes

(2) No

(888888) Do NOT know [DO NOT READ THIS ANSWER]  
(988888) No response [DO NOT READ THIS ANSWER]

**ELbk3b. [If yes], why?**

**[Do NOT read these answers, indicate all that are mentioned]**

1. It is an obligation
2. The law decides it, so it is just
3. It helps improve the community
4. There are a lot of needs in the community and property taxes are the principal way to pay for them.
5. Creates employment (jobs)
6. We have a safe (secure) community
7. Building the market
8. Improving the management of the city
9. Building public spaces
10. Building public libraries
11. People who have should share with those that do not (redistribution)
12. (Proverb meaning) Many hands make light work
13. (Proverb meaning) Some tasks can only be accomplished when we work together
14. Other \_\_\_\_\_

**ELbk3c. [If no], why not ?**

**[Do NOT read these answers, indicate all that are mentioned]**

1. There aren't any services
2. The services are insufficient
3. The rate is too high
4. There isn't any work/lack of adequate work/lack of means to pay
5. The state did not help me build my house
6. I do not vote for/support the local government
7. Other \_\_\_\_\_

**ELbk3d. [If no], In your opinion, what would be a just property tax rate for a house like yours ?**

\_\_\_\_\_ %

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**ELbk4. What are the legal consequences if you do not pay property taxes?**

**[Do NOT read these answers, indicate all that are mentioned]**

1. None
2. The state can seize your house/ throw you out of your house
3. The state can tear down your house
4. If the mayor is doing work in your area and needs to modify or destroy your house in the process, you will not receive any reimbursement for your losses.
5. Difficulties with the bank or other private institutions that require you to prove ownership and past tax payments on your house.

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**ELbk5a. What percent of your neighbors, in other words how many out of 100, do you believe paid property taxes last year?**

\_\_\_\_\_ %

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**ELbk5b.** What percent of your neighbors, in other words how many out of 100, do you believe paid property taxes at least once in the past 5 years?

\_\_\_\_\_ %  
 (888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**ELbk5c.** What percent of your neighbors, in other words how many out of 100, do you believe would need to pay their property taxes to motivate you to pay yours?

\_\_\_\_\_ %  
 (888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**ELbk6.** What do you think of a person who pays property taxes?

**[Do NOT read these answers, indicate all that are mentioned]**

1. Good citizen
2. Honorable
3. Generous
4. Wants to avoid risk/ has a low tolerance for risk
5. Gullible
6. Naive
7. Foolish
8. Selfish
9. Wants to protect himself/herself
10. Trator
11. Other \_\_\_\_\_

(888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**ELbk7a.** Would you want your neighbors to know if you paid your property taxes ?

(1) Yes  
 (2) No  
 (888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**ELbk7b. [If yes], why?**

**[Do NOT read these answers, indicate all that are mentioned]**

1. To encourage others to pay
2. I would be proud to contribute
3. To improve my social standing
4. Because it is a think we must do
5. To provide a good example for my children.
6. It would not be an issue, so I would not worry if others knew.
7. To avoid confusion with local government so that they would not mistakenly return to collect from me a second time.
8. Other \_\_\_\_\_

(888888) Do NOT know [DO NOT READ THIS ANSWER]  
 (988888) No response [DO NOT READ THIS ANSWER]

**ELbk7c. [If no], why not ?**

**[Do NOT read these answers, indicate all that are mentioned]**

1. To maintain my social status
2. To avoid other people thinking that I'm pressuring them to pay.

3. To avoid other people knowing that I waste (do not spend wisely) my money
  4. To avoid other people thinking I'm gullible
  5. To avoid other people thinking that I am trying to get ahead of them.
  6. To protect my family
  7. Because I would be afraid of what might happen to me or my family if other people knew.
  8. Other \_\_\_\_\_
- (888888) Do NOT know [**DO NOT READ THIS ANSWER**]  
 (988888) No response [**DO NOT READ THIS ANSWER**]

**ELbk8a.** To which level (between 1-7 with 1 meaning "NOT AT ALL" and 7 meaning "I COMPLETELY AGREE") do you agree with the following statement::

If I paid my property taxes, I would want my neighbors to know.

# \_\_\_\_\_ [Write the number between 1-7]  
 888888=Do NOT know  
 988888=No response

**ELbk8b.** To which level (between 1-7 with 1 meaning "NOT AT ALL" and 7 meaning "I COMPLETELY AGREE") do you agree with the following statement::

If I did **NOT** pay my property taxes, I would want my neighbors to know.

# \_\_\_\_\_ [Write the number between 1-7]  
 888888=Do NOT know  
 988888=No response

**ELbk8c.** To which level (between 1-7 with 1 meaning "NOT AT ALL" and 7 meaning "I COMPLETELY AGREE") do you agree with the following statement::

When I do not pay my property taxes, I feel shame or another negative feeling.

# \_\_\_\_\_ [Write the number between 1-7]  
 888888=Do NOT know  
 988888=No response



**ELbk9.** What would increase your willingness to pay property taxes?

**[Do NOT read these answers, indicate all that are mentioned]**

1. If I had work/money, I would pay.
2. If I knew that more people paid.
3. If I received more services from the local government.
4. If I trusted the local government more. / If politicians were less corrupt.
5. If my candidate had won the mayor's election.
6. If the streets were cleaned more often.
7. If trash was removed more often.
8. If my religious leader paid.
9. If there were legal consequences.
10. If there were other consequences (not just legal) if you didn't pay.
11. If I knew someone who lost his house because he didn't pay.
12. If another person knew that I wasn't paying.
13. If payment history was more public.
14. If there were more schools built.
15. If traffic improved.
16. Other \_\_\_\_\_

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**ELbk10a.** Have you received before a letter from the Mayor's office informing you of your obligation to pay property taxes ?

(1) Yes

(2) No

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**ELbk10b.** How many times have you received a letter from the Mayor's office to inform you of your obligation to pay property taxes?

# \_\_\_\_\_ letters

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**ELbk10c. [I greater than zero]** When was the last time that you received a letter from the Mayor's office informing you of your obligation to pay property taxes ?

\_\_\_\_\_ Month

(888888) Do NOT know **[DO NOT READ ANSWER]**

(988888) No response **[DO NOT READ ANSWER]**

\_\_\_\_\_ Ane

(888888) Do NOT know **[DO NOT READ ANSWER]**

(988888) No response **[DO NOT READ ANSWER]**

**ELbk11.** In general, how much money would someone need to pay on the side (as a bribe) to avoid having to pay property taxes completely for a house like yours?

\_\_\_\_\_ \$/HTG/Haitian Dollars **[DO NOT READ: Always circle or indicate the currency the respondent uses for his/her response.]**

**Now let's talk about trust, representation and the political system:**

**IT1.** Let's talk about the people in your neighborhood, would you say that you have a lot of trust, trust, not a lot of trust or no trust at all in the people in your area...?

(1) A lot of trust

(2) trust

(3) Not a lot of trust

(4) No trust at all

(888888) Do NOT know [**DO NOT READ THIS ANSWER**]

(988888) No response [**DO NOT READ THIS ANSWER**]

**Bbk1.** How much (on a scale from 1-7 with 1 meaning "NOT AT ALL" and 7 meaning "A LOT") do you trust the people that live in your neighborhood??

\_\_\_\_\_ [Write the number between 1-7]

888888=Do NOT know

988888=No response

**B18.** How much (on a scale from 1-7 with 1 meaning "NOT AT ALL" and 7 meaning "A LOT") do you trust the national police (PNH)?

\_\_\_\_\_ [Write the number between 1-7]

888888=Do NOT know

988888=No response

**B32.** How much (on a scale from 1-7 with 1 meaning "NOT AT ALL" and 7 meaning "A LOT") do you trust the mayor?

\_\_\_\_\_ [Write the number between 1-7]

888888=Do NOT know

988888=No response

**B13.** How much (on a scale from 1-7 with 1 meaning "NOT AT ALL" and 7 meaning "A LOT") do you trust parliament?

\_\_\_\_\_ [Write the number between 1-7]

888888=Do NOT know

988888=No response

**B21A.** How much (on a scale from 1-7 with 1 meaning "NOT AT ALL" and 7 meaning "A LOT") do you trust the President?

\_\_\_\_\_ [Write the number between 1-7]

888888=Do NOT know

988888=No response

**B47A.** How much (on a scale from 1-7 with 1 meaning "NOT AT ALL" and 7 meaning "A LOT") do you trust national elections?

\_\_\_\_\_ [Write the number between 1-7]

888888=Do NOT know

988888=No response

**PN4.** Generally, with regards to how democracy functions in Haiti, would you say that you are:

(1) Very satisfied

(2) A little satisfied

(3) A little unsatisfied

(4) Not at all satisfied

(888888) Do NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

**VB2.** Did you vote in the first round of the Presidential election in 2016?

**(1)** Yes, he/she voted **[continue]**

**(2)** No, he/she did not vote **[go to VB4NEW2]**

(888888) Does NOT know **[DO NOT READ THIS ANSWER]** **[go to VBbk2]**

(988888) No response **[PA LI REPONS]** **[go to VBbk2]**

**VB3N.** For whom did you vote in the first round of the presidential election in 2016?

**[Do NOT read these answers, select only one response. After responding, go to VBbk2]**

(00) No one (blank ballot)

(97) No one (voided ballot)

(2201) Jovenel Moïse (Pati Ayisyen Tèt Kale - PHTK)

(2202) Jude Célestin (Ligue Alternative pour le Progrès et l'Emancipation Haitienne)

(2203) Jean-Charles Moïse (Platfom Pitit Desalin)

(2204) Maryse Narcisse (Fanmi Lavalas)

(2205) Eric Jean Baptiste (Mouvement Action Socialiste)

(2206) Jean Henry Céant (Renmen Ayiti)

(2277) Another candidate

(888888) Does NOT know **[DO NOT READ THIS ANSWER]**

(988888) No response **[DO NOT READ THIS ANSWER]**

(999999) NOT applicable (Li pat vote) **[DO NOT READ THIS ANSWER]**

**VB4NEW2. [For people who did not vote.]**

Why didn't you vote in the last presidential election?

**[Do NOT read these answers, indicate all that are mentioned]**

- (1) I did NOT trust the candidates
- (9) I did NOT trust what they wanted to do for the country.
- (2) I did NOT like any of the candidates/political parties
- (3) I did NOT trust the election, I did NOT trust the officials that organized the election
- (4) I do NOT trust democracy
- (5) I had a problem with my registration (bureaucratic problem)
- (6) I had a problem with my age (too young, too old)
- (7) I was in another area, I was too far from my home.
- (8) I am NOT interested in politics
- (10) I did NOT know there were elections
- (11) I did NOT have an ID card
- (12) My voting center was too far away
- (77) Other reason
- (888888) Does NOT know **[DO NOT READ THIS ANSWER]**
- (988888) No response **[DO NOT READ THIS ANSWER]**

**VBbk2. Did you vote in the last election for mayor in 2015?**

**(1)** Yes, he/she voted **[continue]**

**(2)** No, he/she did NOT vote **[go to VB11.]**

(888888) Does NOT know **[DO NOT READ THIS ANSWER]** **[go to VB11.]**

(988888) No response **[DO NOT READ THIS ANSWER]** **[go to VB11.]**

**VB3N. Who did you vote for for mayor in 2015?**

**[Do NOT read these answers, select only one response]**

- (00) No one (blank ballot)
- (97) No one (voided ballot)
- (69) Jude Edouard, Pierre (VERITE)
- (23) Jean Berthaud, Blanchard (MAS)
- (54) Iliophene, Fabien (FANMI LAVALAS)
- (5) Yves, Blanchard (Pati Ayisyen Tèt Kale - PHTK)
- (28) Felix Bertin, Jacques (PITIT DESSALINES)
- (10) Frantz Richard, Francois (BOUCLIER)
- (2277) Another candidate
- (888888) Does NOT know **[DO NOT READ THIS ANSWER]**
- (988888) No response **[DO NOT READ THIS ANSWER]**
- (999999) NOT applicable (He/she did NOT vote) **[DO NOT READ THIS ANSWER]**

**VB11.** Which political party do you most support?

**[Do NOT read these answers, select only one response]**

- (2201) Fwon Lespwa
- (2202) RDNP
- (2203) Respè
- (2204) Repons Peyizan
- (2205) MPH
- (2206) Fusion des Sociaux-Démocrates Haïtienne
- (2207) Oganizasyon Pèp Kap Litè
- (2208) Alyans/Alliance Démocratique
- (2209) Renmen Ayiti (2210) Ansanm nou Fo
- (2211) Lavalas
- (2212) Unité
- (2213) PHTK (Pati Tèt Kale) (2214) Pitit Desalinn
- (2215) LA PEH
- (2216) Verite
- (2299) Union des patriotes pour l'avancement natiional (UPAN)
- (2298) Ayiti Ann Aksyon (AAA)
- (2277) Another political party
- (888888) Does NOT know **[DO NOT READ THIS ANSWER]**
- (988888) No response **[DO NOT READ THIS ANSWER]**
- (999999) NOT applicable **[DO NOT READ THIS ANSWER]**

**VB20bk1.** I the next election for mayor was this week, what would you do ?

**[Read the possible answers]**

- (1) You would NOT vote
- (2) You vote for the candidate or the party currently occupying the mayor's office
- (3) You vote for a candidate or a party NOT currently occupying the mayor's office
- (4) You would vote but you would leave your ballot blank or void your ballot.
- (5) (888888) Does NOT know **[DO NOT READ THIS ANSWER]**
- (988888) No response **[DO NOT READ THIS ANSWER]**

**EXC2.** Has a police officer asked you for a bribe in the last 12 months?

- (1) Yes
- (2) No
- (888888) Do NOT know **[DO NOT READ THIS ANSWER]**
- (988888) No response **[DO NOT READ THIS ANSWER]**

**EXCbk6.** Has a local government official asked you for a bribe in the last 12 months?

- (1) Yes
- (2) No
- (888888) Do NOT know **[DO NOT READ THIS ANSWER]**
- (988888) No response **[DO NOT READ THIS ANSWER]**

**EXC6.** Has a national government official asked you for a bribe in the last 12 months??

- (3) Wi
- (4) Non
- (888888) Do NOT know [**DO NOT READ THIS ANSWER**]
- (988888) No response [**DO NOT READ THIS ANSWER**]

**EXC18.** Do you believe, given how things are, that there are times when it is ok to pay a bribe (give something under the table)?

- (1) Yes
- (2) No
- (888888) Do NOT know [**DO NOT READ THIS ANSWER**]
- (988888) No response [**DO NOT READ THIS ANSWER**]

**Finally, let's talk about your house:**

Do you have these appliances and other items in your house?:

**[Read the possible answers]**

<b>R3.</b> Refrigerator	(0) No	(1) Yes			(888888) Does NOT know <b>[DO NOT READ THIS ANSWER]</b>	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>
<b>R4.</b> Fixed telephone	(0) No	(1) Yes			(888888) Does NOT know <b>[DO NOT READ THIS ANSWER]</b>	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>
<b>R4A.</b> Mobile telephone, cell phone (accept smart phone)	(0) No	(1) one	(2) two	(3) three or more than three	(888888) Does NOT know <b>[DO NOT READ THIS ANSWER]</b>	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>
<b>R5.</b> Car or truck <b>[If people say yes without indicating how many mark (1)]</b>	(0) No	(1) Yes			(888888) Does NOT know <b>[DO NOT READ THIS ANSWER]</b>	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>
<b>R6.</b> Washing machine (for clothes)	(0) No	(1) Yes			(888888) Does NOT know <b>[DO NOT READ THIS ANSWER]</b>	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>
<b>R7.</b> Microwave	(0) No	(1) Yes			(888888) Does NOT know <b>[DO NOT READ THIS ANSWER]</b>	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>
<b>R8.</b> Motorcycle	(0) No	(1) Yes			(888888) Does NOT know <b>[DO NOT READ THIS ANSWER]</b>	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>
<b>R12.</b> (Piped) potable water inside the house	(0) No	(1) Yes			(888888) Does NOT know <b>[DO NOT READ THIS ANSWER]</b>	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>
<b>DHS4.</b> Toilet or latrine inside the house <b>[If people say yes without indicating how many mark (1)]</b>	(0) No	(1) one	(2) two	(3) three or more than three	(888888) Does NOT know <b>[DO NOT READ THIS ANSWER]</b>	(988888) No response <b>[DO NOT READ THIS ANSWER]</b>

<b>DHS3.</b> Do you share your toilet	(0) No	(1) Yes			(888888) Does NOT know	(988888) No response
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with another household ?					[DO NOT READ THIS ANSWER]	[DO NOT READ THIS ANSWER]
<b>R14.</b> Shower inside the house	(0) No	(1) Yes			(888888) Do NOT know [DO NOT READ THIS ANSWER]	(988888) No response [DO NOT READ THIS ANSWER]
<b>DHS2.</b> Bedroom [If people say yes without indicating how many mark (1)]	(0) No	(1) one	(2) two	(3) Twa or plis pase twa	(888888) Does NOT know [DO NOT READ THIS ANSWER]	(988888) No response [DO NOT READ THIS ANSWER]
<b>DHS1.</b> Radio	(0) No	(1) one	(2) two	(3) Twa or plis pase twa	(888888) Does NOT know [DO NOT READ THIS ANSWER]	(988888) No response [DO NOT READ THIS ANSWER]
<b>R15.</b> Computer (accept tablet) [If people say yes without indicating how many mark (1)]	(0) No	(1) one	(2) two	(3) Twa or plis pase twa	(888888) Does NOT know [DO NOT READ THIS ANSWER]	(988888) No response [DO NOT READ THIS ANSWER]
<b>R18.</b> Internet access in the house (Do NOT include connection just on telephone or tablet)	(0) No	(1) Yes			(888888) Does NOT know [DO NOT READ THIS ANSWER]	(988888) No response [DO NOT READ THIS ANSWER]
<b>R1.</b> Television [If people say yes without indicating how many mark (1)]	(0) No	(1) one	(2) two	(3) Twa or plis pase twa	(888888) Does NOT know [DO NOT READ THIS ANSWER]	(988888) No response [DO NOT READ THIS ANSWER]
<b>R16.</b> Flatscreen television [If people say yes without indicating how many mark (1)]	(0) No	(1) one	(2) two	(3) three or more than three	(888888) Does NOT know [DO NOT READ THIS ANSWER]	(988888) No response [DO NOT READ THIS ANSWER]



**DHSS.** How (with what type of fuel) do you cook food in your home ?

**[Do NOT read these answers, select only one response]**

- 1 electricity
  - 2 LPG – liquid propane gas
  - 3 natrual gas
  - 5 kerosine
  - 7 charcoal
  - 8 wood
  - 9 straw, shrubs, grass
  - 10 agricultural waste (corn cobs, stalks)
  - 11 animal dung
  - 95 Do NOT cook food in the house
  - 96 Other \_\_\_\_\_
- (888888) Does NOT know **[DO NOT READ THIS ANSWER]**  
(988888) No response **[DO NOT READ THIS ANSWER]**

**Closing :** Those are all of the questions that I have for you today. Thank you for your time. I will send the 50HTG to your telephone as soon as we complete this call. Thank you again for your time today.

[Send credit as soon as the respondent ends the call.]

**KON. [Confirm 50HTG credit sent ]**

- (1) Yes
- (2) No

**TI. [Hour and minute that the survey ends:]** \_\_\_\_\_ :

**INTID. [ID number of surveyor]:** \_\_\_\_\_

*I declare this survey was completed with the person assigned.*

*Surveyor's signature* \_\_\_\_\_ *Date* \_\_\_\_/\_\_\_\_/\_\_\_\_

*Last name* \_\_\_\_\_ *First name* \_\_\_\_\_

*Supervisor's signature* \_\_\_\_\_ *Date* \_\_\_\_/\_\_\_\_/\_\_\_\_

*Last name* \_\_\_\_\_ *First name* \_\_\_\_\_

*Comments:* \_\_\_\_\_

: \_\_\_\_\_

: \_\_\_\_\_

: \_\_\_\_\_

: \_\_\_\_\_

: \_\_\_\_\_

: \_\_\_\_\_

*[NOT for PDA/Android] Signature of person who enters data* \_\_\_\_\_

*[NOT for PDA/Android] Signature of person who verifies data* \_\_\_\_\_

## Quantitative Questionnaire - KREYOL

Hour and minute that the survey starts: \_\_\_\_: \_\_\_\_

FECHA. Date of the survey Day: \_\_ Month: \_\_\_\_\_ Year: 2018

[Note for surveyor : Before each question, there is a code in bold letters, for example: "MUNICIPIO", "HAISEKSYON", or "SD2bk1". Do not read this code. It is simply a tool for data entry. Also, do not the texts written between brackets [... ] ]

**Opening:** Hello, my name is \_\_\_\_\_, and I'm calling you to ask a few questions about community organizations, social services and citizenship in Haiti.

If you complete all of the questions, I will send you phone credit worth 50 HTG as an expression of gratitude for your time.

The survey will take approximately 20 minutes. You can pass on any question and you can end the survey at any time.

Do you agree to proceed?

**[If No] :** Thank you for your time. We wish you a good day.  
**[Wait until the other person ends the call before you hang.]**

**[If Yes]**

To give you a bit of additional information, I'm calling on behalf of Benjamin KRAUSE, a researcher at the University of California BERKELEY in the United States. We will work to assure your confidentiality. All responses will be saved in an anonymized database protected by a password and only Mr. Krause will have access to the information. As with all research there remains the possibility that your confidentiality could be compromised, however we are taking precautions to minimize this risk.

There is no direct benefit to you from taking part in this study. However, we hope that the information gained from the study will help inform future efforts to improve government services and responsiveness.

Do you consent to participate in this study? That is to say, are you OK to continue with this survey? Please respond yes or no.

**[If no] :** Thank you for your time. Who sincerely wish you good day.  
**[Stay on the line until the persn on the other end hangs up before going to your next call.]**

[Si Wi] : Ekselan. Mèsi.

**Pou konmanse ba'm mande ou kèk enfòmasyon jeneral:**

**MUNICIPIO.** Nan ki komin ou rete ?

**[Pa Li repons yo, ekri yon sèl repons]**

- (1) Carrefour
- (2) Port-au-Prince
- (3) Cite Soleil
- (4) Delmas
- (5) Gressier
- (6) Kenscoff
- (7) Petionville
- (8) Tabarre
- (9) Lot \_\_\_\_\_
- (10) (888888) Pa konnen **[PA LI REPONS LA]**
- (11) (988888) Pa reponn **[PA LI REPONS LA]**

**HAISEKSYON.** Nan ki Seksyon Kominal ou rete ?

**[Pa Li repons yo, ekri yon sèl repons]**

- (1) Morne Chandelle
- (2) Platon Dufréné
- (3) Taïfer
- (4) Procy
- (5) Coupeau
- (6) Bouvier
- (7) Laval
- (8) Berly
- (9) Bizoton
- (10) Thor
- (11) Rivière Froide
- (12) Malanga
- (13) Corail Thor
- (14) Lot nan Carrefour \_\_\_\_\_
- (15) Lot deyò Carrefour \_\_\_\_\_

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

**HAISEC.** Nan ki Katye ou rete ?

**[Pa Li repons yo, ekri yon sèl repons]**

[1] "Arcachon/Mon-repos"	[22] "Grand Caye/Marre Bizoton"	[43] "Morne-Tesserot"
[2] "Bas Bizoton"	[23] "Grand Ravine"	[44] "Nan Citron - Village Michel"
[3] "Bas Thorland"	[24] "Haut Chaud-Eau"	[45] "Paloma/Canne-à-Sucre"
[4] "Bertin"	[25] "Haut La Grenade"	[46] "Pandjamen/Sous-Rails"
[5] "Bizoton 51"	[26] "Haut Mariani"	[47] "Petite Saline - Chiwawa"
[6] "Bizoton/Bergamot"	[27] "Haut Rivière-Froide"	[48] "Phare Lamentin"
[7] "Bizoton/Mont-Carmel-Diquini"	[28] "Haut Thor"	[49] "Ravine Grand-Fond"
[8] "Bois Dioute - Ti Kajou"	[29] "Immaculée Conception"	[50] "Ravine Ti-Source"
[9] "Brochette"	[30] "Jérusalem"	[51] "Rivière Froide"
[10] "Calebacher"	[31] "La Belair"	[52] "Rivière Froide (Kay Frè)"
[11] "Campus Diquini"	[32] "La Grenade"	[53] "Sapotille"
[12] "Centre Sportif de Carrefour"	[33] "Lajoie"	[54] "Source Corrossol"
[13] "Changement"	[34] "Laramé"	[55] "St-Charles - Lamentin"
[14] "Chaud-Eau"	[35] "Le Lambi"	[56] "Terrain Ti Carne"
[15] "Cocover/Bas Rails"	[36] "Littoral Est /Gressier"	[57] "Ti Source"
[16] "Côte-Plage"	[37] "Mahotièr"	[58] "Village Petit Paradis"
[17] "Degand"	[38] "Malcom"	[59] "Waney"
[18] "Do Karyann"	[39] "Mariani Est"	[60] Lot nan Carrefour _____
[19] "Domaine Idéal"	[40] "Mariani Ouest"	[61] Lot deyò Carrefour _____
[20] "Fontamara Est"	[41] "Marine Haitienne"	(888888) Pa konnen <b>[PA LI REPONS LA]</b>
[21] "Gaston-Magron"	[42] "Morne Madan Garnot"	(988888) Pa reponn <b>[PA LI REPONS LA]</b>

**UR.** Eske ou rete nan yon zon ki se (1) Lavil oubyen (2) Seksyon kominal

**Q1. [Mande sèlman si ou pa sèten]** Seks: (1) Gason (2) Fanm

**Q2.** Ki laj ou?

# \_\_\_\_\_ Ane **[LAJ LI PA KA PI PITI PASE 18 LANE]**

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

**QBK2.** Ak tèt ou ladan, konbyen moun kap viv nan kay ou rete a?

# \_\_\_\_\_ moun

Pou rès nan sondaj la, lè mwen mande sou, « *ou ak tout manm kay 'w ansanm* » m'ap refere a moun sa yo ou te jis konte tout ansanm.

**LS3.** Eske w satisfè ak lavi w? Eske w ta di ke w ...[Li repons yo]

(1) Tre satisfè

(2) On ti jan satisfè

(3) On ti jan pa satisfè

(4) Pa satisfè di tou

888888=Pa konnen [PA LI REPONS LA]

988888=Pa reponn [PA LI REPONS LA]

### **An nou pale de komin nan :**

**HAIMUNI8.** Tanpri, eske w kapab di mwen ki sa ki ta dwe **3 gwo priyorite** ki pi enpòtan pou gouvènman lokal la nan pwosesis amelyorasyon nan kominote w la.

**[PA LI LIS OPSYON YO, endike von nan chak kolòn]**

	<b>Premye repons HAIMUNI8A</b>	<b>Dezyèm repons HAIMUNI8B</b>	<b>Twazèm repons HAIMUNI8C</b>
Netwaye lari epi espas piblik yo	66	66	66
Retire fatra	67	67	67
Konstwi Lekòl	12	12	12
Sekirite nan katye	2	2	2
Kreye jòb	3	3	3
Konstwi wout	4	4	4
Dlo potab	5	5	5
Elèktrisite ak enèji renouvlab	6	6	6
Lasante	7	7	7
Lojman	8	8	8
Rebati anvivònman	9	9	9
Ranfòse kapasite leta santral	13	13	13
Ranfòse kapasite lameri oubyen gouvènman lokal	11	11	11
Lòt priyorite	77	77	77
Pa konnen [PA LI REPONS LA]	888888	888888	888888
Pa reponn [PA LI REPONS LA]	988888	988888	988888

## A4. Daprè ou menm, ki saw panse ki pi gwo pwoblèm komin nan ?

**[Pa Li repons yo, ekri yon sèl repons]**

Anlèvman, kidnapin	31	Ogmantasyon popilasyon an	20
Chomaj/Pa gen travay	3	Pa gen elektrisite (Kouwan EDH)	24
Debwasman/Pwoblèm sou kesyon anvironman	10	Pa gen kay	55
Deplasman pa lafòs	32	Pa gen kredi	9
Dèt ak lòt peyi	26	Pa gen ledikasyon, move kalite ledikasyon	21
Diskriminasyon	25	Pa gen sekirite	27
Dwòg	11	Pa gen tè pou plante	7
Enpinite	61	Pa genyen dlo	19
Gang, chimè	14	Politisyen yo	59
Koripsyon	13	Povrete/lamizè	4
Krim, ensekirite, vyolans	5	Pran swen moun ki blese nan tranbleman tè a	63
Lagè	30	Pwoblèm ekonomik, kriz ekonomik	1
Lagè kont teroris	17	Pwoblèm transpò	60
Lavi chè, enflasyon	2	Pwotestasyon popilè (manifestasyon, grèv, e latrèye.)	6
Linegalite sosyal	58	Rekonstwi peyi a apre tranbleman tè a	62
Malnitrisyon	23	Trafik dwòg	1
Manke sèvis lasante	22	Vyolasyon dwa moun	56
Migrasyon	16	Zak teroris	33
Move gouvènman, absans leta	15	Netwaye lari epi espas piblik yo	66
Move wout	18		
Retire fatra	67		
		Lòt bagay	70
Pa reponn <b>[PA LI REPONS LA]</b>	888888	Pa konnen <b>[PA LI REPONS LA]</b>	988888

CP. Kounya, mwen pral bay yon lis gwoup ak òganizasyon. Tranpri, eske w kapab di mwen chak ki lè ou te asiste nan reyinyon òganizasyon sa yo: youn fwa pa semèn, youn oswa de fwa pa mwa, youn oswa de fwa pa lane ou byen

ou pa janm fè sa. [Repete “youn fwa pa semen”, “Youn oswa de fwa pa mwa”, “youn oswa de fwa pa lane”, oswa “pa janm fè sa” pou ede moun nan reponn keksyon yo]

	Youn fwa pa semen	Youn oswa de fwa pa mwa	Youn oswa de fwa pa lane	Pa janm fè sa	Pa konnen [PA LI REPONS LA]	Pa reponn [PA LI REPONS LA]	Pa aplike [PA LI REPONS LA]
<b>CP6.</b> Reyinyon gwoup legliz ou byen òganizasyon relijiye? Eske w te asiste...	1	2	3	4	888888	9888888	999999
<b>CP8.</b> Reyinyon komite amelyorasyon pou kominote a (Komite Katye oswa asosyasyon Kominotè)? Eske w te asiste...	1	2	3	4	888888	9888888	999999
<b>CPbk1.</b> Lòt reyinyon oganizasyon lokal, fondasyon, asosyasyon, ONG lokal, komite, gwoup, team, oubyenn yon « tet ansanm »? Eske w te asiste...	1	2	3	4	888888	9888888	999999
<b>CP13.</b> Reyinyon pati politik ou byen mouvman politik? Eske w te asiste...	1	2	3	4	888888	9888888	999999
<b>CP20.</b> [Sèlman pou fanm] Reyinyon gwoup oswa òganizasyon fanm oswa fanm lakay? Eske w te asiste...	1	2	3	4	888888	9888888	999999

**NP1.** Eske w te patisipe nan yon asanble kominal oswa reyinyon majistra yo te fè nan 12 mwa ki fèk sot pase yo?

(1) Wi

(2) Non

(888888) Pa konnen [PA LI REPONS LA]

(988888) Pa reponn [PA LI REPONS LA]

**SD2bk0.** Nan katye ou, èske ou satisfè nèt, satisfè, pa satisfè oswa pa satisfè di tou ak travay otorite lokal fe epi sevis li bay ?

(1) Satisfè nèt

(2) Satisfè

(3) Pa satisfè

(4) Pa satisfè di tou

888888=Pa konnen [PA LI REPONS LA]

988888=Pa reponn [PA LI REPONS LA]

(999999) Pa aplike (Pa itilize sèvis la) [PA LI REPONS LA]

**SD2bk1.** Nan katye ou, èske ou satisfè nèt, satisfè, pa satisfè oswa pa satisfè di tou ak retire fatra?

(1) Satisfè nèt

(2) Satisfè



- (3) Pa satisfè  
 (4) Pa satisfè di tou  
 888888=Pa konnen [PA LI REPONS LA]  
 988888=Pa reponn [PA LI REPONS LA]  
 (999999) Pa aplike (Pa itilize sèvis la) [PA LI REPONS LA]

**SD2bk2.** Nan katye ou, èske ou satisfè nèt, satisfè, pa satisfè oswa pa satisfè di tou ak netwayaj nan lari yo ak espas piblik yo?

- (1) Trè satisfè  
 (2) On ti jan satisfè  
 (3) On ti jan pa satisfè  
 (4) Pa satisfè di tou  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**SD2bk3.** Nan denye mwa, konbyen fwa kamyon biwo majistra a te pase lari ou pou kolekte fatra a?

# \_\_\_\_\_ fwa  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**SD2bk4a.** Panse de tout mwayen ke vwazen nan kominote ou mete tet ansanm pou reyalize yon objektif ansanm swa ou rele travay sa a « kombat », « kove », « douvanjou », « kolon », « tet ansanm », oubyenn yon lot non, nan denye mwa, konbyen fwa kominote ou te realize yon travay konsa ?

# \_\_\_\_\_ fwa  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**SD2bk4b. [Si repons pi gwo ke zero]** Le ou panse de fwa sa yo nan denye mwa, konbyen ed tan ou menm oubyenn manm kay'w te patisipe an total ?

# \_\_\_\_\_ ed tan  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**SD2bk5.** Le ou panse de fwa sa yo nan denye mwa, konbyen kob tout manm kay'w ansanm te bay an total ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**SD2bk6.** Le ou panse de fwa sa yo nan denye mwa, si kay'w te bay materyel oubyenn manje pou travay la, ki sa yo vo an total ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**CPbk7.** Konbyen fwa ou menm oubyen manm kay'w te patisipe nan yon manifestyasyon nan dènye mwa?

# \_\_\_\_\_ fwa  
(888888) Pa konnen [PA LI REPONS LA]  
(988888) Pa reponn [PA LI REPONS LA]

**CPbk8.** Konbyen jou tout ansanm ou menm oubyen manm kay'w te travay kòm yon "Cantonnier" pou majistra-a oswa te anboche nan pwogram travay ke biwo majistra-a bay nan dènye mwa?

# \_\_\_\_\_ jou  
(888888) Pa konnen [PA LI REPONS LA]  
(988888) Pa reponn [PA LI REPONS LA]

### **Kounyea ann pale de revenu ak depans manm kay'w :**

**REVbkALL.** Sou revenu kay la:

Le nou pale de tout mwayen ak sous kob yo ansanm ki gen ladan

- kob touche,
- kob fe nan ti biznis,
- kob resevwa kom don,
- kob resevwa kom pre,
- kob genyen nan lotri oubyenn nan lot jwet aza,
- tout lot mwayen pou jwenn kob ;

**nan denye mwa**, konbyen kob tout manm kay'w ansanm te genyen, sa vle di, nan denye mwa, ki kantite revenu kay la ? \_\_\_\_\_ \$/HTG/Haitian Dollars

[PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]

Sou depans kay la:

**DEPbk1.** Nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou manje ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
(888888) Pa konnen [PA LI REPONS LA]  
(988888) Pa reponn [PA LI REPONS LA]

**DEPbk2.** Nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou transpo?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
(888888) Pa konnen [PA LI REPONS LA]  
(988888) Pa reponn [PA LI REPONS LA]

**DEPbk3.** Nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou telefon?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
(888888) Pa konnen [PA LI REPONS LA]  
(988888) Pa reponn [PA LI REPONS LA]

**DEPbk4.** Nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou jete fatra ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPbk5.** Nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou retire fatra nan katye a ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

Nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou ladim ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPbk6.** Nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou patisipasyon nan lot sant relijiye ?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPbk7.** Nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou patisipasyon nan oganizasyon lokal, fondasyon, asosyasyon, ONG lokal, komite, gwoup, team, oubyenn yon « tet ansanm »?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPbk8.** Nan denye mwa, si ou menm oubyenn manm kay'w te ale mande yon sevis nan biwo polis oubyenn te gen yon entèraksyon ak polis epi te oblije peye kob sou kote, konbyen kob nou te bay sou kote? Sa vle di, nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou bay kob sou kote nan polis yo?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPbk9.** Nan denye mwa, si ou menm oubyenn manm kay'w te ale mande yon sevis nan biwo otorite lokal oubyenn te gen yon entèraksyon ak manm ekip otorite lokal epi te oblije peye kob sou kote, konbyen kob nou te bay sou kote? Sa vle di, nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou bay kob sou kote nan manm otorite lokal yo?

\_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPbk10.** Nan denye mwa, si ou menm oubyenn manm kay'w te ale mande yon sevis nan biwo leta oubyenn te gen yon entèraksyon ak manm ekip leta epi te oblije peye kob sou kote, konbyen kob nou te bay sou kote? Sa vle di, nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse pou bay kob sou kote nan manm leta yo?

\_\_\_\_\_\$/HTG/Haitian Dollars **[PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]**  
 (888888) Pa konnen **[PA LI REPONS LA]**  
 (988888) Pa reponn **[PA LI REPONS LA]**

**DEPbkALL.** Finalman, le nou pale de tout sa ou fè ak kob yo ansanm ki gen ladan kob depanse pou

- tout bagay nou fek mansyone
  - o manje
  - o transpo
  - o telefon
  - o jete fatra
  - o retire fatra nan kay a
  - o kob te bay pou sipote travay kominote
  - o ladim
  - o patispasyon nan lot sant relijiye
  - o patispasyon nan lot oganizasyon lokal
  - o wouleman pou polis ak lòt otorite yo
- Ak tout lot mwayen pou depense kob
  - o rad
  - o kouran
  - o sante
  - o lekol
  - o loteri ak jwet aza
  - o biznis
  - o lwazi

nan denye mwa, konbyen kob tout manm kay'w ansanm te depanse, sa vle di, nan denye mwa, ki kantite depans kay la ?

\_\_\_\_\_\$/HTG/Haitian Dollars **[PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]**  
 (888888) Pa konnen **[PA LI REPONS LA]**  
 (988888) Pa reponn **[PA LI REPONS LA]**

**DEPAbk1a.** Nan denye ane, eske ou te peye pou kay la ?

- (1) Wi
- (2) Non

(888888) Pa konnen **[PA LI REPONS LA]**  
 (988888) Pa reponn **[PA LI REPONS LA]**

**DEPAbk1b. [Si wi]** Nan ki mwa ou te peye kay la ?

\_\_\_\_\_ mwa  
 (888888) Pa konnen **[PA LI REPONS LA]**  
 (988888) Pa reponn **[PA LI REPONS LA]**

**DEPAbk1c. [Si wi]** Konbyen kob ou te depanse pou kay la ?

\_\_\_\_\_\$/HTG/Haitian Dollars **[PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]**  
 (888888) Pa konnen **[PA LI REPONS LA]**  
 (988888) Pa reponn **[PA LI REPONS LA]**

**DEPAbk2a.** Nan denye ane, eske ou te peye empo lokatif ?

- (1) Wi  
 (2) Non  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPAbk2b. [Si wi]** Nan ki mwa ou te peye empo lokatif ?

- # \_\_\_\_\_ mwa  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPAbk2c. [Si wi]** Konbyen kob ou te depanse pou empo lokatif la ?

- \_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPAbk3a.** Nan denye ane, eske ou te peye patant ?

- (1) Wi  
 (2) Non  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPAbk3b. [Si wi]** Nan ki mwa ou te peye patant ?

- # \_\_\_\_\_ mwa  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPAbk3c. [Si wi]** Konbyen kob ou te depanse pou patant la ?

- \_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPAbk4a.** Eske ou genyen lot depans anuel ?

- (1) Wi  
 (2) Non  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**DEPAbk4b. [Si wi]** Nan denye ane, ki kantite an total ou te depanse pou tout lot yo ansanm ?

- \_\_\_\_\_ \$/HTG/Haitian Dollars [PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**Kounyea ann pale de empo lokatif :****ELbk1.** Ki jan kay la dwe ye pou met kay la dwe peye empo lokatif ?

(Fok kay la . . . )

**[PA LI repons yo. Pa aspete « fini ». Si repondan an di « fini », reponn ak kesyon, « ki sa vle di ‘fini’ ?»]**

1. pentire
2. abite
3. konstri ak blok
4. konstri ak bwa
5. gen twati
6. pa bezwen gen kay la, si ou gen teren, fok ou peye empo lokatif

(888888) Pa konnen **[PA LI REPONS LA]**(988888) Pa reponn **[PA LI REPONS LA]****ELbk2.** Ki se to empo lokatif aktyèl la pou yon kay tankou kay ou?

\_\_\_\_\_ %

(888888) Pa konnen **[PA LI REPONS LA]**(988888) Pa reponn **[PA LI REPONS LA]****ELbk3a.** Eske ou panse empo lokatif jis ?

(1) Wi

(2) Non

(888888) Pa konnen **[PA LI REPONS LA]**(988888) Pa reponn **[PA LI REPONS LA]**

**ELbk3b. [Si wi],** poukisa wi?

**[Pa Li repons yo, endike tout sa yo mansyone]**

1. Se yon obligasyon
2. Lalwa deside li jis
3. Ede kominote avanse
4. Gen anpil bezwen nan kominote a ak empo lokatif se mwayen pricipal pou fianse yo.
5. Krete travay (job yo)
6. Nou genyen bon sekirite nan kominote a
7. Konstri mache a
8. Amelyore jesyon sant dechaj la
9. Konstri plas piblik
10. Konstri bibliotek kominal
11. Moun ki genyen dwe pataje avek lot ki pa genyen
12. Men anpil chay pa lou
13. Yon sel dwet pa manje kalalou
14. Lot \_\_\_\_\_

**ELbk3c. [Si non],** poukisa non ?

**[Pa Li repons yo, endike tout sa yo mansyone]**

1. Pa gen sevis
2. Pa gen ase sevis
3. To a se two wo
4. Pa gen travay/manke travay
5. Leta pa ede'm nan konstriksyon kay la
6. Mwen pa vote pou/pa sipote otorite lokal la
7. Lot \_\_\_\_\_

**ELbk3d. [Si non],** Nan opinyon ou, ki sa ki ta yon to jis pou empo lokatif pou yon kay tankou kay ou ?

\_\_\_\_\_ %

(888888) Pa konnen [PA LI REPONS LA]

(988888) Pa reponn [PA LI REPONS LA]

**ELbk4.** Ki konsekans legal yo genyen si ou pa peye empo lokatif?

**[Pa Li repons yo, endike tout sa yo mansyone]**

1. Anyen
2. Leta pran kay ou / retire ou.
3. Leta kraze kay ou.
4. Si majistra-a fè travay nan zòn nan epi li bezwen modifye oswa detwi kay ou nan pwosesis la, ou p'ap resevwa okenn ranbousman.
5. Ou ka gen difikilte avèk bank yo ak lòt enstitisyon prive ki mande prèv pwopriyetè ak taks peye.

(888888) Pa konnen [PA LI REPONS LA]

(988888) Pa reponn [PA LI REPONS LA]

**ELbk5a.** Ki pousantaj vwazen ou, sa vle di, konbyen sou 100, ou kwe te peye empo lokatif nan lane pase?

\_\_\_\_\_ %

(888888) Pa konnen [PA LI REPONS LA]

(988888) Pa reponn [PA LI REPONS LA]

**ELbk5b.** Ki pousantaj vwazen ou, sa vle di, konbyen sou 100, ou kwe te peye empo lokatif o mwens yon fwa nan denye 5 ane?

\_\_\_\_\_ %  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**ELbk5c.** Ki pousantaj vwazen ou, sa vle di, konbyen sou 100, ou kwe ta bezwen pou peye empo lokatif yo pou motive ou pou peye empo lokatif ou?

\_\_\_\_\_ %  
 (888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**ELbk6.** Ki sa ou panse de yon moun ki peye empo lokatif la?

**[Pa Li repons yo, endike tout sa yo mansyone]**

1. Bon sitwayen
2. Notab
3. Jenerè
4. Vle evite risk. / Gen ba tolerans pou risk
5. Kredul
6. Nayif
7. Malonèt
8. Egoyis
9. Vle pwoteje tèt li
10. Trayi
11. Lot \_\_\_\_\_

(888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**ELbk7a.** Èske ou ta vle vwazen ou yo konnen si ou peye empo lokatif la?

- (1) Wi
- (2) Non

(888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**ELbk7b.** [Si wi], poukisa wi ?

**[Pa Li repons yo, endike tout sa yo mansyone]**

1. Ankouraje lòt moun peye.
2. Mwen ta fyè pou kontribye.
3. Pou amelyore sitiyasyon sosyal mwen.
4. Paske li se bagay ki dwat fè.
5. Pou mete yon bon egzanp pou pitit mwen yo.
6. Li pa ta yon pwoblèm, se konsa mwen pa ta enkyete si lòt moun konnen.
7. Pou evite konfizyon ak otorite lokal yo pou yo pa retounen nan kolekte nan men'm yon dezyèm fwa.
8. Lot \_\_\_\_\_

(888888) Pa konnen [PA LI REPONS LA]  
 (988888) Pa reponn [PA LI REPONS LA]

**ELbk7c.** [Si non], poukisa non ?

**[Pa Li repons yo, endike tout sa yo mansyone]**

1. Pou kenbe sitiyasyon sosyal mwen
2. Pou evite lòt moun panse ke mwen presyon yo



3. Pou anpeche lòt moun nan konnen ke mwen gaspiye lajan mwen
4. Pou evite lòt moun panse ke mwen se gullible
5. Pou evite lòt moun panse ke mwen eseye jwenn devan yo
6. Pou pwoteje fanmi mwen
7. Paskè mwen ta pè de sa ki ka rive mwen oswa fanmi'm si lòt moun konnen.
8. Lot \_\_\_\_\_  
(888888) Pa konnen [PA LI REPONS LA]  
(988888) Pa reponn [PA LI REPONS LA]

**ELbk8a.** Nan ki nivo (ant 1-7 le 1 vle di « PA DAKO DI TOU » epi 7 vle di « MWEN KONPLETMAN DAKO») ou dako ak dekarasyon sa a :  
Si mwen peye empo lokatif mwen, mwen ta vle vwazen mwen yo konnen.

# \_\_\_\_\_ [Ekri nimewo 1-7]  
888888=Pa konnen  
988888=Pa reponn

**ELbk8b.** Nan ki nivo (ant 1-7 le 1 vle di « PA DAKO DI TOU » epi 7 vle di « MWEN KONPLETMAN DAKO») ou dako ak dekarasyon sa a :  
Si mwen pa peye empo lokatif mwen, mwen ta vle vwazen mwen yo konnen.

# \_\_\_\_\_ [Ekri nimewo 1-7]  
888888=Pa konnen  
988888=Pa reponn

**ELbk8c.** Nan ki nivo (ant 1-7 le 1 vle di « PA DAKO DI TOU » epi 7 vle di « MWEN KONPLETMAN DAKO») ou dako ak dekarasyon sa a :  
Le mwen pa peye empo lokatif mwen, mwen santi mwen wont oubyenn lòt santiman negatif.

# \_\_\_\_\_ [Ekri nimewo 1-7]  
888888=Pa konnen  
988888=Pa reponn

**ELbk9.** Ki sa ki ta ogmante volonte ou pou peye empo lokatif?

**[Pa Li repons yo, endike tout sa yo mansyone]**

1. Si mwen te gen yon travay / lajan, mwen ta peye.
2. Si mwen te konnen ke plis moun te peye.
3. Si mwen te resevwa plis sèvis nan men otorite lokal.
4. Si mwen te gen plis konfyans nan otorite lokal. / Si politisyen yo te mwens koripsyon.
5. Si kandida mwen te genyen biwo majistra-a.
6. Si lari yo te netwaye pi souvan.
7. Si fatra yo te ranmase pi souvan.
8. Si lidè relijye mwen peye.
9. Si te gen konsekans legal
10. Si te gen lòt konsekans (pa jis legal) pou pa peye.
11. Si mwen te konnen yon moun ki te pèdi kay la paske li pa't peye.
12. Si lòt moun te konnen ke mwen pa't peye.
13. Si istwa pèman te plis piblik.
14. Si gen plis lekòl yo te konstwi.
15. Si trafik amelyore.
16. Lot \_\_\_\_\_

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

**ELbk10a.** Eske ou te resevwa avan yon lèt biwo Mairie an pou fè ou konnen obligasyon ou pou peye empo lokatif?

(1) Wi

(2) Non

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

**ELbk10b.** Konbyen fwa ou te resevwa yon lèt biwo Mairie pou fè ou konnen obligasyon ou pou peye empo lokatif?

# \_\_\_\_\_ lèt yo

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

**ELbk10c. [Si pi gwo pase zero]** Kilè ki te dènye fwa ou te resevwa yon lèt biwo Mairie pou fè ou konnen obligasyon ou pou peye empo lokatif?

\_\_\_\_\_ Mwa

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

\_\_\_\_\_ Ane

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

**ELbk11.** An mwayèn, konbyen lajan yon moun ta dwe peye sou kote (fè woulman) pou evite peye empo lokatif konplè pou yon kay tankou kay pa'w?

\_\_\_\_\_ \$/HTG/Haitian Dollars **[PA LI: Toujou sèk oswa endike ki kalite lajan yo bay nan repons lan]**

**Kounyea ann pale de konfyans, reprezantasyon, ak sistèm politik la:**

**IT1.** Ann pale de moun nan katye kote w rete, eske w ka di m si w genyen anpil konfyans, konfyans, pa anpil konfyans, osinon ankenn konfyans nan moun nan zòn nan...?

(1) Anpil konfyans

(2) Konfyans

(3) Pa anpil konfyans

(4) Ankenn konfyans

(888888) Pa konnen [PA LI REPONS LA]

(988888) Pa reponn [PA LI REPONS LA]

**Bbk1.** Nan ki nivo (ant 1-7 le 1 vle di « PA DI TOU » epi 7 vle di « ANPIL») ou genyen konfyans nan moun nan katye kote w rete?

\_\_\_\_\_ [Ekri nimewo 1-7]

888888=Pa konnen

988888=Pa reponn

**B18.** Nan ki nivo (ant 1-7 le 1 vle di « PA DI TOU » epi 7 vle di « ANPIL») ou genyen konfyans nan polis nasyonal Ayisyen (PNH) an ?

\_\_\_\_\_ [Ekri nimewo 1-7]

888888=Pa konnen

988888=Pa reponn

**B32.** Nan ki nivo (ant 1-7 le 1 vle di « PA DI TOU » epi 7 vle di « ANPIL») ou genyen konfyans nan majistra an?

\_\_\_\_\_ [Ekri nimewo 1-7]

888888=Pa konnen

988888=Pa reponn

**B13.** Nan ki nivo (ant 1-7 le 1 vle di « PA DI TOU » epi 7 vle di « ANPIL») ou genyen konfyans nan palman an?

\_\_\_\_\_ [Ekri nimewo 1-7]

888888=Pa konnen

988888=Pa reponn

**B21A.** Nan ki nivo (ant 1-7 le 1 vle di « PA DI TOU » epi 7 vle di « ANPIL») ou genyen konfyans nan Prezidan an?

\_\_\_\_\_ [Ekri nimewo 1-7]

888888=Pa konnen

988888=Pa reponn

**B47A.** Nan ki nivo (ant 1-7 le 1 vle di « PA DI TOU » epi 7 vle di « ANPIL») ou genyen konfyans nan eleksyon nan peyi a?

\_\_\_\_\_ [Ekri nimewo 1-7]

888888=Pa konnen

988888=Pa reponn

**PN4.** Jeneralman, jan demokrasi ap fonksyone an Ayiti, eske w ta di w:

(1) Trè satisfè

(2) On ti jan satisfè

(3) On ti jan pa satisfè

(4) Pa satisfè di tou

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

**VB2.** Eske w te vote nan premye tou dènye eleksyon prezidansyèl 2016 la?

**(1)** Wi, li te vote **[kontinye]**

**(2)** Non, li pat vote **[ale nan VB4NEW2]**

(888888) Pa konnen **[PA LI REPONS LA]** **[ale nan VBbk2]**

(988888) Pa reponn **[PA LI REPONS]** **[ale nan VBbk2]**

**VB3N.** Pou ki moun ou te vote nan premye tou pou prezidan nan dènye eleksyon prezidansyèl 2016 la?

**[Pa Li repons yo, ekri yon sèl repons. Apre yo fin reponn, ale nan VBbk2]**

(00) Ankenn moun (Vòt blan )

(97) Ankenn (li te anile vòt li a)

(2201) Jovenel Moïse (Pati Ayisyen Tèt Kale - PHTK)

(2202) Jude Célestin (Ligue Alternative pour le Progrès et l'Emancipation Haitienne)

(2203) Jean-Charles Moïse (Platòm Pitit Desalin)

(2204) Maryse Narcisse (Fanmi Lavalas)

(2205) Eric Jean Baptiste (Mouvement Action Socialiste)

(2206) Jean Henry Céant (Renmen Ayiti)

(2277) Lòt moun

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

(999999) Pa aplike (Li pat vote) **[PA LI REPONS LA]**

**VB4NEW2. [Pou moun ki pat vote.]**

Pou ki sa ou pat vote nan dènye eleksyon prezidansyèl la?

**[Pa Li repons yo, endike tout sa yo mansyone]**

- (1) M pat gen konfizyon sou kandida yo
- (9) M pat gen konfizyon sou sa yo te vle fè pou peyi a
- (2) M pat renmen ankenn nan kandida yo, m pat renmen kanpay la
- (3) M pa gen konfyans nan eleksyon, m pa gen konfyans nan otorite ki òganize eleksyon yo
- (4) M pa gen konfyans nan demokrasi
- (5) M te gen pwoblèm enrejistremant non mwen (pwoblèm biyokratik)
- (6) M te gen pwoblèm pou laj mwen (twò jenn, twò vye)
- (7) M te nan yon lòt zòn, m te twò lwen lakay mwen
- (8) Politik pa enteresem
- (10) M pat konnen ke te gen eleksyon
- (11) M pat gen kat idantifikasyon
- (12) Sant vot la te twò lwen
- (77) Lòt rezon
- (888888) Pa konnen **[PA LI REPONS LA]**
- (988888) Pa reponn **[PA LI REPONS LA]**

**VBbk2. Eske w te vote nan dènye eleksyon pou majistra 2015 la?**

**(1)** Wi, li te vote **[kontinye]**

**(2)** Non, li pat vote **[ale nan VB11.]**

(888888) Pa konnen **[PA LI REPONS LA]** **[ale nan VB11.]**

(988888) Pa reponn **[PA LI REPONS]** **[ale nan VB11.]**

**VB3N. Pou ki moun ou te vote nan majistra a nan 2015?****[Pa Li repons yo, ekri yon sèl repons]**

- (00) Ankenn moun (Vòt blan)
- (97) Ankenn (li te anile vòt li a)
- (69) Jude Edouard, Pierre (VERITE)
- (23) Jean Berthaud, Blanchard (MAS)
- (54) Iliophene, Fabien (FANMI LAVALAS)
- (5) Yves, Blanchard (Pati Ayisyen Tèt Kale - PHTK)
- (28) Felix Bertin, Jacques (PITIT DESSALINES)
- (10) Frantz Richard, Francois (BOUCLIER)
- (2277) Lòt moun
- (888888) Pa konnen **[PA LI REPONS LA]**
- (988888) Pa reponn **[PA LI REPONS LA]**
- (999999) Pa aplike (Li pat vote) **[PA LI REPONS LA]**

**VB11.** Pou ki pati politik ou genyen panchan?

**[Pa Li repons yo, ekri yon sèl repons]**

- (2201) Fwon Lespwa
- (2202) RDNP
- (2203) Respè
- (2204) Repons Peyizan
- (2205) MPH
- (2206) Fusion des Sociaux-Démocrates Haïtienne
- (2207) Oganizasyon Pèp Kap Litè
- (2208) Alyans/Alliance Démocratique
- (2209) Renmen Ayiti (2210) Ansanm nou Fo
- (2211) Lavalas
- (2212) Unité
- (2213) PHTK (Pati Tèt Kale) (2214) Pitit Desalinn
- (2215) LA PEH
- (2216) Verite
- (2299) Union des patriotes pour l'avancement natiional (UPAN)
- (2298) Ayiti Ann Aksyon (AAA)
- (2277) Lòt pati politik
- (888888) Pa konen **[PA LI REPONS LA]**
- (988888) Pa reponn **[PA LI REPONS LA]**
- (999999) Pa aplike **[PA LI REPONS LA]**

**VB20bk1.** Si pwòchèn eleksyon pou majistra ta nan semèn sa a, ki sa ou ta fè?

**[Li repons yo]**

- (1) Ou pa tap vote
- (2) Ou ta vote pou kandida oubyen pati ki sou marie kounya
- (3) Ou ta vote pou kandida oubyen pati ki pa sou marie kounya
- (4) Ou ta vote men ou ta vote blan ou byen ou ta anile vòt ou
- (5) (888888) Pa konen **[PA LI REPONS LA]**
- (988888) Pa reponn **[PA LI REPONS LA]**

**EXC2.** Eske yon polisye te mande w yon woulman nan 12 mwa ki sot pase a?

- (1) Wi
- (2) Non
- (888888) Pa konnen **[PA LI REPONS LA]**
- (988888) Pa reponn **[PA LI REPONS LA]**

**EXCbk6.** Eske yon anplwaye otorite lokal te mande w yon woulman nan 12 mwa ki sot pase a?

- (1) Wi
- (2) Non
- (888888) Pa konnen **[PA LI REPONS LA]**
- (988888) Pa reponn **[PA LI REPONS LA]**

**EXC6.** Eske yon anplwaye leta/piblik te mande w yon woulman nan 12 mwa ki sot pase a?

- (3) Wi
- (4) Non
- (888888) Pa konnen [PA LI REPONS LA]
- (988888) Pa reponn [PA LI REPONS LA]

**EXC18.** Eske w kwè, jan bagay yo ye la a, gen de lò gen rezon pou w fè yon woulman (bay yon ti bagay anba tab)?

- (1) Wi
- (2) Non
- (888888) Pa konnen [PA LI REPONS LA]
- (988888) Pa reponn [PA LI REPONS LA]

**Finalman, an pale sou lakay ou :**

Eske ou genyen aparèy oswa bagay sa yo lakay ou a:  
[Li repons yo]

<b>R3.</b> Frijidè (Refrijeratè)	(0) Non	(1) Wi			(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>R4.</b> Telefòn fiks	(0) Non	(1) Wi			(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>R4A.</b> Telefòn potab, selilè (Asepte Telefòn entèlijjan)	(0) Non	(1) yon	(2) de	(3) Twa oswa plis pase twa	(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>R5.</b> Machin oswa kamyon [Si moun nan pa di konbyen make yon (1)]	(0) Non	(1) Wi			(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>R6.</b> Machin pou lave rad	(0) Non	(1) Wi			(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>R7.</b> Fou micro-onde	(0) Non	(1) Wi			(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>R8.</b> Motosiklèt	(0) Non	(1) Wi			(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>R12.</b> Dlo potab (tiyo) anndan kay la	(0) Non	(1) Wi			(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>DHS4.</b> Twalèt oswa latrin nan kay ou [Si moun nan pa di konbyen make yon (1)]	(0) Non	(1) yon	(2) de	(3) Twa oswa plis pase twa	(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>DHS3.</b> Èske ou pataje twalèt la oswa latrin ak lòt kay	(0) Non	(1) Wi			(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA LI REPONS LA]
<b>R14.</b> Douch	(0) Non	(1) Wi			(888888) Pa konnen [PA LI REPONS LA]	(988888) Pa reponn [PA



anndan kay la						LI REPONS LA]
<b>DHS2.</b> Chanm pou dòmi <b>[Si moun nan pa di konbyen make yon (1)]</b>	(0) Non	(1) yon	(2) de	(3) Twa oswa plis pase twa	(888888) Pa konnen <b>[PA LI REPONS LA]</b>	(988888) Pa reponn <b>[PA LI REPONS LA]</b>
<b>DHS1.</b> Radio	(0) Non	(1) yon	(2) de	(3) Twa oswa plis pase twa	(888888) Pa konnen <b>[PA LI REPONS LA]</b>	(988888) Pa reponn <b>[PA LI REPONS LA]</b>
<b>R15.</b> Òdinatè (Asepte tablet) <b>[Si moun nan pa di konbyen make yon (1)]</b>	(0) Non	(1) yon	(2) de	(3) Twa oswa plis pase twa	(888888) Pa konnen <b>[PA LI REPONS LA]</b>	(988888) Pa reponn <b>[PA LI REPONS LA]</b>
<b>R18.</b> Entenèt nan kay la (Sa inkli entenet nan telefòn li osinon nan Tablet)	(0) Non	(1) Wi			(888888) Pa konnen <b>[PA LI REPONS LA]</b>	(988888) Pa reponn <b>[PA LI REPONS LA]</b>
<b>R1.</b> Televizyon <b>[Si moun nan pa di konbyen make yon (1)]</b>	(0) Non	(1) yon	(2) de	(3) Twa oswa plis pase twa	(888888) Pa konnen <b>[PA LI REPONS LA]</b>	(988888) Pa reponn <b>[PA LI REPONS LA]</b>
<b>R16.</b> Televizyon Flat panel ouswa figi plat <b>[Si moun nan pa di konbyen make yon (1)]</b>	(0) Non	(1) yon	(2) de	(3) Twa oswa plis pase twa	(888888) Pa konnen <b>[PA LI REPONS LA]</b>	(988888) Pa reponn <b>[PA LI REPONS LA]</b>

**DHS5.** Ak kisa ou sevi pou kwit manje lakay ou?

**[Pa Li repons yo, ekri yon sèl repons]**

- 1 elektrisite
- 2 LPG - gaz pwopan likid
- 3 gaz natirèl
- 5 kewozèn
- 7 chabon
- 8 bwa
- 9 pay, touf bwa, zèb
- 10 pwodwi agrikòl (kale mayi, pye)
- 11 bèt fimye
- 95 Pa gen manje ki kwit nan kay la
- 96 lòt \_\_\_\_\_

(888888) Pa konnen **[PA LI REPONS LA]**

(988888) Pa reponn **[PA LI REPONS LA]**

**Fèmti :** Sa yo se tout kesyon mwen genyen pou ou jòdi a. Mèsi anpil pou ou tan. Mwen pral voye 50HTG sou telefon ou pi rapidman ke nou kapab. Mèsi ankò pou tan ou jodi a.

[Voye kredi le pli vit ke repondan an fini apèl la.]

**KON.** [Konfime 50HTG kredi voye ]

- (1) Wi
- (2) Non

**TI.** [Lè ak minit ankèt la fini:] \_\_\_\_\_ :

**INTID.** [Nimewo Identifikasyonl anketè a]: \_\_\_\_\_

*Mwen deklare ke ankèt sa sa te fèt ak moun mwen di a.*

*Siyati anketè a* \_\_\_\_\_ *Dat* \_\_\_\_/\_\_\_\_/\_\_\_\_

*Non anketè a* \_\_\_\_\_ *Prenon anketè a* \_\_\_\_\_

*Siyati sipèvisè a* \_\_\_\_\_ *Dat* \_\_\_\_/\_\_\_\_/\_\_\_\_

*Non sipèvisè a* \_\_\_\_\_ *Prenon sipèvisè a* \_\_\_\_\_

*Komantè:* \_\_\_\_\_

\_\_\_\_\_

\_\_\_\_\_

\_\_\_\_\_

\_\_\_\_\_

\_\_\_\_\_

\_\_\_\_\_

[Pa pou PDA/Android] *Siyati moun ki antre done yo* \_\_\_\_\_

[Pa pou PDA/Android] *Siyati moun ki te verifye done yo* \_\_\_\_\_