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Paul E. Carrillo
Edgar Castro
Carlos Scartascini

Inter-American Development Bank
Department of Research and Chief Economist

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Paul E. Carrillo*
Edgar Castro**
Carlos Scartascini**

* George Washington University
** Inter-American Development Bank

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Abstract[*]

This paper evaluates the effect of positive inducements on tax behavior by exploiting a natural experiment in which a municipality of Argentina randomly selected 400 individuals among more than 72,000 taxpayers who had complied with payment of their property tax. These individuals were publicly recognized and awarded the construction of a sidewalk. Results indicate that: i) being selected in the lottery and publicly recognized by the government has a positive but not persistent effect on future compliance; ii) receiving the sidewalk has a large positive and persistent effect; iii) high and persistent spillover effects exist: some neighbors of those who receive the reward comply more too, and these effects can be even larger than the direct effects; and iv) there is no financial motive effect; i.e., people do not pay their taxes just to participate in the lottery. Recognition serves only as a short-term incentive, but the provision of a durable and visible good has more persistent and broader effects. These findings provide evidence on features that make a positive inducement more successful, whether for tax compliance or other policy purposes.

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1 Introduction

Whether it is exercising more, cutting down on certain foods, or kicking old habits most people tend to renege on their New Year resolutions after only a couple of weeks into the new year. Even when people are induced to follow "healthier" behaviors, changes tend to fade away over time. Rewards may help to curb some unhealthy behaviors and promote the provision of public goods. For example, financial incentives may induce people to attend a gym (Aclan and Levy, 2015) and have a positive effect on inducing weight loss (John et al., 2010). Non-cash rewards may also induce people to donate blood (Lacetera et al., 2012, 2013). Moral messages may induce taxpayers to pay their taxes (Luttmer and Singhal, 2014), and households to reduce energy consumption (Ito et al., 2015; Allcott and Rogers, 2014). The effectiveness of these positive incentives is not independent of their design. On the one hand, the effect of these financial incentives tends to fade away as the incentives are removed (Aclan and Levy, 2015; John et al., 2010).[1] On the other, they may even backfire if the financial incentives crowd out intrinsic motivations (Gneezy et al., 2011). How fast rewards are delivered matter too, as the motivating power of incentives vanishes when rewards are handed out with a delay (Levitt et al., 2016). In most cases, when they do have an effect they only affect those treated but not their friends or neighbors. Consequently, as Paloyo et al. (2014) indicates, more work is needed, particularly large-scale interventions, to identify the right reward mechanism.[2]

While studying the effect of rewards has become conspicuous in the behavioral economics literature, it has been mostly absent from the burgeoning experimental empirical literature on tax compliance. The few papers that have looked at this issue show that offering rewards does not always pay off. Dwenger et al. (2016) find that the offer of rewards can significantly impact tax compliance, but the effect depends on the taxpayer's underlying motivation. In particular, rewards seem to increase compliance among the intrinsically motivated individuals but decrease compliance among the extrinsically motivated. Dunning et al. (2015) do

[1] Allcott and Rogers (2014), Rogers and Frey (2015), and Frank (2004) provide comprehensive surveys of the literature.

[2] Evaluating the role of rewards has become relevant also in the marketing and consumer behavior literature as reward and loyalty programs have grown in popularity. U.S. companies alone spend more than $1.2 billion per year (Steinhoff and Palmatier, 2016). Despite their popularity, the private sector faces challenges similar to those of the public sector in finding the right reward system. Many programs fail, their effects are not persistent, they affect only a fraction of consumers, and they may even generate negative spillover effects over other consumers. Steinhoff and Palmatier (2016) provides a good overview of programs in the private sector and the state of the discussion in that literature.

not find positive effects for a policy implemented in Uruguay in which taxpayers are rewarded with a "tax holiday." The promise of the reward did not increase compliance, and compliance dropped for those who were awarded the tax holiday. One reason behind this result is that the taxpayers who won the lottery lost the habit of paying. Finally, Koessler et al. (2016) show the results of an intervention aimed at fostering the commitment to pay taxes. In this paper, treated taxpayers were offered a reward if they fulfilled their promise of paying their taxes on time. Compliance only increased when the reward was non-financial, with no compliance effect for cash rewards. Similar conflicting results have been found in laboratory settings. For example Brockmann et al. (2016) shows that reward treatments improved female participants compliance but men reacted in the opposite way.

The results coming from the behavioral and the tax compliance literature show that the success of a reward is not independent of its design. Some rewards seem to backfire by crowding out intrinsic motivations, and when they do work their effect seems to last not very long. Moreover, rewards seem to affect only the intended recipient and have no spillover effects over other individuals. Finding a reward mechanism that works seems to be of utmost importance, particularly because rewards are becoming an instrument widely used by policymakers.[3]

In this paper, we show that there is at least one type of reward that may help to crowd in intrinsic motivation, be long lasting, and have positive spillover effects on third parties by evaluating a policy innovation introduced by the Municipality of Santa Fé (Argentina).[4] The city announced a lottery for those with no pending arrears at a certain future date. After the due date, the city government randomly selected 400 individuals among more than 72,000 taxpayers who had complied with the payment of their property tax.[5] These individuals were publicly recognized in local media and awarded the construction of a sidewalk.[6] The way the program was designed allows us to evaluate the effect of having the choice to join the lottery

[3]Rewards for good compliance are becoming quite common. For example, Giarrizzo and Brudersohn (2013) identified more than 25 reward programs at the local level in Argentina. Most of them take the form of a monetary reward either in terms of discounts of future taxes or lotteries with cash prizes, travels, cars, etc. The IRS (USA's revenue agency) is evaluating the introduction of rewards as part of their programs according to conversations held with IRS personnel in charge of the process.

[4]Feld et al. (2006) shows strong support for testing the effect of rewards based on the positive results found in laboratory experiments. Some of these laboratory experiments that found positive effects from introducing rewards in tax compliance games include Alm et al. (1992) and Torgler (2003).

[5]Because we are working with property tax instead of a tax that relies on self-declaration there was no chance of rewarding somebody who could be evading.

[6]As we describe later, sidewalk construction is the responsibility of the owner of the property. That is why we refer throughout this paper as a private good -otherwise it would have been owner-provided- of public use -anybody can walk on it.

(financial motive), the effect of public recognition (moral channel), and the effect of the provision of a visible and durable good (reciprocity and peer effect channels). Also, we can evaluate the immediate and the persistence effect of each channel. Finally, it also allows to evaluate across groups of beneficiaries: effect on the winners (direct effect) and on their neighbors (spillover), and the effect on compliers and non-compliers. It has some other more specific benefits. On the one hand, evaluating the compliance on the property tax has the advantage of providing a direct measure of compliance and change of behavior induced by the reward, it reduces the probability of rewarding a non-complier (there are no information problems), and makes it easier to evaluate persistence from administrative records. On the other, the actual provision of recognition and a public reduces the uncertainty that individuals face with other positive incentives and moral messages, which makes it easier to evaluate the actual effect of the policy abstracting from trust issues.

This reward system has several potential benefits as a policy tool. First, using non-monetary rewards, particularly public recognition and the provision of a private good of public use, may crowd in intrinsic motivations better than monetary rewards. Second, because the reward is a highly visible and durable good, both the taxpayers who receive the reward and the neighbors who walk on those sidewalks can witness the government in action and the use it makes of public monies, such as system may enhance *reciprocity* for the winners and have spillover effects on their neighbors.[7] Third, because the reward can only be received by those in good standing, neighbors of the winner have direct and concrete evidence that people around them are paying the tax, which should lead them to update their beliefs about compliance in the neighborhood: *peer effects*. As such, the reward may increase the sense of duty for those who received the reward (moral responsibility), and those who did not. Fourth, compared to most reward programs that provide private and low visibility prizes, this type of reward, a durable and visible good, may generate a longer lasting effect than the more traditionally strategies of providing recognition, providing information through messaging, or paying for good behavior.[8]

Results indicate that rewarding taxpayers for good behavior has large positive and persistent effects for lottery winners and for the neighbors of winners. First, we find that lottery winners were 3 percentage

[7] Gonzalez-Navarro and Quintana-Domeque (2015) show that the connection between public goods provision and tax compliance can be relevant. In their work, provision of pavement seems to increase compliance by about 7 percentage points.

[8] Finding persistent effects tends to be an elusive quest in the literature working with positive inducements. On the one hand, many papers have a hard time evaluating the long-term effects of the interventions because of data availability. On the other, those who have been able to look at longer time series tend to find that most effects are short-lived.

points more likely to continue paying on time over the next few years compared to their peers (*ITT*). This effect is persistent for at least 3 years after the intervention. This average effect comes from a positive effect from winning the lottery and from the construction of the sidewalk. Those selected in the lottery and recognized publicly by the government as a good taxpayer are about 5 percentage points more likely to comply (short-term effect). Those who received the sidewalk are about 7 percentage points more likely to comply (persistent effect). Given that most taxpayers who pay their taxes already do so on time (about 86 percent at the baseline) these estimates imply a reduction in late payment of about 36, and 50 percent.[9] Second, we also find high and persistent spillover effects from the winners to their neighbors. These effects are not universal but seem to depend on the salience of the reward, which provides another indication in favor of the mechanisms favored in our framework. As an example, we find that a homeowner who is neighbor of a winner but did not participate in the lottery, who is living in an area of the city with low public service provision, is about 7.5 percentage points more likely to pay on time, about 10 (15) percentage points more likely to pay within 3 (6) months, and about 15 percentage points more likely to pay at all. Again, these results are persistent in the long run. Finally, we find no significant financial motive effect.

Overall, these results have relevant implications for the literature and for policymakers. First, design matters for finding positive effects. Those rewards that crowd in intrinsic motivations work better than those that crowd out them. Second, design is also important for ensuring that effects do not fade away rapidly and that rewards have positive spillovers over third parties. Using a highly visible and durable good may have contributed to the persistence of the effect and its spillovers. If the reward had been limited to the recognition of the taxpayers in the newspapers, the effect would have been high, but short-lived. Third, the promise of a reward seems to be a relatively inefficient tool for changing behaviors. On the one hand, it is difficult to design a lottery with high expected value that is financially viable at the same time. On the other, as with any promise, trust issues enter into play, which even further reduces the expected value of the reward. Fourth, the rewards have heterogeneous effects across individuals. Not every individual reacts the same to a reward. Therefore, targeting seems to be an intrinsic part of a good design. Finally, given that

[9]The estimates are also large in terms of the dependent variables that look at overall payment behavior given that there is a high level of persistence shown by tax compliance for those who comply and those who do not. In Castro and Scartascini (2015), the probability of paying in period t given that the taxpayer had paid in $(t-1)$ was about 100%; similarly, Dwenger et al. (2016) find that those who evaded in 2010 were 87 times more likely to evade in 2011. Running an auto-regressive model we find that the probability of not paying on t given no payment on $(t-1)$ is 90%. For those who paid on time, 64%; 55% for payment within 3 months, and 80% for payment within 6 months.

providing a public good seems to affect the relationship between the taxpayer and the government (increases reciprocity) and among taxpayers (increases peer effects), targeting the provision of a public good may have positive implications beyond taxation: targeting of public works may help improve other outcomes such as increasing trust, and reducing corruption and crime. As Feld et al. (2006) argued before us, "Rewards could be an effective tool to increase compliance". In this paper, we show how effective they are, and by comparison with previous attempts, we hint at some characteristics that may have helped to determine those positive effects.

The paper is organized as follows. Section 2 presents the conceptual framework, Section 3 presents the institutional background, Section 4 presents the data, Section 5 presents the empirical results for the lottery winners, Section 6 presents the spillover effects, and Section 7 discusses financial motives behind the results. Finally, Section 8 concludes.

2 Conceptual Framework

Why do people pay taxes? Most people do so because of a combination of extrinsic and intrinsic motivations. On the one hand, the probability of being caught and being forced to pay a fine serves as deterrence to evading. On the other, people pay their taxes because they derive utility from doing so. Cultural factors play a role in this, but also the individual's relationship to the state (*reciprocity* or fiscal exchange), and the relationship and behavior of other people (*peer effects* or social influence). People tend to comply more if they believe that others comply as well, and if they believe that the government makes good use of the money it collects (Alm, 2006; Luttmer and Singhal, 2014; Scholz and Lubell, 1998; Torgler, 2005).

Rewards to taxpayers who comply can affect an individual's decision by increasing the value of complying, which may sway some taxpayers in the margin. Which channels rewards influence would depend on the characteristics of the rewards. Monetary rewards may only work through financial incentives (and may crowd out some intrinsic motivations) (Bonner and Sprinkle, 2002). Now, those who pay, in addition to avoiding a potential penalty may receive a prize, but engaging in this transaction may affect the value of compliance on the feeling of pride that accompanies honesty and the fulfillment of civic duties. Non-monetary rewards may work better at influencing the moral determinants of compliance.

Willingness to pay taxes may depend on the individual relationship with the state (*reciprocity* and *peer-*

effects). Most of the evidence supporting the role of *reciprocity* comes either from laboratory experiments (Alm et al., 1992; Torgler, 2003; Brockmann et al., 2016) or field experiments that have relied on sending messages to taxpayers regarding the use of money by the government. For example, Hallsworth et al. (2014) finds a positive effect of reminding people of the relationship between taxes and public goods received. Still, these results are not universal (Hallsworth, 2014; Luttmer and Singhal, 2014). One reason may be that different people update their beliefs differently and average effects mask valuable heterogeneous responses (Castro and Scartascini, 2015). Another reason may be that a letter may just not be enough to cause taxpayers to change their beliefs, or their change in beliefs may not be sufficiently large as to cause changes in behavior. An alternative setting to mailing letters or giving cash rewards includes providing a (divisible) public good directly to taxpayers. That is, a public good that, while it can be enjoyed by everybody (non-excludable), can be assigned to an individual. Constructing an individual's sidewalk has that characteristic.

Regarding *peer effects*, people tend to comply more if they know that other people are complying too. Evidence so far -usually based on messages delivered in letters- has been mixed (Luttmer and Singhal, 2014). Again, part of the reason may have to do with the design of the treatments. Providing a visible reward to compliers could serve as a strong source of information to non-compliers regarding peers behavior because it sends a clear signal about the winners' compliance to the group (and about the likelihood that others are complying too).

The following simple model incorporates these effects into the analysis. Taxpayers earn an income Y and are billed property taxes T. Individuals get utility from consumption c and also from paying taxes on time. Individuals who pay taxes on time receive non-pecuniary benefits to their utility.

Letting $I = 1$ if taxes are paid on time and zero otherwise, we define individual's utility $u = u(c, I * s)$ where, as in Dwenger et al. (2016), s is a parameter that measures the strength of the relationship between paying taxes on time and utility. We also make the standard assumption that $u_1 > 0$ and $u_2 > 0$.

Unlike income taxes, property tax liabilities are calculated by the municipality and individuals play no role in its determination. However, taxpayers can choose between complying (paying the tax bill) and not complying. The value of having an opportunity to pay her property tax bill is equal to

$$\pi = \max[u(Y - T, s), (1 - \theta)u(Y, 0) + \theta u(Y - \tau T, 0)] \tag{1}$$

where θ is the detection-and-enforcement probability and $\tau > 1$ is a penalty. While the enforcement probability is a deterministic number known only to the tax authority, we assume that individuals' perceptions about θ are heterogeneous ($\theta \sim F$).

Individuals will choose to pay their tax bill on time if the left side of the maximum operator in equation 1 is no less than the right side.

The optimal choice can be illustrated in Figure 1.

Individuals with perceptions about $\theta > \widehat{\theta}$ will pay the tax and vice versa. Before the intervention, the proportion of individuals who pay the property tax on time is equal to $1 - F(\widehat{\theta})$. An intervention that increases the amount of public services that taxpayers receive increases s to s' (with $s' > s$) through two mechanisms. First, taxpayers who won the lottery increase their loyalty to the government for recognizing their effort. Second, once the sidewalk is constructed, taxpayers can witness the "government in action" providing a highly durable and visible public good. Both channels increase *reciprocity*. Third, neighbors of the winner can update their beliefs regarding compliance in the neighborhood (*peer effect*). [10] As summarized in Figure 1, the compliance rate would go up as a result of the intervention.

Because the sidewalk is a durable good, we expect its effects to be longer lasting than recognition or a message. We do not expect too much heterogeneity for the winners of the reward given that all the winners would be affected similarly by the treatment. However, we do not expect the spillover effect to be homogeneous for the neighbors. First, we expect the sidewalk to be more salient in some areas of the city than in others, hence to have a differential impact in how much they affect taxpayers' beliefs. In particular, we expect the effect to be more prominent in those areas of the city with fewer services from the government (e.g., where there is no pavement). Second, we expect taxpayers with lower compliance records to react more to the evidence coming from a neighboring winner given that their beliefs regarding others' compliance would be lower than the beliefs of those with good compliance records.[11]

[10] We have summarized the moral channels with s for simplicity. Following Traxler (2006) we could split s into a *reciprocity* and a *peer-effect* component such as $\phi_i c(n)_i$. Then, the moral benefit would come from the degree of norm internalization ϕ_i, which represents the degree of reciprocity with the government, and the function $c(n)_i$ which captures the strength of the norm according to the estimated share of tax evaders.

[11] Castro and Scartascini (2015) find heterogeneous effects for the *reciprocity* and *peer effect* treatments. Taxpayers in areas where there is lower provision of public goods seem to update their perceptions (and change their behavior) more than people residing in high public provision areas. Similarly, taxpayers who had not paid before were more likely to change their behavior following a treatment informing about average compliance.

3 Institutional Background and Intervention

3.1 *Property Taxes in Santa Fé*

The Municipality of Santa Fé is the eight largest city in Argentina (about 391 thousand residents). It is the capital city of the Province of Santa Fé (the third most populated province in the country). The Municipality collects real estate property taxes (which are locally called "Tasa General de Inmuebles"). Almost all real estate properties are taxed, including homes, vacant lots, and business premises. Taxes are proportional to the properties assessed values. The public assessor office (Servicio de Catastro e Información Territorial) assesses the value of all real estate properties in the province, including those in the city of Santa Fé.[12] The Municipality of Santa Fé then applies a fixed tax rate to the properties' assessment. Property tax rates are identical for all properties within the city. Santa Fé also imposed a minimum property tax that varied within the city.[13]

Assessed values remained constant between 2008 and 2011. Tax rates and minimum taxes, however, experienced some changes. In 2008, the average and median taxpayer paid ARG $32.30 (USD $9.50) and ARG $20 (USD $5.80), respectively each month.[14] In January 2009, tax rates increased and mean and median tax payments almost doubled; ARG $46 (USD $13.60) and ARG $33 (USD $9.80) respectively. It should nonetheless be noted that tax rates and minimum taxes experienced moderate but not drastic changes during our period of interest January 2009 to December 2011.

Taxpayers are billed monthly, but the bill is delivered to the owners' address every 3 months (every trimester each taxpayer receives at the same time the bills necessary to pay the following 3 months). From the moment they receive the bills they have approximately 10 days to pay the first month of the trimester bill before its first due date. Late payments are charged a monthly interest rate of 3%. Late fees cannot be larger than 3 times the original tax liability.

[12] The province did not update the assessed values during the period of study. Because of that, the tax system in the city underwent an important overhaul in 2012.

[13] The city is divided into 10 "tax districts," each of them with its own minimum tax. In 2008, about one quarter of taxpayers were billed the minimum tax.

[14] The average annual tax amount in 2008 is approximately 25% of a monthly minimum wage.

3.2 *Intervention*

In an effort to reward good taxpayers and improve property tax compliance, in January 2009, the municipal government of Santa Fé organized a lottery (called "Premio al Buen Contribuyente"), that entitled winners to a full sidewalk construction or renovation. The prize had an additional purpose, to showcase a "model" sidewalk as an enticement for residents to adopt it.[15] The sidewalk renovations included the removal of the old sidewalk, sewerage adjustments, and convenient features such as a trash receptacle that would not be accessible to animals. The main distinctive characteristic of this sidewalk is that it contained a "green line", a section of the sidewalk that could be neither covered nor asphalted and dedicated to plants and trees (also provided by the Municipality). This type of sidewalk increases water absorption, a relevant feature in a city prone to torrential precipitation. How much of a green line each sidewalk has depends on the size of the property given that by regulation at least 1.6 meters from the house to the street should be paved for pedestrian circulation.[16] The city estimated that the average sidewalk renovation would cost ARG $5,250 (approx. USD $1,553) (Decree 1716). This is equivalent to 14.4 times the average yearly tax payment (ARG $363.5) in 2008 and 9.7 times the average yearly tax payment of 2009 (ARG $539.5).

The rules of the lottery were officially announced on December 16, 2008 (Decree 1716). Lottery rules were straightforward. Owners of residential units, commercial properties and/or vacant lots were eligible to participate in the lottery as long as they had paid their 2008 property tax liabilities by January 12, 2009. Each eligible property received a unique number, and 400 properties were randomly chosen from a set of 72,742. The lottery took place on February 27, 2009. City officials contacted each of the winners and also announced lottery results in local newspapers.[17]

There are no concerns about potential biases in the lottery. It was conducted by Santa Fé's Provincial Lottery using standard procedures: eligible properties received a unique number and 400 numbers were chosen at random. A public notary was present at the time of the lottery to guarantee that the process was

[15] In Santa Fé, the owner of the property is required to build and maintain the sidewalk in spite of being a public good (Decree 7279). The minimum characteristics of the sidewalk are stipulated in the city regulations. Consequently, the municipality is not expected to build the sidewalks, and it has not implemented other sidewalk renovation program in a systematic fashion.

[16] Figure 2 shows different types of sidewalk. For example, on the first row, the first picture shows a property with a "green line" and new trees and a trash receptacle while the second shows a sidewalk in which the width of the sidewalk does not allow either. While the examples on the first row show sidewalks in parts of the city where the taxpayers receive all the public services the city provides, the houses pictured in the second row are examples of sidewalks constructed in areas of the city where some services, such as pavement, are lacking.

[17] The list of eligible participants and winners could also be found on the municipality's website.

fair.[18] Moreover, in the next section we will show that there are no statistically significant differences in observed characteristics between lottery winners and non-winners.

Sidewalk renovations could not take place all at the same time, mostly due to resource constraints and public procurement procedures. Table 1 shows the number of renovations by quarter in 2009 and 2010 (and the timeline of the intervention). The second and third columns show the number of projects that started and were completed during each quarter, respectively. The construction of new sidewalks started after the lottery, peaked in June 2009 and lasted for a little over a year. The average duration of a renovation is 11 days.[19] At the time we collected our data, 223 sidewalks had been built, and 16 were started but no date of completion was reported. The rest of the sidewalks were not constructed for various reasons, such as, the owner did not respond to the invitation, the existing sidewalk was in perfect condition, or the lottery winner donated the sidewalk to another taxpayer. Unfortunately, from the existing records we cannot differentiate among the reasons.

4 Data Source

We have access to administrative data from the Municipality of Santa Fé. We work with three sets of data. The first data set contains the roster of all properties in the city. For each property, we observe its address and some characteristics about the building such as the size of the construction and the size of the lot. The city also knows what public services each property receives. For example, records show if (a) a unit is connected to the public water, gas and sewerage networks; (b) street lightning is available; (c) the street surface is asphalt, concrete, gravel or dirt; and (d) the property receives garbage collection services.

The second data set contains information about property tax payments. For every month between January 2008 and December 2011 and for every property in the city, we observe the monthly property tax liability (as it appears on the bill), and its due date. The data also include information about each taxpayer's payment history. This information is used to compute a series of variables that measure individuals' tax compliance. In particular, we compute three indicator variables that equal to one if a payment (a) was made

[18] Lottery rules specify that individuals who own more than one property cannot receive multiple prizes. None of the 400 properties selected in the initial draw belonged to the same owner. This is not surprising given that the percentage of individuals who own more than one property is very low (14.2%).

[19] We have checked that the order of construction and length of time it takes for the work to be completed is not correlated to any observable characteristics.

on time, (b) was made within three months, and (c) was made within six months. These are the main outcome variables we analyze in the empirical section when we look at the results in the sample of individuals that participated in the lottery. Once we expand the analysis to their neighbors, including those who had not been paying their taxes regularly, we can also compute two additional dependent variables: (i) bill was never paid; and (ii) joining a tax debt restructuring plan.

The third data set includes details about the lottery. We can identify all taxpayers that participated in the lottery as well as lottery winners.[20] The data also contain information on each sidewalk renovation's start and completion dates. The data are used to compute the treatment variables.

5 Direct Effects: Data, Empirical Strategy, and Results

5.1 *Descriptive Statistics of Lottery Participants*

We focus on all properties in the payment database that received a tax bill in the month *before* the lottery was announced (November 2008). Some of these records feature unusually low or unusually high tax liabilities. These unusual values are likely due to measurement error and/or very specific tax exemption rules.[21] To avoid biases due to outliers, we trim the top and bottom 1 percentiles from the sample.

We then identify the properties that were eligible to participate in the lottery. That is, within the group of properties that received a tax bill during November, 2008, we pick those that had no pending tax obligations due by January 12, 2009. About 56% of properties in Santa Fe fall into this category. Notice that the group of properties that are eligible to participate in the lottery is clearly not representative of the population, because, by definition, it features higher tax compliance rates. Later, we look at the overall population when we evaluate spillover effects to those who participated and did not participate in the lottery.

Table 2 presents the number of observations. During 2008, the pre-treatment period, our sample includes 71,346 properties eligible to participate in the lottery. We can match 394 lottery winners to this sample.[22] The number of properties that could be followed up during the subsequent years is slightly smaller because

[20] The sample of properties eligible to receive the prize is found by identifying all properties that had no pending tax obligations as of January 12, 2009. Winners were identified from publicly available lists.

[21] For example, some churches and other charities pay very little property tax.

[22] Some of the winners are individuals who are exempted from paying the tax, such as public sector properties; hence, they are not included in our database of taxpayers.

of attrition.

Table 3 shows descriptive statistics for the sample of eligible properties during 2008, the pre-treatment period. The typical property in our sample has 123 m^2 of interior space and a lot area of 270 m^2. Most properties are connected to water, gas and sewerage networks, and about 19% face a dirt or a gravel road. The City also computes a *Public Service Index* for each property that summarizes how many services it receives. Theoretically, the index would be bounded between 0 and 1, but effectively it only varies between 0.5 and 1. A property with an index of 1 is located in a neighborhood with asphalt pavement and street lightning, has access to the public water, sewerage and gas networks, and receives garbage collection services; a property with an index of zero would receive no services but every property receives garbage collection.[23] The average index in our sample is 0.79. Tax compliance is high in our sample of eligible taxpayers. During 2008, 85% of tax bills were paid on time and 99% were paid within 6 months.

The second and third column of Table 3 compute descriptive statistics for lottery winners and non-winners. It is clear that there are no statistically significant differences between these groups. Figure 3 plots the spatial distribution of lottery winners. As it can be observed, while winners are distributed all around the city there is a slightly higher concentration of winners in the south part of the city, but this is precisely the downtown area where population density is the highest. We use a Pearson $\tilde{\chi}^2$ test to test if the spatial distribution of "winners" is the same as the spatial distribution of all properties. Results from a Pearson $\tilde{\chi}^2$ test do not allow us to reject the null that these two distributions are the same.[24] The combined evidence above reassures that the lottery outcome was truly random.

5.2 *Identification and Results*

Assessing the *direct* effects of winning the lottery on tax compliance is straightforward. Because treatment (lottery assignment) was randomized, the causal effect of the lottery can be simply assessed by comparing average outcomes of treated and non-treated *lottery participants* during the post-treatment period (February

[23] Each property receives 0.5 points if garbage collection services are available. If a property is connected to the water, sewerage and gas network, it receives additional 0.1, 0.125 and 0.1 points respectively. When the street's pavement is asphalt or concrete (gravel), 0.125 (0.05) points are added. Finally, properties with street lightning receive additional 0.05 points.

[24] In order to do this, properties were assigned to groups (East vs West; North vs South) according to their geographical location (degrees plus two decimals). To perform the test, we estimate the distribution of properties across groups and compare it with the distribution of winners' properties. A standard Pearson $\tilde{\chi}^2$ test is used to check if these two distributions are the same. East vs West (longitude): Pearson $\tilde{\chi}^2(17)$=12.1905 Pr=0.788 and North vs South (latitude): Pearson $\tilde{\chi}^2(14)$=10.1287; Pr=0.753.

2009 - December 2011). Formally, we estimate the following linear probability model.

$$Y_{it}^{v} = \alpha D_i + X_i\Delta + Z_{it}\Gamma + \lambda_t + \mu_{it}, \tag{2}$$

where X_i is a vector of time-invariant characteristics of property i, Z_{it} is a vector of time variant characteristics of the property, which is basically the tax liability of property i in month t, D_i is an indicator that takes the value of one if the property was selected in the lottery (to be awarded a sidewalk renovation) and zero otherwise, λ_t are time fixed effects, and μ is an unobserved random term. Y^v represents each of the outcome variables ($v = a, b, c, ...$) described in the previous section for lottery participants. The coefficient α measures the causal effect of the lottery, or the "intent-to-treat" (ITT) effect of the program.

Using our post-treatment sample (February 2009 - December 2011) of lottery participants, we estimate equation 2 with OLS and report results in Table 4. The first row in the table displays the estimate of α, when the first outcome variable (Y^a) is regressed on different sets of covariates. Each column represents a different specification. Column (1) includes only the treatment effect D; columns (2) and (3) add property and tax bill characteristics, respectively; column (4) includes time fixed effects (for each month-year combination). The second and third rows in this table display $\widehat{\alpha}$ when Y^b and Y^c are regressed on the same set of covariates.

All estimates of α are positive, statistically significant, and robust across specifications. Results suggest that the lottery had a positive and statistically significant effect on tax compliance over the 3-year period we are considering. The lottery increased the likelihood that winners pay their tax obligations on time by about 3.1 percentage points (not the instantaneous effect but the average over the period), and it raised the probability that property taxes are paid within 3 (6) months by 2.4 (2.1) percentage points. It is notable to observe such a positive effect considering that compliance rates in our sample are very large to begin with: at the baseline, 86% of lottery participants had paid their tax liability on time, 89% paid within 3 months and 99% within 6 months. Therefore, a 3 percentage points increase in paying on time reduces delays in payment by 21%.

As described, the results presented before show the average effect over the three-year period. A relevant question is whether the effect is permanent or if the effect of the lottery on timeliness and compliance declines with time. To check this, we interact the treatment with the number of months since treatment. Results shown in Table 5 strongly suggest that the direct effect of the interventions is persistent, at least in

the first three years after the intervention.[25] There seems to be no significant tendency for compliance as the coefficient for M is very small and the sign is positive only for one of the variables and negative for the other two. Our study is one of the firsts to find positive persistent effects of interventions to boost tax compliance.[26]

Given the previous results, we could safely conclude that the reward was successful for increasing the share of individuals paying on time and maintaining tax compliance among those who were selected by the lottery. The conceptual framework points to two mechanisms through which the lottery and the provision of a durable good can affect tax compliance: *reciprocity* and *peer-effect*. The lottery and the provision of the good can increase the loyalty of taxpayers to the Municipality and the intrinsic motivation of taxpayers to "pay back" to the Municipality, and it allows the taxpayers to witness how the local government uses the money it collects. All of these channels would enhance *reciprocity*. A priori, *reciprocity* may be increased by the lottery and the dissemination of results in the press, and by the provision of the good. The provision of the public good, because it also signals compliance in the group, can affect taxpayers' priors about compliance; hence, the *peer-effects* channel. Notice, however, that the treatment should not affect lottery winners expectations about their peers' behavior. The sidewalk renovation provides recognition and a private good (of public use) for the winner but no (or little) information about their neighbors. Hence, the positive effects found in the previous section are likely explained by an increase in the winners' *reciprocity* to the state.

One remaining question may be how much of the effect is generated by the lottery itself (being publicly recognized by the Municipality) and how much of the effect is generated by the construction of the sidewalk. In order to elicit this information we can make use of the fact that almost no construction was initiated immediately after the lottery winners were announced and the fact that not every winner received a new sidewalk. First, we look at the effect of the lottery winner's announcement (*recognition*) by estimating equation 2 using the March 2009 data (lottery took place on February 27, 2009) period in which only one

[25] The Municipality passed a thorough tax reform afterwards; that is why we do not look at the effect beyond December 2011.

[26] As Mascagni (2014) and Slemrod (2016) indicate in their surveys of the current literature, very few papers have looked at persistence (researchers tend to have access to data for a brief period of time and most interventions are relatively recent). Among them, Manoli and Turner (2016) show that the effect of a reminder notice has a large short-term effect (80%) but the effect quickly dissipates a year later (22%). Guyton et al. (2016) intervention to induce filing among non-filers who are eligible to receive credits has an effect, but one that does not persist in future years when the mailed reminders stop. Similar results have been reported in related literature. For example, Ito et al. (2015) shows significant short-run effects of moral suasion messages for reducing electricity consumption, but the effects diminish quickly after repeated interventions.

sidewalk was renovated. Therefore, any effect should be consequence of the announcement of the lottery winners and not from construction. We find a positive and large effect for timely payment of 5.4 percentage points as it can be observed in Table 6 column 1 (this column includes the full set of control variables.) Then, we estimate the same model for the rest of months for consistency even though we can't say much given that construction picks up afterwards. For robustness purposes, we also show in the first two rows of the Table that there is no effect in the 2 months previous to the intervention (no effects on winners before the announcement of the results).

So far we have shown that the lottery rewarding the construction of a sidewalk has a positive and long-lasting effect (ITT results), and that the lottery by itself has a large positive and significant effect on the month after the lottery was drawn. Is the effect from winning the lottery long lasting, or is the persistence generated by the provision of the durable good? In order to test this we follow a different strategy. As mentioned in the previous section, not every lottery winner received the prize (the sidewalk was not constructed on their property) and some non-winners received a sidewalk renovation (winners donated the sidewalk to them). To evaluate the differences between the direct effect of winning the lottery and the effect of the sidewalk renovation, we estimate the effect on those that won the lottery but did not receive the renovation. Because receiving the sidewalk might be endogenous (e.g., some winners decided to donate their prize), we estimate the results in a difference in difference model as described below.

$$Y_{it}^{a} = \gamma R_{it} + Z_{it}\Delta + \lambda_t + \rho_i + \mu_{it} \tag{3}$$

Here Y_{it}^{a} is equals one if the bill for the period t and taxpayer i was paid on time and zero otherwise. R_{it} equals 1 for lottery winners that did not receive a sidewalk renovation in the post-treatment period and Z_{it} is a vector of time-variant characteristics. λ_t and ρ_i are time and taxpayer fixed effects respectively. The sample covers both pre-treatment and post-treatment periods hence the parameter γ is the Diff-in-Diff estimator. Again, we estimate this model by looking at the effect on a month-by-month basis.

The results in Table 6 column 2, where the coefficient for each month is shown, present again a positive effect in the short run after the lottery is drawn but the effect fades away rapidly (there is no significant effect after 5 months). Again, for robustness purposes, we also show in the first two rows of the table that there are no significant effects before the lottery was drawn (months of January and February).

Summarizing, results in Table 6 indicate that: (i) there is a positive effect from winning the lottery; (ii) for those who won the lottery but construction did not take place, the effect fades away after a few months. Results in Table 4 and Table 5 show that there is persistent effect of the overall program. Therefore, the persistence of the effects may be coming from the construction of the durable good. Can we estimate the effect?

To try to get an approximate estimate of the effect of the construction of the sidewalk we can exploit the fact that, even though there is a positive effect coming from being recognized, that effect is short-lived. Hence, by the time construction starts and the individual is considered to be "treated," for most winners the effect of winning the lottery has faded away completely, which would allow us to use the lottery as an instrument given that there would be no remaining significant effect from winning the lottery on compliance. To ensure this in even stronger terms, we also restrict the sample by eliminating the whole first year after the lottery was drawn, hence ensuring that the short-term effects coming from recognition have fully dissipated. Therefore, we can estimate a "local average treatment effect" (LATE) by instrumenting the construction of the sidewalk with the lottery assignment and using Two Stage Least Squares (2SLS) as described in the equation below to estimate the effect of the construction of the sidewalk.

$$Y_{it}^{v} = \beta T_{it} + X_i \Phi + Z_{it}\zeta + \lambda_t + \mu_{it} \tag{4}$$

Here T_{it} equals one if property i featured a renovated sidewalk in period t.[27] Unlike the lottery assignment, however, T is not random. To alleviate concerns about selection into treatment we instrument T_{it} by the assignment to treatment D_i. The coefficient β measures the causal effect of a sidewalk renovation on outcome variable Y^v conditional on construction (LATE).

Results are displayed in Table 7. The table shows $\widehat{\beta}$, the causal effect of a sidewalk renovation on tax compliance. As before, each row features a model with a different outcome variable, and each column a different specification. The first four columns use the full sample data. The last column restricts the analysis to the years 2010-2011. That is, it evaluates the effect a full year after the assignment to the lottery to reduce concerns about the validity of the instrument. The evidence indicates a large and statistically significant

[27] As described, sidewalk construction was not performed immediately and uniformly for everybody, but it was phased over a two-year period.

effect of a sidewalk renovation on tax compliance. Results in the fourth column suggest that renovating a sidewalk increases timely payment rates by 7.1 percentage points (average effect for the whole period), and the likelihood that the tax bill is paid within 3 and 6 months increases by 5.5 and 4.8 percentage points, respectively. All estimates are notably robust across specifications. The results are quite similar when we restrict the sample to 2010-2011 as shown in the last column for each dependent variable. The results indicate once more how persistent the effect is as the estimates do not change much in spite of dropping the effects from the first year after the lottery took place. Because almost half of lottery winners did not receive a sidewalk renovation, it is not surprising that the LATE ($\widehat{\beta}$) is about twice the size of the ITT ($\widehat{\alpha}$).

In order to check if these effects are persistent we follow the same strategy than before by interacting the treatment with the number of months since treatment. Results shown in Table 8 strongly suggest that the direct effect of the interventions is persistent, which suggests once more that the type of reward may be a key factor.

5.2.1 *Discussion, Robustness Checks, and Placebo Analysis*

The results presented so far indicate that there is a positive and persistent effect of winning the lottery. These effects are explained by a positive but short-term effect of winning the lottery and being recognized and a positive and longer lasting effect derived from actually receiving the prize, the construction of a durable private good (of public use). Both results point towards *reciprocity* as the mechanism behind higher compliance and timeliness of payment. They also indicate that being recognized publicly as a compliant taxpayer may not be enough to encourage persistence. Providing a durable and visible good may do the trick.

One may worry that part of the results may be explained by an alternative potential mechanism, a "gambling effect" either for the winners or for the eligible non-winners. That is, winners keep paying because they overestimate the probability of winning in the future. Given that those who won once could not be eligible to participate in future lotteries, we believe this mechanism is unlikely to affect the behavior of winners. Eligible neighbors, however, may decide to increase (or maintain) their tax compliance to participate in future lotteries. If this were the case, as well as with any other source of spillover, our estimates would be a lower bound of the potential effect of the program on the winners.

One could also argue that the construction of a sidewalk could have affected beliefs about tax compliance of the control group. If lottery participants of a neighbor who received a sidewalk renovation updated their overall beliefs about their peers' tax compliance as a result of the treatment, the average treatment effect discussed above should be biased downwards and represent a lower bound of the true effect.

Finally, we would like to discuss the robustness of the previous results by looking at a placebo analysis. First, we have already described two preliminary placebo test before when we looked at the immediate effect of the lottery on the winners, and to the effect on the winners who did not get the sidewalk constructed. While we found an effect for the winners in the month of March that effect was non-existent in previous months. Second, we run a more comprehensive placebo analysis by running equation 2 in alternative samples of winners ("fake winners") to check if our result could be replicated in other samples. For that purpose, we draw 1,000 different random sets of winners and perform a permutation analysis. Basically, every other sample generates zero effects, and the result we estimated with the actual sample of winners is 8 standard deviations higher than the average result that we would get by chance. Figure 4 shows the results for the 3 dependent variables. These results convincingly show that the positive treatment effects we report are very unlikely to have occurred by chance.

6 Spillover Effects: Data, Empirical Strategy, and Results

We now turn our interest to the potential effect of the lottery and the construction of the sidewalks on the neighbors of those who were eligible to participate in the lottery. As we have discussed, a reward system would be more effective if it could have an effect not only on the intended recipients but also on third parties. With a reward like the one we have described, there is a potential for a spillover effect. There are two potential sources of this spillover. First, neighbors who walk on the sidewalks can witness the government in action and the use it makes of public monies, which may increase *reciprocity*. Second, it may affect compliance through another of the moral channels: *peer effects*. Because the reward can only be received by those in good standing, neighbors have direct and concrete evidence that people around them are paying the tax, which should lead them to update their beliefs about compliance in the neighborhood.

Because of the way rewards have usually been designed, spillover effects are hardly estimated in the general behavioral economics literature. In the more general tax compliance literature, the evidence, albeit

scant, shows that taxpayers use the information they gain about their colleagues or neighbors to update their beliefs, which affects their behavior, as we argue in this paper. For example, Paetzold and Winner (2014) shows that an individual's evasion decision is influenced by the compliance behavior of other coworkers, with job changers from low- to high-cheating companies starting to evade much more after they move. As information is transmitted, it increases their knowledge about the possibilities of non-compliance. Information transmission is also a mechanism behind Drago et al. (2015) results. In this work, neighbors of treated individuals change their behavior in the same direction as treated ones, with effects being larger for those closer in the network.

In this section, we evaluate the effect of the reward on the neighbors of those who won the lottery and show a positive spillover effect consistent with the workings of the *peer-effect* and *reciprocity* channels.

6.1 *Descriptive Statistics*

As we move from looking at the lottery participants to include their neighbors as well, we run the regressions on a larger set of individuals that includes both the taxpayers who were eligible to participate in the lottery and taxpayers who were not eligible. Our treated individuals will be the individuals that were neighbors of those who won the lottery. We define as treated neighbors those taxpayers located in the same geographic block as the winner, which is identified in the Municipality database by the first 5 digits of the identification number.

Table 9 presents descriptive statistics for the new sample and shows the balance across the treatment and control groups. This larger sample includes 124 thousand taxpayers, which corresponds to (98%) of all properties in the city (we remove from the analysis people living in blocks with no eligible taxpayers). As expected, average compliance is smaller in this larger sample than in the previous one as we include taxpayers who do not pay regularly and when they do pay they usually do so with some delay.

Because the treatment and control groups were not randomly selected (again, treatment and control are determined according to whether a taxpayer lives in the same block than a winner or not) there could be some systematic differences between them (theoretically, people living next to winners may be systematically different than those living next to non-winners). However, these differences are necessarily small by design (only a few taxpayers won the lottery from the pool of eligible taxpayers). Moreover, given that

the probability of having a winner in the block depends on the number of lottery participants in the block and the total number of properties in that block, we expect that differences between these groups would be statistically insignificant after controlling for these two block-specific features. We test this assumption in Table 9 and do not find any differences that are significant at the 5% level. Three of the outcomes variables (paid on time, paid within 3 months and paid within 6 months) have statistically significant differences at the 10% level only. Interestingly, these differences are in favor of the control group, which would work against finding any results in the post-treatment analysis. There are no statistically significant differences for a fourth dependent variable, "bill never paid."

6.2 *Identification and Results*

To evaluate the *spillover* effect of the program we use the combined sample of lottery participants and non-participants and a simple linear model

$$Y_{ijt}^{v} = \alpha B_j + E_j\Theta + X_{ij}\Delta + Z_{ijt}\Gamma + \lambda_t + \mu_{ijt}. \tag{5}$$

Here Y_{ijt}^{v} is tax compliance (measured according to variable $v = a, b, c, ...$) in period t of property i located in block j. The treatment B_j equals to one if there is at least one lottery winner in block j. As we discussed, the randomization among eligible taxpayers was done at the property and not the block level. Hence, taxpayers in blocks with more lottery participants are more likely to be treated. Because the probability of treatment depends on the number of lottery participants in the block, we include in all specifications the number of households eligible to participate in the lottery in block j and the number of taxpayers in each block j (both are included in the vector of block level characteristics E_j). X_{ij} is a vector of time-invariant property characteristics, and Z_{ijt} a vector of time-variant property characteristics. λ_t are time fixed effects.

Results are reported in Table 10. Besides controlling for the number of lottery participants and the number of taxpayers in each block, and some property's characteristics, the first column in this table includes the treatment variable B; columns (2) and (3) add tax bill characteristics and time fixed effects, respectively. All coefficients of interest are precisely estimated zeroes which strongly suggest that the intervention produced no *average* spillover effects.

This average result is not particularly surprising when we take into account that, first, any potential

spillover would be concentrated in a subset of neighbors, the non-eligible to participate from the lottery. This is the case because any change in behavior has to come from a change in the underlying beliefs of the taxpayers. Eligible taxpayers, because they pay their taxes regularly and on time, would tend to have a more positive view of the government's use of resources and a belief that overall compliance is also higher. Therefore, changes in reciprocity and peer effects would be more pronounced in the non-eligible group.[28] Consequently, new information provided by a neighbor winning the lottery and new information about the use of resources and quality of goods provided by the government would not make eligible taxpayers to revise their beliefs upward. On the contrary, non-eligible taxpayers are the ones that would update their beliefs after learning that one of the neighbors won the lottery and after walking on the sidewalk. To tackle this issue, we add to the baseline model described in equation 5 an indicator that takes the value of one if the taxpayer was not eligible to participate in the lottery and zero otherwise, and an interaction between the treatment variable and this indicator.

Results are shown in Table 11. Note that now, as expected, we find strong positive effects for the non-eligible neighbors of those who won the lottery. Basically, a non-eligible treated individual is about 1.5 percentage points more likely to pay on time (column 3), and almost 2.2 (3.5) percentage points more likely to pay within 3 (6) months. These taxpayers are also about 3.5 percentage points more likely to pay their bill altogether. These are relatively large, statistically significant, and robust effects. Again, it is almost unheard of reward programs having positive spillover effects on people other than the winners of the rewards.

A second reason behind small average effects is that any potential spillover would not be uniform across the city but higher were construction is more salient (neighbors need to learn about the reward in order to react). Therefore, the probability that a neighbor would update their beliefs and change her behavior should be heterogeneous. To deal with this issue, we proxy the salience of the intervention with the existing level of provision of public goods in the block. That is, a new sidewalk will be more salient in areas of the city that are being serviced less by the local government (e.g., where there is no pavement, there are fewer lights, there is fewer access to water and sewage, etc.) -see Figure 2 for examples. On the one hand, for taxpayers to revise their beliefs regarding compliance they should be aware that a sidewalk is being constructed and that the sidewalk is a prize for compliance. Consequently, *peer-effects* should be stronger not only for non-

[28]Castro and Scartascini (2015) shows that taxpayers who are compliant and taxpayers who are not update their beliefs about what their neighbors do very differently when faced with new information about how many people actually pay their taxes.

eligible taxpayers, as discussed previously, but also in areas with lower provision of public goods. On the other hand, taxpayers will be more likely to revise their beliefs upward regarding the use of public monies by the local government when the sidewalks add to the capital stock of the city in areas of lower public good provision than when they provide a sidewalk in an area of the city which is very well-serviced. Moreover, again, beliefs about the efficiency of the local government would be high already in highly-serviced areas and much lower where there is little presence of the local government. Hence, new construction should move beliefs more in the second area than in the first.

To test if the intervention has stronger spillover effects in places where it is more salient, we interact the Public Service Index (PSI) with our treatment variable. Results displayed in Table 12 suggest that there are very strong heterogeneous effects. The effects are statistically significant at all conventional significance levels and are large in magnitude. For example, for properties located in the areas with the lowest PSI (PSI=0.5), the effect is about 4.5 percentage points for payment on time, 6 (9) percentage points for payment within 3 (6) months, and 11 percentage points for paying the bill.

Finally, to make sure that we are not capturing a similar effect in both regressions (i.e., that non-eligibility and PSI proxy the same phenomena) we run a triple interaction that incorporates both interactions in the same specification. Results in Table 13 and Figure 6 show that these effects are additive. That is, treatment effect is higher for non-eligible taxpayers and even larger when these taxpayers are located in low PSI areas. A non-eligible taxpayer living in an area of the city with low public service provision is about 5.3 percentage points more likely to pay on time, about 7.5 (12.25) percentage points more likely to pay within 3 (6) months, and about 15 percentage points more likely to pay at all.

6.2.1 *Discussion*

We found strong spillover effects for the group of non-eligible to participate in the lottery particularly in areas where the intervention was more salient. What are the mechanisms that drive this result? Part of it may be driven by an increase in *reciprocity* as taxpayers can evaluate better the works done by the local government. Part may be driven by the fact that a visible intervention such as a sidewalk renovation will likely remind the neighborhood about the tax lottery and presumably can change individuals perceptions about overall rates of tax compliance. The intervention is more visible and salient in places with low or no

public services and it affects those taxpayers whose beliefs about compliance can change the most. Hence, we interpret the results as evidence that *peer-effects* can be one important mechanism that accounts for the positive spillovers effects.

Of course, we cannot completely rule out some "gambling" effects. That is, that the neighbors who witnessed the new sidewalks started complying more in order to be eligible to participate in a potential future lottery. There are several reasons why we believe this effect is close to zero or negligible. First, there was no communication done by the government suggesting that the lottery was going to be performed again. Second, there is no reason why non-eligible individuals would decide to start paying their owed taxes right after construction of the sidewalk in front of their neighbors property starts instead of waiting for the new lottery to be announced. Third, as we discuss in the following section, we find no significant evidence of a financial motive for mobilizing compliance when the lottery was originally announced.

7 Ex-ante Financial Motives

As we have mentioned, to participate in the lottery, taxpayers had to be up-to-date with their tax liabilities for 2008 by January 12, 2009. Did taxpayers who had some pending tax liabilities by the moment the lottery was announced find it profitable to pay outstanding tax liabilities in order to being able to participate in the lottery?[29] We evaluate this in several ways. First, we look at the distribution of payments of outstanding liabilities. In Figures 7 and 8 we look at the cancellation of outstanding liabilities incurred over the year from December 1st onward, for the years 2007, 2008, 2009, and 2010. The vertical lines indicate the date of announcement of the lottery (16 December 2008) and the due date for canceling existing liabilities to participate in the lottery (12 January 2009). As can be observed in the figures, for every year considered there is a spike in payments around the due date of the December payments and the trends are very similar afterwards. That is, there is no evidence of a significant spike of pending payments for the year 2008 either in terms of number or in terms of amounts. The trend for payments of liabilities from 2008 is not significantly different than the trends for the other years in between the vertical lines.

[29] In terms of the model presented in Section 2, the financial motive would enter as an additional component of the financial benefit from paying the tax. Now, the optimization model would have the following form

$$\pi = \max[u(Y - T + E(V), s), (1 - \theta)u(Y, 0) + \theta u(Y - \tau T, 0)] \tag{6}$$

where $E(V)$ is the expected value from the lottery. The larger is $E(V)$ the higher the share of taxpayers that would pay.

One reason we may find no overall effects in payments is that the lottery might have been interesting only for those taxpayers with a smaller number of pending payments. Consequently, we perform a second analysis by looking not only at the overall number of payments in between the announcement of the lottery and the due date but also by checking if we could find some heterogeneous effects according to the number of pending payments for each taxpayer. In order to test this we run a regression using as dependent variable, a dichotomous variable that takes the value one when a payment occurs within the period Dec 16 - Jan 12. As independent variables we include the complete set of control variables we have used so far and a dummy variable that takes the value one for every tax bill due in 2008 and zero otherwise. This variable is interacted with the number of monthly tax bills already canceled by Dec 16. Table 14 shows the results from running this regression. Looking at the last column, which includes all the controls and the month*year fixed effects, results indicate that payments during the period between the announcement and the due date for eligibility for the year 2008 are basically equal to zero.

Consequently, there is no significant evidence of a financial motive effect. That is, individuals with outstanding liabilities when the lottery was announced did not cancel their debts to participate in the lottery. A reason behind this result may be that the expected value of the prize was not worth it. Basically, given that the prize was about 10 times the yearly tax payments (or 120 times the monthly payment) but the odds of winning the prize were relatively low, the expected value of the lottery is below the cost of making a payment, even if the taxpayer owes only one payment, as described below.

$$E(V) = \frac{winners}{participants} \times prize \quad \approx \quad \frac{400 \times 120}{72000} \times T_t \quad = \quad \frac{2}{3} \times T_t \quad < \quad T_t \tag{7}$$

8 Conclusion

There has been very little work on evaluating the effect of rewards and positive inducements on tax compliance. The papers that have been written show mixed results. Similarly has happened in other fields, where rewards have a positive effect only when they crowd in intrinsic motivations but they tend to fail when they crowd them out. Finding rewards that have permanent effects is rare, and having positive spillovers on non-treated individuals is non-existent, as far as we know. The evidence indicates that the key for finding permanent effects and spillover effects lies in the design of the reward system.

In this paper we go beyond what the literature on positive incentives and rewards has looked at in several ways. First, instead of evaluating the effect of monetary compensations or positive messages we look at the role of financial motives and the effect of recognition and the provision of a durable and visible good. This is important because it allows us to compare their effectiveness in a context where there is no crowding out of intrinsic motivations (a problem with other reward systems). Second, because we have access to very detailed administrative records over a long period of time we can evaluate persistence of the rewards in the long run. Third, because of data access but more importantly the reward system, we can also evaluate the effect of a reward on third-parties, both compliers and non-compliers. Fourth, again, because of the provision of a good instead of relying on messages or promises we can also evaluate better the effect of the positive incentive by abstracting from trust issues that may confound the results. Finally, the design also allows us to concentrate on evaluating the role of *reciprocity* and *peer-effects* independently of potential effects that may be coming from deterrence, a problem with other papers that use contact with taxpayers as the treatment.

Results show that the reward had a positive effect on compliance for those who won the lottery. First, taxpayers feel recognized by the government and feel good about having their name posted on the news as "good taxpayers." This effect seems to be short-lived. Second, once construction takes place, taxpayers can evaluate better the work that the local government provides and how it uses the money it collects. Hence, winning the lottery generates a short term positive effect on compliance (about 5 percentage points in timely payment) and having the sidewalk constructed generates a positive and permanent effect (about 7 percentage points on average over a 3-year period from the moment the sidewalk is constructed). These effects are most likely explained by an increase in *reciprocity*, one of the moral channels behind voluntary tax compliance.

The reward did also have an effect on the neighbors of those who won the reward. This effect can be explained by the combination of *reciprocity* and *peer-effects*: first, once construction takes place, neighbors can better evaluate the goods that the local government provides and how it uses the money it collects (*reciprocity*); second, because winning a lottery is a rare event (400 sidewalks out of more than 70 thousand eligible and 120 thousand total taxpayers), neighbors of a winner can use this information to update their beliefs about compliance in the neighborhood (*peer-effect*) -if somebody in the surrounding area won the lottery, it must be the case that the number of taxpayers participating in the lottery (hence, tax compliance) is

relative high. The change in beliefs regarding compliance should be higher for the previously non-compliant taxpayers, and the change in beliefs about government works should be higher where the works were more salient (areas with lower public service provision). Empirical tests confirm these hypotheses. A non-eligible taxpayer living in an area of the city with low public service provision is about 7.5 percentage points more likely to pay on time, about 10 (15) percentage points more likely to pay within 3 (6) months, and about 15 percentage points more likely to pay at all. We also show that there is no significant evidence of a financial motive effect. That is, there is no evidence that individuals with outstanding liabilities when the lottery was announced canceled those debts to participate in the lottery.

Given how costly sidewalks are, was it worth it for the local government to conduct this program? Our estimates of the effect, particularly thanks to the positive and large spillover effects on non-compliant neighbors, would indicate so. Additionally to the pure financial analysis, it is important to note that a second objective of the local government was to showcase the new type of sidewalk so individuals who construct their own would use it as a benchmark -this sidewalk reduces the amount of water running on the street, which has a positive welfare effect we cannot estimate.

The lessons are still numerous in terms of design and expected returns. First, there is no immediate financial motive effect (people canceling their debts in order to get into the lottery) -which is similar to what other papers have found. Consequently, the promise of prizes, particularly when the expected value is low, does not seem to entice people to change their behavior. Second, public recognition for those "doing the right thing" is positive and significant, but short-lived. After a few months people return to their previous behavior, as has been the case in most policy domains. Therefore, recognition should be accompanied by more permanent incentives that move individuals to a better equilibrium. Third, the effect on eligible taxpayers is high but mostly in terms of increasing timely payments. Consequently, the effect is highest for enticing payment when it creates incentives for those who were non-compliers before. For that to occur it is important for the reward to be highly visible and durable. Targeting according to visibility and potential spillover effects could generate ample benefits. Given that providing a public good seems to affect the relationship between the taxpayer and the government, public goods targeting may have implications beyond taxation: targeting may help improve other outcomes such as increasing trust, and reducing corruption and crime.

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Table 1:
Timeline of the Intervention

Date	Activities
16 Dec 2008	Lottery announcement
12 Jan 2009	Due date for participants to be in good standing with 2008 tax liabilities
27 Feb 2009	Lottery date
01 Mar 2009	Beginning of the treatment implementation
09 Sep 2010	End of the treatment implementation

	Timing of Renovations	
	Started	Completed
2009.q1	1	0
2009.q2	16	14
2009.q3	57	56
2009.q4	40	40
2010.q1	66	58
2010.q2	24	22
2010.q3	19	5
2010.q4	0	1
Total	223	207

Table 2:
Number of Properties in Sample

Year	All Properties	Lottery Participants	Winners
2008	127,152	71,346	394
2009	127,091	71,346	394
2010	126,778	71,346	394
2011	126,310	71,057	393

Table 3:
Descriptive Statistics of Lottery Participants
and Balance Table (Year 2008)

Variables	All	Winners	Non-winners	Diff	p-val
Y^a: 1(Tax bill paid on time)	0.856	0.868	0.856	0.012	0.316
	(0.351)	(0.339)	(0.351)		
Y^b: 1(Bill paid within 3 months)	0.892	0.903	0.892	0.012	0.132
	(0.311)	(0.296)	(0.311)		
Y^c: 1(Bill paid within 6 months)	0.989	0.989	0.989	-0.000	0.940
	(0.105)	(0.106)	(0.105)		
Y^d: 1(Bill never paid)	0	0	0	0	-
	(-)	(-)	(-)		
Tax Liability (ARG $)	33.4	33.3	33.4	-0.100	0.911
	(29.2)	(31.20)	(29.20)		
Interior space (m^2)	123.0	123.38	123.042	0.336	0.956
	(122.5)	(115.5)	(122.5)		
Lot area (m^2)	270.5	194.2	270.861	-76.648	0.803
	(2400.3)	(198.8)	(2406.6)		
l(Access to Water Network)	0.885	0.862	0.885	0.-227	0.216
	(0.319)	(0.345)	(0.319)		
l(Access to Sewerage Network)	0.733	0.700	0.734	-0.033	0.165
	(0.442)	(0.458)	(0.442)		
l(Access to Gas Network)	0.844	0.839	0.844	-0.047	0.809
	(0.363)	(0.367)	(0.327)		
l(Street Lightning)	0.836	0.812	0.836	-0.0240	0.235
	(0.370)	(0.310)	(0.370)		
l(Street Surface: Asphalt or Concrete)	0.815	0.787	0.815	0.028	0.195
	(0.389)	(0.410)	(0.388)		
Public Service Index	0.796	0.790	0.796	-0.006	0.257
	(0.099)	(0.102)	(0.099)		
Number of properties	71,346	394	70,952		

Notes: Table shows the average and standard deviation (in parenthesis) of variables during the pre-treatment period. The indicator function $1(.)$ equals to one if the condition inside the parenthesis is true and zero otherwise.

Table 4:
Effect of Lottery on Tax Compliance (OLS)
ITT Results

	Model Specification			
Outcome variable:	[1]	[2]	[3]	[4]
Y^a: Tax bill was paid on time	0.027**	0.031**	0.031**	0.031**
	(0.012)	(0.012)	(0.012)	(0.012)
Y^b: Tax bill was paid within 3 months	0.020*	0.024**	0.024**	0.024**
	(0.010)	(0.010)	(0.010)	(0.010)
Y^c: Tax bill was paid within 6 months	0.018**	0.021***	0.021***	0.021***
	(0.008)	(0.008)	(0.008)	(0.008)
Covariates:				
Characteristics of property	N	Y	Y	Y
Characteristics of tax bill	N	N	Y	Y
Time fixed effects (35)	N	N	N	Y
Number of observations:	2,557,679	2,557,679	2,557,679	2,557,679

Notes: Table displays the estimate of OLS regression models when outcome Y^v $(v = a, b, c)$ of property i in month t is regressed on the lottery outcome D and a set of covariates. Sample includes all properties eligible to participate in the lottery during the post-treatment period (January 2009 - December 2011). Each column in the table corresponds to a different specification. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Table 5:
Effect of Lottery on Tax Compliance (OLS)
Short vs. Long Term Effects

Dependent Variable: Y^a: Tax bill was paid on time				
	[1]	[2]	[3]	[4]
D x M	-0.001	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
D: l(Lottery Winner)	0.037**	0.031**	0.031**	0.031**
	(0.016)	(0.014)	(0.014)	(0.014)
M: # Months Since Lottery	0.001***	0.001***	0.001***	0.003***
	(0.000)	(0.000)	(0.000)	(0.000)
Dependent Variable: Y^b: Tax bill was paid within 3 months				
	[1]	[2]	[3]	[4]
D x M	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
D: l(Lottery Winner)	0.024**	0.017*	0.017*	0.017*
	(0.012)	(0.009)	(0.009)	(0.009)
I: # Months Since Lottery	-0.001***	-0.001***	-0.001***	-0.004***
	(0.000)	(0.000)	(0.000)	(0.000)
Dependent Variable: Y^c: Tax bill was paid within 6 months				
	[1]	[2]	[3]	[4]
D x M	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
D: l(Lottery Winner)	0.019**	0.010**	0.010**	0.010**
	(0.008)	(0.004)	(0.004)	(0.004)
M: # Months Since Lottery	-0.001***	-0.002***	-0.002***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Other Covariates:				
Characteristics of property	N	Y	Y	Y
Characteristics of tax bill	N	N	Y	Y
Time fixed effects (34)	N	N	N	Y

Notes: Table displays the estimate of OLS regression models when outcome Y^v $(v = a, b, c)$ of property i in month t is regressed on the lottery outcome D and a set of covariates. Sample includes all properties eligible to participate in the lottery during the post-treatment period (January 2009 - December 2011). Each column in the table corresponds to a different specification. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Table 6:
Month by Month Effects of the Lottery
OLS and DiD estimations

Dependent variable: Y^a: Tax bill was paid on time	[1]	[2]
Independent variable	D	D_{wr}
January	0.018	-0.011
	(0.022)	(0.026)
February	0.024	0.012
	(0.018)	(0.020)
March	0.054***	0.040**
	(0.017)	(0.019)
April	0.049***	0.035
	(0.019)	(0.023)
May	0.061***	0.055***
	(0.018)	(0.021)
June	0.058***	0.043**
	(0.017)	(0.021)
July	0.055***	0.038*
	(0.017)	(0.021)
August	0.050***	0.021
	(0.018)	(0.023)
September	0.033*	0.017
	(0.018)	(0.023)
October	0.037*	0.026
	(0.019)	(0.023)
November	0.006	-0.003
	(0.020)	(0.025)
December	0.044**	0.016
	(0.018)	(0.023)
Covariates:		
Estimation	OLS	DiD
Characteristics of property	Y	N
Characteristics of tax bill	Y	Y
Taxpayer fixed effects	N	Y
Time fixed effects	Y	Y

Notes: Table displays the coefficients of regression models when outcome Y^a of property i in month t. The first column show the results of an OLS model identical to Equation 2 but on a month by month basis. Each row shows the results for α of a different regression. The second column shows the results of a DiD model as described in Equation 3. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Table 7:
Effect of Sidewalk Renovation on Tax Compliance (2SLS)

	Model Specification				
Outcome variable:	[1]	[2]	[3]	[4]	[5]
Y^a: Tax bill was paid on time	0.062**	0.071**	0.071**	0.071**	0.060**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Y^b: Tax bill was paid within 3 months	0.047*	0.055**	0.055**	0.055**	0.050*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Y^c: Tax bill was paid within 6 months	0.042**	0.048***	0.048***	0.048***	0.049**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Covariates:					
Characteristics of property	N	Y	Y	Y	Y
Characteristics of tax bill (pre-treatment)	N	N	Y	Y	Y
Time fixed effects (23)	N	N	N	Y	Y
Excluding 2009 from sample	N	N	N	N	Y
Results From First Stage:					
LM test statistic for underidentification (Anderson or Kleibergen-Paap)	159.0	159.1	159.1	159.1	158.2
p-value of underidentification LM statistic	0.00	0.00	0.00	0.00	0.050
F statistic for weak identification (Cragg-Donald or Kleibergen-Paap)	277.7	278.1	278.1	278.1	275.6
Number of observations:	2,550,598	2,550,598	2,550,598	2,550,598	1,702,726

Notes: Table displays 2SLS estimates when outcome Y^v $(v = a, b, c)$ of property i in month t is modeled as a function of sidewalk renovation T and a set of covariates. The sample includes all properties eligible to participate in the lottery during the post-treatment period (January 2009 - December 2011). Instruments for T are the lottery assignment D. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Table 8:
Effect of Lottery on Tax Compliance (2SLS)
Persistence of the Intervention

Dependent Variable: Y^a: Tax bill was paid on time					
	[1]	[2]	[3]	[4]	[5]
D x M	-0.001	-0.001	-0.001	-0.001	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.002)
D: l(Renovatted Sidewalk)	0.084**	0.072**	0.072**	0.072**	0.063
	(0.04)	(0.03)	(0.03)	(0.03)	(0.056)
M: # Months Since Lottery	0.001***	0.001***	0.001***	0.002***	0.003***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.000)
Dependent Variable: Y^b: Tax bill was paid within 3 months					
	[1]	[2]	[3]	[4]	[5]
D x M	-0.001	0.000	0.000	0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.002)
D: l(Renovatted Sidewalk)	0.054**	0.040*	0.041*	0.041*	0.055
	(0.03)	(0.02)	(0.02)	(0.02)	(0.043)
M: # Months Since Lottery	-0.001***	-0.001***	-0.001***	0.001***	-0.005***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.000)
Dependent Variable: Y^c: Tax bill was paid within 6 months					
	[1]	[2]	[3]	[4]	[5]
D x M	-0.000	0.001	0.001	0.001	-0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.001)
D: l(Renovatted Sidewalk)	0.043**	0.025**	0.025**	0.025**	0.069**
	(0.02)	(0.01)	(0.01)	(0.01)	(0.034)
M: # Months Since Lottery	-0.001***	-0.002***	-0.002***	0.001***	-0.001***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.000)
Other Covariates:					
Characteristics of property	N	Y	Y	Y	Y
Characteristics of tax bill	N	N	Y	Y	Y
Time fixed effects (34)	N	N	N	Y	Y
Excluding 2009 from the sample	N	N	N	N	Y

Notes: Table displays 2SLS estimates of equation 4, when outcome Y^v $(v = a, b, c)$ of property i in month t is modeled as a function of sidewalk renovation T and a set of covariates. The sample includes all properties eligible to participate in the lottery during the post-treatment period (February 2009 - December 2011). Instruments for T are the lottery assignment D and its interaction with property characteristics. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Table 9:
Descriptive Statistics Spillovers
and Balance Table (Year 2008)

Variables	Average	Diff Treat. and Control *	p-val
Y^a: 1(Tax bill paid on time)	0.566	-0.018	0.073
	(0.50)		
Y^b: 1(Bill paid within 3 months)	0.604	-0.018	0.085
	(0.49)		
Y^c: 1(Bill paid within 6 months)	0.692	-0.018	0.098
	(0.46)		
Y^d: 1(Bill never paid)	0.173	0.011	0.258
	(0.37)		
Tax Liability (ARG $)	32.343	-0.851	0.525
	(83.20)		
Interior space (m2)	118.005	-3.631	0.349
	(262.10)		
Lot area (m2)	200.260	-140.9	0.339
	(190.49)		
l(Access to Water Network)	0.802	-0.003	0.841
	(0.38)		
l(Access to Sewerage Network)	0.608	-0.002	0.937
	(0.49)		
l(Access to Gas Network)	0.758	0.011	0.549
	(0.42)		
l(Street Lightning)	0.773	-0.005	0.820
	(0.43)		
l(Street Surface: Asphalt or Concrete)	0.694	-0.008	0.685
	(0.45)		
Public Service Index	0.765	-0.000	0.987
	(0.11)		
	Total	Treatment	Control
Number of blocks	3,667	315	3,352
Number of properties	124,190	22,849	101,341

Notes: Table shows the average and standard deviation (in parenthesis) of variables during the pre-treatment period. The indicator function $1(.)$ equals to one if the condition inside the parenthesis is true and zero otherwise. * denotes difference between treatment and control groups after controlling by the number of eligible taxpayers and number of properties in each block.

Table 10:
Average Spillover Effect of Lottery on Tax Compliance (OLS)
ITT Results

	Model Specification		
Outcome variable:	[1]	[2]	[3]
Y^a: Tax bill was paid on time	0.000	-0.002	-0.004
	(0.003)	(0.003)	(0.003)
Y^b: Tax bill was paid within 3 months	0.003	-0.003	-0.004
	(0.003)	(0.003)	(0.003)
Y^c: Tax bill was paid within 6 months	-0.000	-0.003	-0.003
	(0.003)	(0.003)	(0.003)
Y^d: Tax bill was never paid	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)
Covariates:			
Characteristics of property	Y	Y	Y
Characteristics of tax bill	N	Y	Y
Time fixed effects (35)	N	N	Y
Number of blocks	4191	4191	4191
Number of observations:	4,520,461	4,520,461	4,520,461

Notes: Table displays the estimate of OLS regression models when outcome Y^v $(v = a, b, c, d, e)$ of property i in month t is regressed on the lottery outcome D_{block} and a set of covariates. Sample includes all properties eligibles and non-eligibles to participate in the lottery located in blocks with at least one lottery participant during the post-treatment period (January 2009 - December 2011). Each column in the table corresponds to a different specification. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Table 11:
Heterogeneous Spillover Effect of Lottery on Tax Compliance (OLS)
Interaction with Non-Elegible Taxpayers

Dependent Variable: Y^a: Tax bill was paid on time			
	[1]	[2]	[3]
D x H	0.024***	0.024***	0.025***
	(0.005)	(0.005)	(0.005)
D: I(There's a lottery winner in $block_f$)	-0.006**	-0.008***	-0.010***
	(0.003)	(0.003)	(0.003)
H: I(Non-eligible property)	-0.241***	-0.233***	-0.222***
	(0.006)	(0.005)	(0.005)
Dependent Variable: Y^b: Tax bill was paid within 3 months			
	[1]	[2]	[3]
D x H	0.035***	0.035***	0.035***
	(0.005)	(0.005)	(0.005)
D: I(There's a lottery winner in $block_f$)	-0.010***	-0.012***	-0.013***
	(0.002)	(0.002)	(0.002)
H: I(Non-eligible property)	-0.246***	-0.238***	-0.232***
	(0.006)	(0.006)	(0.006)
Dependent Variable: Y^c: Tax bill was paid within 6 months			
	[1]	[2]	[3]
D xH	0.054***	0.055***	0.055***
	(0.005)	(0.005)	(0.005)
D: I(There's a lottery winner in $block_f$)	-0.017***	-0.019***	-0.020***
	(0.002)	(0.002)	(0.002)
H: I(Non-eligible property)	-0.273***	-0.265***	-0.263***
	(0.006)	(0.006)	(0.006)
Dependent Variable: Y^d: Tax bill was never paid			
	[1]	[2]	[3]
D x H	-0.056***	-0.056***	-0.056***
	(0.005)	(0.005)	(0.005)
D: I(There's a lottery winner in $block_f$)	0.018***	0.020***	0.019***
	(0.002)	(0.002)	(0.002)
H: I(Non-eligible property)	0.239***	0.231***	0.234***
	(0.005)	(0.005)	(0.005)
Other Covariates:			
Characteristics of property	Y	Y	Y
Characteristics of tax bill	N	Y	Y
Time fixed effects (34)	N	N	Y

Notes: Table displays the estimate of OLS regression models when outcome Y^v $(v = a, b, c, d, e)$ of property i in month t is regressed on the lottery outcome D and a set of covariates. Sample includes all properties eligibles and non-eligibles to participate in the lottery located in blocks with at least one lottery participant during the post-treatment period (January 2009 - December 2011). Each column in the table corresponds to a different specification. All specifications include the number of eligible properties and total number of properties in block j as controls. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Table 12:
Heterogeneous Spillover Effect of Lottery on Tax Compliance (OLS)
Interaction with the Public Service Index

Dependent Variable: Y^a: Tax bill was paid on time			
	[1]	[2]	[3]
D x I	-0.170***	-0.130***	-0.145***
	(0.028)	(0.028)	(0.028)
D: I(There's a lottery winner in $block_f$)	0.142***	0.107***	0.117***
	(0.023)	(0.023)	(0.023)
I: Public Service Index	0.495***	0.403***	0.400***
	(0.010)	(0.010)	(0.010)
Dependent Variable: Y^b: Tax bill was paid within 3 months			
	[1]	[2]	[3]
D x I	-0.216***	-0.176***	-0.185***
	(0.029)	(0.029)	(0.029)
D: I(There's a lottery winner in $block_f$)	0.179***	0.144***	0.151***
	(0.023)	(0.023)	(0.023)
I: Public Service Index	0.489***	0.396***	0.394***
	(0.010)	(0.010)	(0.010)
Dependent Variable: Y^c: Tax bill was paid within 6 months			
	[1]	[2]	[3]
D x I	-0.310***	-0.267***	-0.271***
	(0.030)	(0.030)	(0.030)
D: I(There's a lottery winner in $block_f$)	0.255***	0.219***	0.221***
	(0.025)	(0.025)	(0.025)
I: Public Service Index	0.496***	0.398***	0.397***
	(0.011)	(0.011)	(0.011)
Dependent Variable: Y^d: Tax bill was never paid			
	[1]	[2]	[3]
D x I	0.403***	0.359***	0.358***
	(0.034)	(0.034)	(0.034)
D: I(There's a lottery winner in $block_f$)	-0.331***	-0.294***	-0.293***
	(0.029)	(0.029)	(0.029)
I: Public Service Index	-0.484***	-0.384***	-0.384***
	(0.012)	(0.012)	(0.012)
Other Covariates:			
Characteristics of property	Y	Y	Y
Characteristics of tax bill	N	Y	Y
Time fixed effects (35)	N	N	Y

Notes: Table displays the estimate of OLS regression models when outcome Y^v $(v = a, b, c, d, e)$ of property i in month t is regressed on the lottery outcome D and a set of covariates. Sample includes all properties eligibles and non-eligibles to participate in the lottery located in blocks with at least one lottery participant during the post-treatment period (January 2009 - December 2011). Each column in the table corresponds to a different specification. All specifications include the number of eligible properties and total number of properties in block j as controls. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Table 13:
Heterogeneous Spillover Effect of Lottery on Tax Compliance (OLS)
Triple interaction

Dependent Variable: Y^a: Tax bill was paid on time			
	[1]	[2]	[3]
D x I x H	0.077**	0.057	0.059
	(0.037)	(0.037)	(0.037)
D x H	-0.038	-0.021	-0.023
	(0.028)	(0.028)	(0.028)
D x I	-0.284***	-0.240***	-0.253***
	(0.033)	(0.033)	(0.033)
I x H	0.092***	0.080***	0.081***
	(0.018)	(0.018)	(0.018)
D: l(There's a lottery winner in $block_q$)	0.225***	0.187***	0.195***
	(0.028)	(0.027)	(0.027)
I: Public Service Index	0.431***	0.358***	0.355***
	(0.013)	(0.014)	(0.014)
H: l(Non-eligible property)	-0.574***	-0.562***	-0.562***
	(0.014)	(0.014)	(0.014)
Dependent Variable: Y^b: Tax bill was paid within 3 months			
	[1]	[2]	[3]
D x I x H	0.027	0.007	0.009
	(0.037)	(0.036)	(0.036)
D x H	0.010	0.027	0.025
	(0.028)	(0.028)	(0.028)
D x I	-0.293***	-0.249***	-0.256***
	(0.032)	(0.031)	(0.031)
I x H	0.228***	0.216***	0.216***
	(0.018)	(0.018)	(0.018)
D: l(There's a lottery winner in $block_q$)	0.228***	0.190***	0.195***
	(0.026)	(0.026)	(0.026)
I: Public Service Index	0.355***	0.282***	0.281***
	(0.012)	(0.013)	(0.013)
H: l(Non-eligible property)	-0.686***	-0.674***	-0.674***
	(0.014)	(0.014)	(0.014)
Dependent Variable: Y^c: Tax bill was paid within 6 months			
	[1]	[2]	[3]
D x I x H	-0.080**	-0.101***	-0.100***
	(0.039)	(0.038)	(0.038)
D x H	0.111***	0.128***	0.128***
	(0.029)	(0.029)	(0.029)
D x I	-0.304***	-0.259***	-0.260***
	(0.031)	(0.031)	(0.031)
I x H	0.487***	0.475***	0.475***
	(0.019)	(0.019)	(0.019)
D: l(There's a lottery winner in $block_q$)	0.232***	0.193***	0.194***
	(0.026)	(0.026)	(0.026)
I: Public Service Index	0.225***	0.151***	0.150***
	(0.011)	(0.012)	(0.012)
H: l(Non-eligible property)	-0.879***	-0.866***	-0.866***
	(0.015)	(0.015)	(0.015)

Dependent Variable: Y^d: Tax bill was never paid			
	[1]	[2]	[3]
D x I x H	0.069*	0.090**	0.091**
	(0.041)	(0.040)	(0.040)
D x H	-0.103***	-0.121***	-0.121***
	(0.032)	(0.031)	(0.031)
D x I	0.344***	0.297***	0.294***
	(0.031)	(0.031)	(0.031)
I x H	-0.846***	-0.834***	-0.833***
	(0.020)	(0.020)	(0.020)
D: l(There's a lottery winner in $block_q$)	-0.265***	-0.225***	-0.223***
	(0.026)	(0.026)	(0.026)
I: Public Service Index	-0.035***	0.042***	0.042***
	(0.010)	(0.010)	(0.010)
H: l(Non-eligible property)	0.970***	0.957***	0.957***
	(0.016)	(0.016)	(0.016)
Other Covariates:			
Characteristics of property	Y	Y	Y
Characteristics of tax bill	N	Y	Y
Time fixed effects (34)	N	N	Y

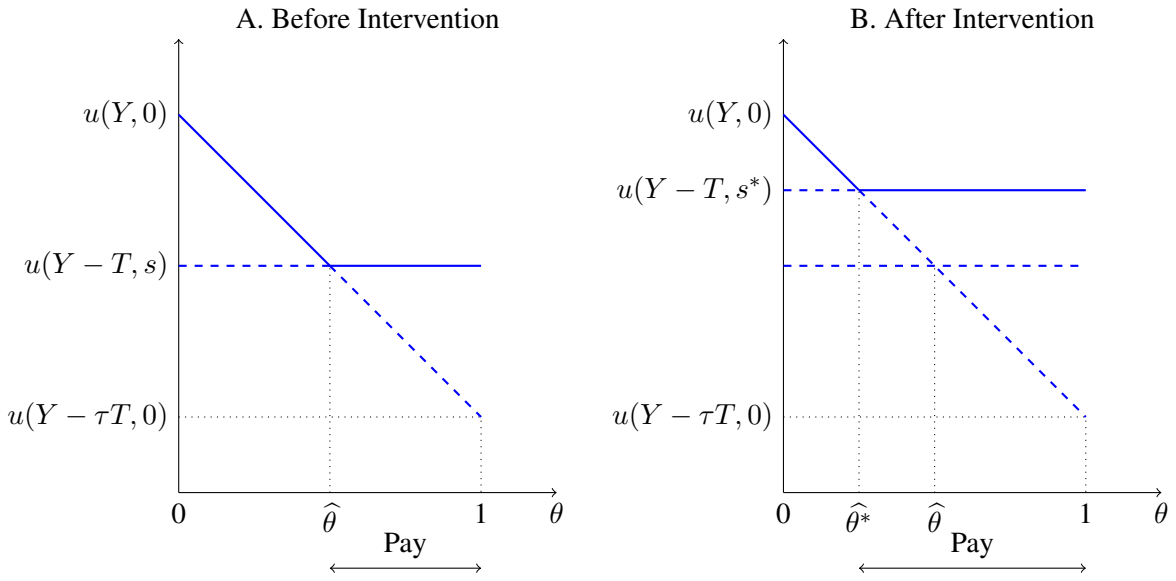
Notes: Table displays the estimate of OLS regression models when outcome Y^v $(v = a, b, c, d, e)$ of property i in month t is regressed on the lottery outcome D and a set of covariates. Sample includes all properties eligibles and non-eligibles to participate in the lottery located in blocks with at least one lottery participant during the post-treatment period (January 2009 - December 2011). Each column in the table corresponds to a different specification. All specifications include the number of eligible properties and total number of properties in block j as controls. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Table 14:
Financial Motive Regressions

Dependent variable: Y^{fm}: l(bill paid between Dec 16 and Jan 12)				
	Model Specification			
	[1]	[2]	[3]	[4]
D_{2008} x K	.0004***	.0008***	.0008***	.0008***
	(0.000)	(0.000)	(0.000)	(0.000)
D_{2008}: l(It's a 2008 bill)	0.005***	0.005***	-0.000	-0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
K: # payments prior to lottery announcement	0.005***	0.005***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
Covariates:				
Characteristics of property	N	Y	Y	Y
Characteristics of tax bill	N	N	Y	Y
Time fixed effects (59)	N	N	N	Y
Number of observations:	7,642,679	7,642,679	7,642,679	7,642,679

Notes: Table displays the estimate of OLS regression models when outcome Y^{fm} of property i in month t is regressed on D_{2008} which takes the value of 1 for 2008 bills and zero otherwise after controlling for a set of covariates. Sample includes all properties in the city from January 2007 to December 2011. Each column in the table corresponds to a different specification. Standard errors have been clustered at the property level and are reported in parenthesis. *, **, and *** denote statistical significance at the 10, 5 and 1 percent level, respectively.

Figure 1:
Tax-Payer Compliance Problem



Notes: The figure illustrates the taxpayer problem described in equation 1. Before the intervention (Panel A), consumers compare $u(Y-T,s)$ with a weighted average of $u(Y,0)$ and $u(Y-\tau T,0)$. The solid line denotes the optimal choice: it is optimal for all taxpayers with $\theta > \widehat{\theta}$ to pay the tax and *vice versa*. Panel B illustrates the effect of an increase in s. Since $s^* > s$, $u(Y-T,s^*) > u(Y-T,s)$ and tax payment rates increase.

Figure 2:
Sidewalk Renovations in Santa Fe

Notes: Pictures above feature lottery winners' renovated sidewalks. The first picture (from left to right and by row) shows a stylized "green line" sidewalk, that contains new trees and the trash receptacle. The second picture shows a narrow sidewalk in the center of the city, where there is no space for the "green line" and there is little to no difference between the renovated sidewalk and the rest of sidewalks. The two pictures in the second row show renovated sidewalks in areas of the city where they are more salient (there is no pavement, neighbors do not have sidewalks, etc.).

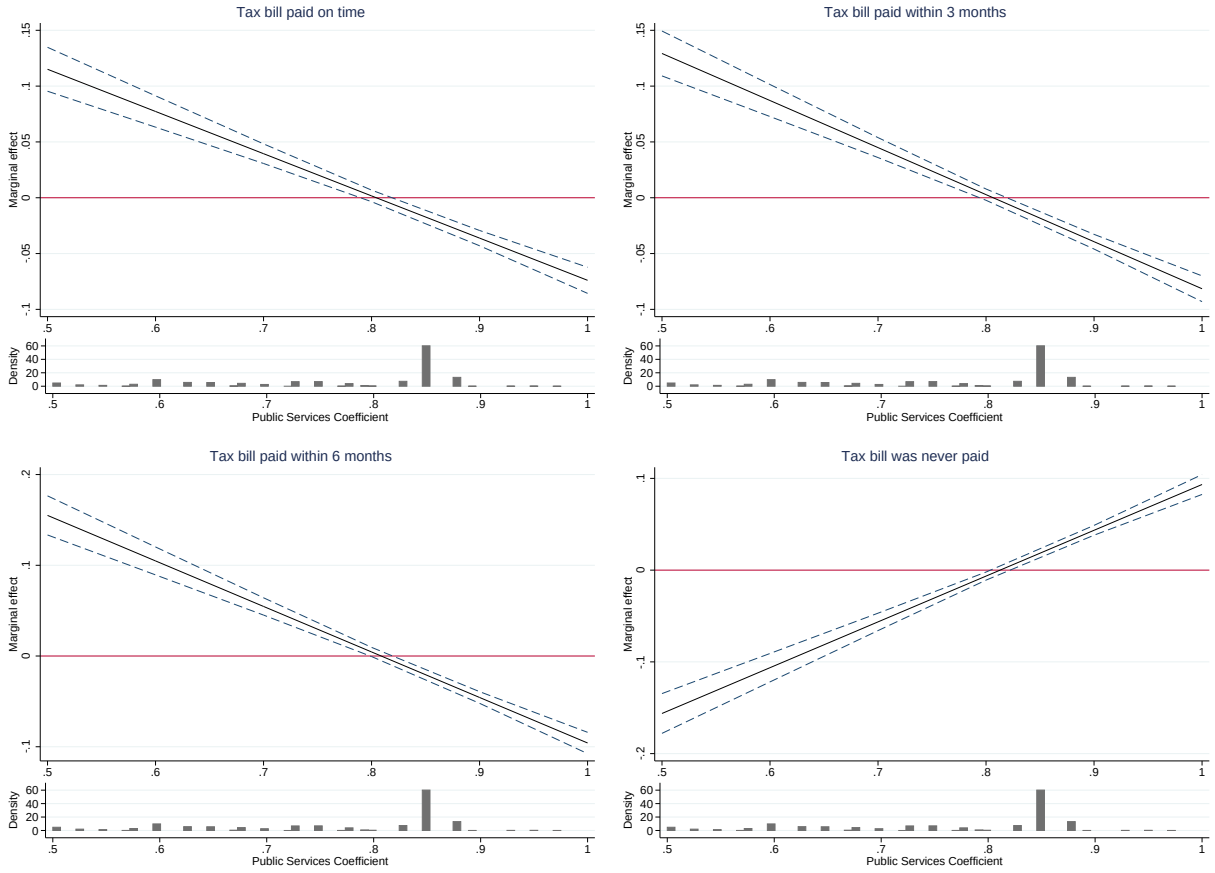
Figure 3:
Spatial Distribution of Lottery Winners

Notes: Each point shows the location of a property that was awarded a sidewalk renovation in the city of Santa Fe.

Figure 4:
Placebo Analysis

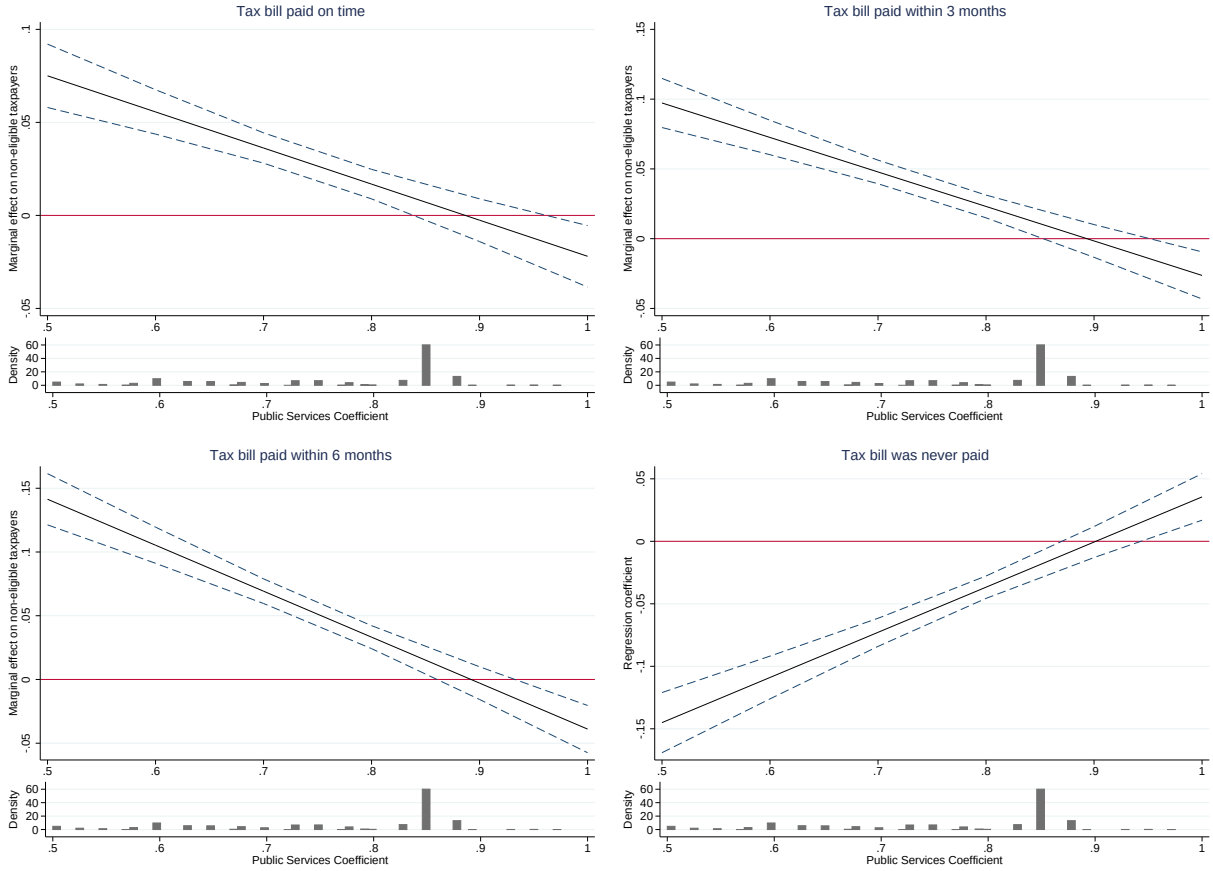
Notes: The graph above shows the results of a permutation test for the effects found in Table 4. The test proceeds as follows. First, we generate one thousand different sets of 400 lottery winners at random without replacement. Second, for each "fake" lottery we estimate the regression coefficient in equation 2. Then, we compare the calculated coefficients with the marginal effect found for the actual lottery (this value cannot fit in the graph) resulting in a calculated p-value of 1/1001.

Figure 5:
Spillovers Marginal Heterogeneous Effects



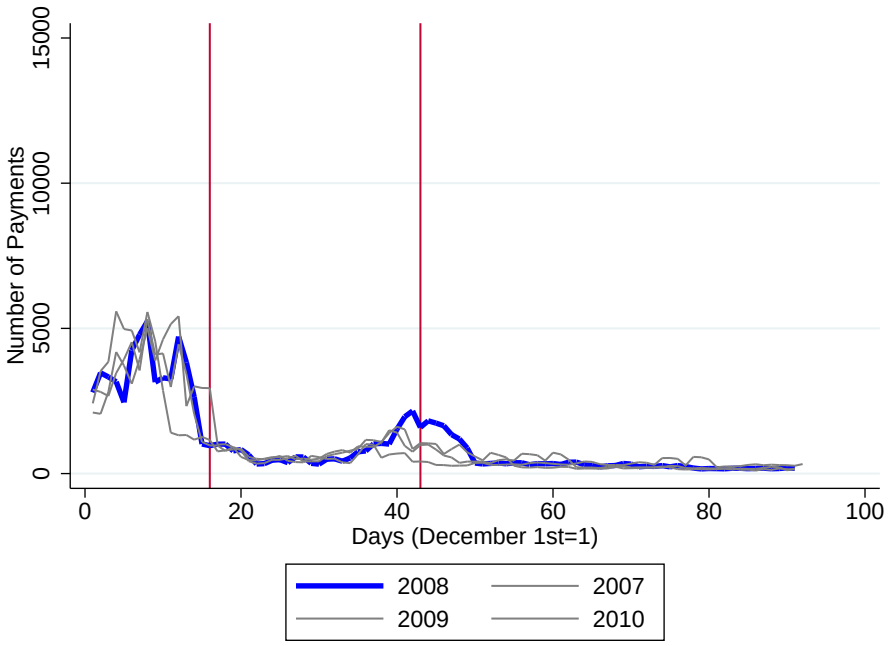
Notes: The graphs above present marginal effect estimates for all taxpayers located in the same block as a lottery winner. All specifications in this graph include the full set of controls. Confidence intervals calculated at 10% significance level.

Figure 6:
Spillovers Marginal Heterogeneous Effects
Triple Interaction



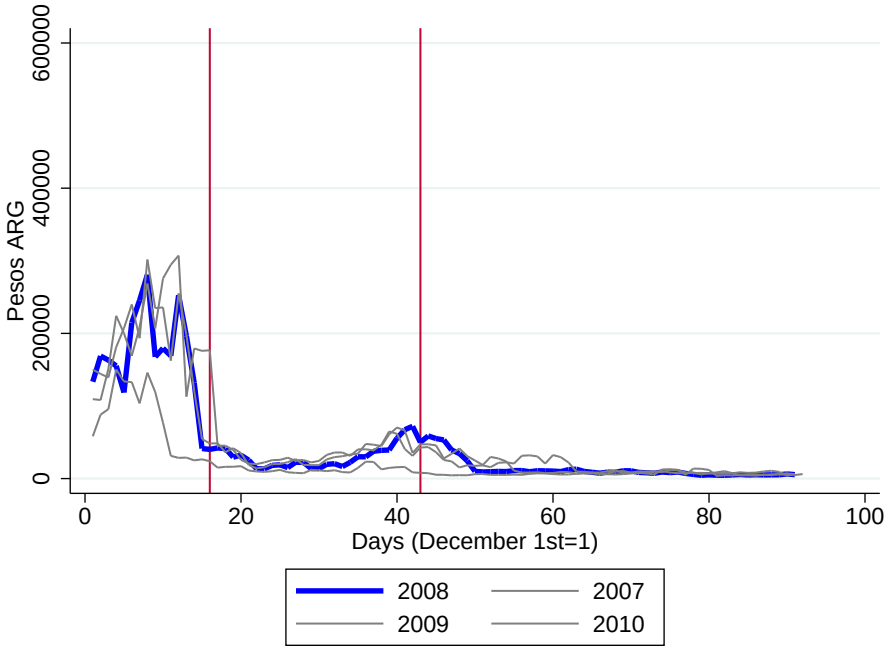
Notes: The graphs above present the marginal effect estimates for Non-eligible taxpayers located in the same block as a lottery winner. All specifications in these graphs include the full set of controls. Confidence intervals calculated at 10% significance level.

Figure 7:
Financial Motive: Number of Payments



Notes: From left to right, the first vertical line denotes the date of the reward announcement while the second line denotes the deadline for paying all previous year tax bills.

Figure 8:
Financial Motive: Amount of Payments



Notes: From left to right, the first vertical line denotes the date of the reward announcement while the second line denotes the deadline for paying all previous year tax bills.