# Is paying taxes habit-forming? Evidence from a tax holiday lottery in Uruguay

Thad Dunning,\* Felipe Monestier<sup>†</sup> Rafael Piñeiro,<sup>‡</sup>

Fernando Rosenblatt,§ and Guadalupe Tuñón<sup>¶</sup>

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<sup>\*</sup>University of California, Berkeley.

<sup>&</sup>lt;sup>†</sup>Universidad de la República, Montevideo, Uruguay

<sup>&</sup>lt;sup>‡</sup>Universidad Católica, Montevideo, Uruguay

<sup>&</sup>lt;sup>§</sup>Universidad Diego Portales, Santiago, Chile

<sup>&</sup>lt;sup>¶</sup>University of California, Berkeley

#### Abstract

Developing states face enduring obstacles to generating tax revenue by punishing non-payment. Can they instead boost compliance by offering rewards to good taxpayers? We study the effects of a randomized lottery in Montevideo, Uruguay, in which the municipal government raffles tax holidays to taxpayers who are current on past payments. Drawing on this natural experiment as well as field and survey experiments in which we provide varied information to taxpayers, we assess (1) whether winning a tax holiday affects future tax compliance; (2) whether knowledge of the lottery policy encourages good taxpayers to keep paying their taxes and bad taxpayers to bring their accounts up to date; and (3) how the impact of this positive inducement compares to the effective of negative incentives, such as fines for non-payment. Using our access to individual tax payment records as well as survey data, we find negative effects of winning the lottery on tax compliance; null effects of information about costs and benefits of payment on behavior; and some evidence that knowledge of the lottery affects attitudes towards taxation and governance. Variation in effects across different types of municipal taxes suggests that breaking the habit of paying taxes may account for the negative effects, a hypothesis we will assess systematically with future interventions.

**Keywords**: Tax compliance, developing countries, state capacity, positive vs. negative incentives; field experiment, natural experiment, information, lottery

## **1** Introduction

Developing countries face enduring obstacles to engendering tax revenue, a key facet of state capacity. To explain these obstacles, researchers often focus on the difficulty of boosting tax compliance using *negative* incentives. Enforcing sanctions for non-payment can pose serious challenges, even where administrative capacity is strong, and even with taxes for which monitoring problems are negligible.

Consider non-compliance with municipal property tax bills in Montevideo, the capital city of Uruguay—a Latin American country thought to have comparatively effective state institutions. Figure 1 shows the average accumulated number of property tax payments owed per taxpayer account (left panel); the average proportion of bills paid on time (middle panel); and the proportion of "good taxpayers," those who have made triennial tax payments on time over the previous year (right panel). In this setting, the appraised value of properties, and thus the obligations of each taxpayer, is known; and in principle, the state could punish non-payment with fines or even seizures of property. Tax compliance is nonetheless markedly low: the municipality routinely classifies only about 50% of taxpayers as "good taxpayers." Our fieldwork suggests a number of factors we discuss later, beyond monitoring problems, that can undermine the enforcement of tax compliance through negative incentives.

These difficulties may explain why governments in Latin America and elsewhere have increasingly turned to *positive* incentives to boost tax payments. Montevideo implemented one of the earliest versions of these policies: since 2004, and across four kinds of taxes (property, vehicle, sewage, and head), the municipal government has randomly selected taxpayers and—conditional on a recent history of good taxpaying—rewarded them with a year free of tax payments. Rather than being punished for poor tax-paying behavior, citizens are therefore rewarded for their history of tax compliance. Municipalities in Argentina, Brazil, Peru, and other Latin American countries now raffle prizes, from televisions to new cars, to taxpayers who pay their taxes on time.<sup>1</sup>

<sup>&</sup>lt;sup>1</sup>We took a random sample of municipalities in 5 Latin American countries (Mexico, Peru, Argentina, Brazil, and Uruguay) and investigated the existence of such a program using municipal websites and follow-up phone calls; Brazil and Uruguay have the largest proportion of such programs, at around 10% and 15% of municipalities, respectively.



#### Figure 1: Non-Compliance Over Time: Property Taxes In Montevideo, Uruguay

Based on a simple random sample of 2,579 property tax accounts in Montevideo, Uruguay. The left panel gives average accumulated debt (payments owed) per account; the middle panel graphs the proportion of accounts paid on time, as of each triennial due date; and the right panel shows the proportion of "good taxpayers," a municipal measure that establishes eligibility for the tax holiday lottery we study in this paper.

The rise of such rewards programs raises several questions. First, do they effectively influence tax payment? Insights from psychology and behavioral economics suggest that promised rewards may have substantially different effects than threatened punishments. Yet, there are also many reasons, discussed in section 2, for why such policies could fail. Second and relatedly, how does the effect of positive inducements compare to the impact of sanctions for non-payment—i.e. do positive or negative incentives more effectively influence compliance?<sup>2</sup> Finally, do rewards policies impact broader attitudes towards taxation and governance? Rewarding good taxpayers may generate a form of "quasi-voluntary compliance" (Levi, 1988), in which the transparency and equity of the policy may itself shift taxpayers' attitudes and behaviors.

These are important questions for both researchers and policy-makers, but given selection problems

<sup>&</sup>lt;sup>2</sup>Researchers have highlighted the possible role of positive and negative incentives such as lotteries and state amnesties (Parle and Hirlinger, 1986; Loftus, 1985; Alm, McKee and Beck, 1990; Falkinger and Walther, 1991; Alm et al., 1990). Yet others have suggested that the costs of implementing governing programs that give taxpayers positive materialistic incentives to comply with tax laws may outweigh the benefits (Smith and Stalans, 1991).

in most observational studies, they are typically challenging to answer. In this paper, we use several strategies to investigate the impact of positive inducements for tax compliance and to compare this effect to the impact of more typical negative sanctions.

First, we leverage the natural experiment provided by Montevideo's randomized tax holiday, and our access to a panel of administrative tax payment records as well as household survey data, to study the impact of winning a tax holiday. The lottery sets up a straightforward comparison, among eligible taxpayers—those with good tax-payment records—between winners and a randomly selected control group of eligible non-winners. Thus, we can assess whether winning the lottery—which not only provides a year free of tax payments but also informs many taxpayers of the existence of the program—influences subsequent tax compliance. Yet, the impact of the program is plausibly greater than these comparisons would suggest, because knowledge of the program may influence citizens to bring their tax accounts up to date (to become "good taxpayers") in order to gain eligibility for tax holidays.

Second, we therefore also utilize a field experiment to assess whether informing citizens, including "bad taxpayers," about the existence of the lottery for property taxes affects subsequent tax compliance. The tax holiday program appears to have been poorly advertised by the municipal government: in our surveys of the control group in our field experiment, only about 8% of respondents could identify the tax lottery as a municipal policy that rewards good taxpayers. In collaboration with the municipal government, we thus mailed flyers stamped with the municipal logo—which appear very much like tax bills themselves—reminding taxpayers of the due date for taxes and providing experimentally varied messages to taxpayers. We compare a control group of taxpayers who receive no flyers to taxpayers who are 1) simply reminded of an impending due date for real estate taxes (Reminder group); 2) also informed of the potential benefit provided by the tax holiday lottery<sup>3</sup>; or 3) also informed of punishments for non-payment of taxes. We also use alternative interventions that prime either 4) the social rationale for the lottery or 5) the social rationale for punishment of non-compliance with taxes. To estimate the impacts of these treatments, we use individual records of actual tax payments in the period after our intervention.

Third and finally, we use survey experiments embedded in our household survey to assess the im-

<sup>&</sup>lt;sup>3</sup>For group 2., we further subdivide the treatment group into those who receive 2A. no information about the probability of winning the lottery and 2B. those who are told their probability of winning (which we compare to taxpayers' priors about the likelihood of winning, estimated from follow-up survey data on the control group).

pact of information about the lottery on attitudes towards tax compliance, as well as broader attitudes about the efficiency of the state and equity of public policy. Our research strategies are thus designed to compare the impact of different ways of manipulating negative and positive incentives for tax payment, thus shedding light on basic motivations for tax compliance, and also evaluate the best means of boosting future impact through informational interventions that effectively advertise the policy.

Our study design has several key advantages. First and foremost, the randomization of the natural and field experimental interventions sets up straightforward, unconfounded comparisons between the treatment groups, aiding inferences about the causal impact of the interventions. Second, we make use of detailed, unobtrusive outcome measures—namely, individualized administrative records on tax compliance and extent of tax debt, as well as survey experimental measures from household surveys. We also make use of a measure of "intended compliance": whether taxpayers accessed their online Web accounts to view a duplicate of their tax bill after receiving our flyers. Third, the experimental realism of our interventions is quite high, since we designed flyers that match the format of tax bills and arrive bearing the municipal logo—just as would real interventions designed to boost compliance by publicizing negative or positive incentives for tax payment. Finally, our tests are quite high-powered: if a large effect of the lottery program exists, our design should reliably detect it. We registered a pre-analysis plan before receiving any data from the municipality and subsequently posted two amendments, one in which we presented code to analyze a mock data set in which we randomly reshuffled treatment labels prior to analysis.<sup>4</sup> Complete results produced using the code in our registered mock analysis will be available in an online Appendix.

We find that far from promoting tax compliance, the tax holiday lottery may inhibit it: among eligible good taxpayers, winning the lottery results in a 3 percentage point reduction in subsequent tax compliance, an effect that lasts for up to three years. Our interpretation is that winning the lottery interrupts the habit of tax payment—a conjecture bolstered by the fact that we do not find a negative effect for types of municipal taxes in which the habit of paying is typically not interrupted. We are assessing this hypothesis more systematically using ongoing interventions, in conjunction with the municipality, and welcome feedback and suggestions for interventions. Meanwhile, our informational

<sup>&</sup>lt;sup>4</sup>See https://www.socialscienceregistry.org/trials/452 and http://egap.org/design-registration/ registered-designs/(design 84: 20140723).

interventions did succeed in boosting access to online tax bills, which we interpret as a measure of intended compliance; among bad taxpayers, the message about punishments also boosted actual tax compliance by around 4 percentage points, relative to a control group that received no flyers. However, neither positive information about the lottery nor negative information about fines and sanctions have any greater impact on intended or actual compliance than a simple reminder that taxes are due. We do find evidence from our survey experiment that information about the lottery impacts attitudes towards the fairness of the tax system, as well as assessments of whether it is "worth it" to be up to date on one's taxes.

On balance, our evidence therefore underscores the difficulties of eliminating tax non-compliance through positive inducements. Yet our findings should have implications for those municipalities that have developed tax lottery programs: for example, they suggest the possible wisdom of raffling up-front rewards such as houses or cars, rather than offering interruptions of tax payment. We return to these interpretations in the conclusion, after discussing the theory that motivates our study, particularly our comparison of negative vs. positive incentives for tax compliance (Section 2); describing our empirical strategy, including our natural and field experimental designs and the timing and nature of data collection (Section 3); and presenting our hypotheses, tests, and results (Section 4).

## 2 Theory and Context

#### 2.1 Enforcing compliance: the limits of sanctions

Engendering tax compliance is a persistent difficulty, especially in developing countries. According to most accounts, states with weak administrative capacity fail to "penetrate" society sufficiently, thereby allowing citizens to avoid payment of taxes by not imposing credible penalties for non-payment. Scholars typically see information and monitoring problems at the heart of the problem, in which lack of state capacity explains failures to elicit compliance. This account relies on an underlying behavioral theory: the decision not to pay taxes is driven by the benefit of evasion, minus the cost of punishment discounted by the probability of detection. The problem, according to this theory, is thus that in developing countries the probability of detection for non-compliance is negligible. Failure to generate tax

revenue is seen as a problem of enforcement that is due in part to weak state capacity.

Yet even in settings of relatively capable administrations—and even when non-compliance is perfectly observable—failure to pay taxes can be high. Consider the case of municipal property taxes.<sup>5</sup> Like the payment of vehicle, sewage, and head taxes—other levies which we study in this paper—here the amount of tax owed is known by the taxing authority, e.g. because the value of assets is appraised. Thus, information and monitoring problems are much less severe than in many settings, such as for the payment of income tax.<sup>6</sup> Nonetheless, enforcement remains a central problem. The municipal government can decide to pursue delinquent taxpayers more or less aggressively—using the courts to expropriate the property of bad taxpayers is often a legal option—yet such maximal solutions are rarely pursued.<sup>7</sup>

In our interviews with tax officials and elected politicians in Montevideo and several other Latin American cities, the difficulties of using legal processes to recover unpaid taxes was readily apparent. In principal, many municipal governments can use judicial proceedings to seize and liquidate or auction properties on which excessive taxes are owed. In practice, such takings are rare, despite the existence of a legal process that terminates in the confiscation of properties for which taxes remain delinquent. At most, an embargo is placed on a property so that it cannot be privately sold until debts are cleared (and this is enforced through the public notary that must regulate any sale). The most typical outcome is instead renegotiation of a debt, perhaps once a court sentence is made, and delinquent taxpayers agree to a payment plan in which past debts are frozen and monthly payments are made. This does not of course suggest that the *threat* of legal proceedings has no effect. A former mayor (and current deputy) from the state of São Paulo, Brazil says "many negotiations … happen before [court proceedings are initiated] … the majority of the legal actions of City Hall [against delinquent taxpayers] fall by the wayside because they were successful before."<sup>8</sup> And a tax official in Tigre, Argentina says "In the last

<sup>&</sup>lt;sup>5</sup>With the exception of Castro and Scartascini (2015) most existing experimental literature on tax compliance focuses on income taxes (e.g. Blumenthal et al. (2001); Slemrod, Blumenthal and Christian (2001); Kleven et al. (2011)), VAT (Pomeranz (2013)), or very specific fees (Fellner, Sausgruber and Traxler, 2013).

<sup>&</sup>lt;sup>6</sup>In this sense, the problem facing the state is akin to that of a credit-card company faced with non-payment by consumers: the value of debt is known, and hiding the amount owed is not an option, but the company can still face substantial barriers to collecting what is owed.

<sup>&</sup>lt;sup>7</sup>A recent paper by Chen (2014) studies variation in the effective tax rate in China and why governments do not collect tax liabilities that are reported by taxpayers.

<sup>&</sup>lt;sup>8</sup>Interview, Geraldo Cruz, current Diputado from San Pãolo and former mayor of Embu das Artes; translation ours.

20 years, we have never auctioned neither a commercial nor a residential property. In general, we end up with an agreement."<sup>9</sup> Yet it does suggest that taxpayers can remain delinquent on their payment of taxes for many years without losing their properties or even paying substantial fines. Renegotiation, meanwhile, poses its own problems: as another Argentine official told us, "Payment agreements don't cure the situation, they generate a new problem for you. Now the person doesn't only have pay the monthly rate but also the quota from the agreement. So one has to manage these agreements so it is not just a qualitative change but also a real quantitative change in terms of collection of the debt. If not the guy comes, makes an agreement with you, and doesn't pay it, and the only thing you did was change an 'administrative' debt into a 'negotiated' debt."<sup>10</sup>

The reasons that tax penalties are not fully assessed—and that court cases can take many years to be resolved, if ever—likely stretch beyond inefficiencies in the judiciary or bureaucratic agencies. It it a truism that collecting taxes can be politically unpopular, but this can be especially salient in the case of municipalities, where face-to-face negotiations with delinquent taxpayers is common.<sup>11</sup> As one tax official in the municipality of Tigre, on the outskirts of metropolitan Buenos Aires, Argentina told us, "the national powers have much greater strength [to collect taxes] than municipal powers ... proximity means that a neighbor can approach the administration to justify why he doesn't pay. By contrast, at the national level nobody will pay him any mind."<sup>12</sup> Indeed, in light of such constraints, Montevideo's city government has issued general amnesties for bad tax payers nearly yearly since 2000 (though for different types of taxes each year).<sup>13</sup> It often renegotiates debt with individual taxpayers as well.

It is in the context of these difficulties enforcing penalties for non-payments—and thus the widespread amnesties for bad taxpayers—that the lottery programs for good taxpayers arise. Some of our interviewees explicitly saw these prize lotteries as complements to sanctions. "We have four programs with positive incentives so that the taxpayer pays. We think it is necessary to act in more than one way to

<sup>&</sup>lt;sup>9</sup>Interview, Daniel Chillo, Secretario de Ingresos Publicos, Tigre, Buenos Aires, Argentina; translation ours.

<sup>&</sup>lt;sup>10</sup>Carlos Maisterrena, Sub-Director de la Administración de Fiscal Municipal, Ciudad de Paraná, Entre Ríos, Argentina; translation ours.

<sup>&</sup>lt;sup>11</sup>On the political costs of enforcement see Holland (2014).

<sup>&</sup>lt;sup>12</sup>Interview, Daniel Chillo, Secretario de Ingresos Publicos, Tigre, Buenos Aires, Argentina; translation ours.

<sup>&</sup>lt;sup>13</sup>The municipality has issued amnesties for different types of taxpayers in 1997 (vehicle, property, and head), 2000 (property and head), 2001 (property and head), 2002 (vehicle, property, head, and sewage), 2003 (vehicle, property, head, and sewage), 2004 (property), 2005 (vehicle, property, head, and sewage), 2008 (vehicle, property, head, and sewage), 2009 (vehicle, property, head, and sewage), and 2013 (property, sewage, head).

achieve the greatest level of compliance ... It is very important to give the taxpayer that does not pay a sense of risk ... and for the taxpayer who pays, a sense that he is recognized." The tax holiday policy we study in this paper was developed by Montevideo's City Hall (Intendencia de Montevideo—IM) and explicitly intends to counter negative perceptions among citizens of forgiveness for non-compliance. The policy was initiated by the center-left government of the Frente Amplio in the context of an amnesty for many delinquent taxpayers following the economic crisis of 2002. The idea was to counteract perceived negative incentives of the tax amnesty.<sup>14</sup> As officials at the IM have told us, the economic crisis generated a dilemma: how to lower the burden for those under dire circumstances while at the same time continuing to promote compliance. The lotteries were their answer.<sup>15</sup> After almost ten years, however, no evaluation of the program's effectiveness in boosting compliance exists.

#### 2.2 Are positive inducements an effective alternative?

Positive incentives for good taxpayers appear increasingly commonplace in Latin America, especially Brazil. To assess the prevalence of such incentive programs, we took a 10% sample of municipalities in Argentina, Brazil, Colombia, Mexico, and Peru, and also evaluated a census of municipalities in Uruguay (which only has 18 municipalities). We coded whether each municipality has a website; and in turn whether, among those with websites, the municipality has a lottery for good taxpayers or offers some other prize. Among 558 Brazilian municipalities in our sample, nearly 24% offer some sort of prize for good taxpaying, such as discounts on tax payments owed; seven percent have randomized tax rebate or prize lotteries. Similarly, 79% of sampled municipalities in Uruguay and 25% in Colombia, 10% in Mexico, 5% in Argentina, and 2% in Peru offer prizes of some sort to good taxpayers. The frequency of lotteries outside of Brazil is lower, and the 10% sample offers a smaller *N* of municipalities, so inferences should be drawn with caution. Nonetheless, our initial investigation does suggest the prevalence of positive incentive programs for good taxpayers.

Municipal administrators in Argentina, Brazil, and Uruguay whom we interviewed do believe that

<sup>&</sup>lt;sup>14</sup>For discussion, see http://historico.elpais.com.uy/Suple/LaSemanaEnElPais/04/10/29/lasem\_ciud\_ 118264.asp; http://www.montevideo.com.uy/notnoticias\_66228\_1.html; and http://www.180.com.uy/ articulo/14284. Accessed May 2, 2015.

<sup>&</sup>lt;sup>15</sup>In October 2013, the municipality announced a renewed amnesty for certain bad taxpayers, underscoring the difficulties of cracking down on non-compliance. There have been amnesties in 2004, 2008, and 2010, among other years. See also URLs in previous note.

these positive inducements have a substantial effect on compliance. An interviewee in Tigre, Argentina noted, "Today with the Municipal Service Tax, we have a compliance rate of 85%, whereas when we started [the lotteries] in 2009, the compliance rate was at 68%."<sup>16</sup> A former Brazilian mayor noted, "The effect of [starting the lottery] meant that by the second year, many indebted people went to look for payment plans [so that they could become eligible for the lotteries]. After the results of these ….lotteries were shown, a lot of municipalities adopted these policies."<sup>17</sup> An Argentine bureaucrat noted that putting in place a lottery "had two types of effects. One was that people came to regularize their debt. But what happened is that … they could not pay their debt and they made a payment plan. But one of the conditions [to participate in the lottery] was not to be in a payment plan. What did people say? 'Make me a payment plan with six quotas, I'll finish the sixth and enter [the lottery]."<sup>18</sup>

Yet, why would such positive inducements work? A simple formalization of the decision problem facing a taxpayer highlights the difficulty of encouraging tax compliance, either through punishments or the provision of material benefits. Let *y* be an asset value, *t* be the annual tax rate, and *z* be the unpaid annual amount of taxes due; with full nonpayment, z = ty. The expected utility of full nonpayment in any year is thus

$$z - pc, \tag{1}$$

where *c* is the penalty for nonpayment and *p* the probability of punishment. In the setting we study, the cost of punishment *c* could include (1) fines and interest charges for delayed payments, and ultimately (2) losing one's house or other property.<sup>19</sup> However, taxpayers may count on individual renegotiations or on the possibility of a future amnesty for bad taxpayers, which here occur with probability (1 - p); and the fine for nonpayment *c* is often small. Under the lottery policy we study, by contrast, good taxpayers win a year free of tax payments with probability 1/5,000 in any tax payment period. Thus, the expected utility of paying the full year's taxes this year is (without discounting)

$$\frac{1}{5,000}z - z.$$
 (2)

<sup>&</sup>lt;sup>16</sup>Interview, Daniel Chillo, Secretario de Ingresos Publicos, Tigre, Bs As, Argentina, translation mine.

<sup>&</sup>lt;sup>17</sup>Beto Trícoli, Prefeito de Atibaia (2001-2008), Brazil.

<sup>&</sup>lt;sup>18</sup>Interview, Carlos Maisterrena, Sub-Director de la Administración de Fiscal Municipal, Ciudad de Paraná, Entre Ríos, Argentina; translation ours.

<sup>&</sup>lt;sup>19</sup>As noted, property owners also cannot sell their houses until clearing their property tax accounts.

Note, then, that in the case of full year's debt, a taxpayer has to pay a year's worth of taxes *z*—in order to gain a year free of tax payments *z* in the following year, but only with probability 1/5,000! Indeed, comparing (2) with (1), we see that paying taxes is only optimal when

$$pc > \frac{9,999}{5,000}z.$$
 (3)

In words, the cost of the penalty discounted by the probability of punishment must be essentially as large as two full years of taxes due on the asset.

This simple framework suggests that manipulating benefits may have a bigger effect on *marginal taxpayers*, in terms of pushing them over the threshold to compliance. Such taxpayers may be, e.g., those who owe less (e.g. for whom the amount owed z < ty), that is, "bad taxpayers" who are not very delinquent on their taxes; or "good taxpayers at risk," i.e., those who are classified as good taxpayers at the moment of a lottery but have been bad taxpayers in the past.<sup>20</sup> We discuss and test such hypotheses about heterogeneous effects for such marginal taxpayers further in Section 4.

Yet, this framework also makes clear that the *material* rewards of the lottery may be unlikely on their own to induce much additional compliance, especially among bad taxpayers. Among property owners, the average appraised property value is US\$36, 035 (956,000 Uruguayan pesos) and the annual value of property taxes is over US\$265 (7,044 Uruguayan pesos), which is non-trivial.<sup>21</sup> However, the *expected* value of winning any lottery is then US\$265/5,000, or about five US cents. For virtually all delinquent taxpayers, then, the expected monetary value of rectifying accounts is likely to be negative. To be sure, casinos would make no money if gamblers did not take bets with negative expected values. Still, material incentives alone seem unlikely to cause many bad taxpayers to bring their accounts up to date.

A natural question that arises is: does manipulation of the expressive or perceived benefit of paying taxes further influence compliance? Let *b* be the perceived benefit of paying taxes, in terms of the social return or legitimacy of taxation. The expected utility of paying taxes is then (1/5, 000)z - z + b,

<sup>&</sup>lt;sup>20</sup>Our pre-analysis plan suggested other hypotheses that we are not able to explore, for example, that those with extreme asset values or those who overestimate the probability of winning p might be more likely to be induced to pay by the existence of the program. Previous experimental results suggest compliance occurs at least partly because individuals overweight the probability of an audit (Alm, McClelland and Schulze, 1992).

<sup>&</sup>lt;sup>21</sup>We estimate this value from the control group in our natural experiment.

so tax payment occurs whenever  $pc > \frac{9,999}{5,000}z - b$ , which is satisfied more easily than is (3). Depending on the size of *b*—the perceived benefit of paying taxes—it could be incentive compatible for taxpayers to comply, even when *pc* is small.

Our theory is indeed that, in addition to providing a direct benefit—which is vanishly small in expectation—Montevideo's lottery may also affect non-material benefits *b*, e.g. by influencing perceptions of the fairness and equity of taxes and/or the individual normative benefit of paying taxes. Several interviewees pointed to exactly these kinds of nonmaterial benefits of the lottery. For example, some flagged the importance of the lottery's *transparency*. In one Argentine municipality, an official noted that "in the lottery, to give it legality, there are two city councilors and two functionaries from other departments of the municipality present. And in the same lottery appears the winner from the monthly lottery immediately prior—so we do the drawing of lots and then give a TV to the person who won the preceding lottery."<sup>22</sup> As one Brazilian mayor put it, "the best weapon [against non-compliance] is transparency."<sup>23</sup>

The public nature of this lottery also underscores an additional theme, *recognition*.<sup>24</sup> One official emphasized "the direct contact with the taxpayer who has always complied, that is not common. Because tax administrations in general do not have contact with those who pay in an adequate [rather than delinquent] way, so for us this is a way of recognizing them ... we take a photo and put the program on the webpage of the municipality, we publish a list of the taxpayers among whom we do the lottery ... we do not seek to give [the winning taxpayer] value but rather recognition"<sup>25</sup> Another interviewee noted, "every time we award prizes we make a kind of show. In some way this helps us to create a tax-paying culture. People participate ... and it is good communication ... it is not worth it to hit unless you caress those you need to caress ... we take these two tacks: punish those who do badly and reward those who do well."<sup>26</sup>

Finally, some interviewees point to the benefits of lotteries in terms of generating legitimacy as well as *political support* for taxes. As a popular and transparent benefit policy, lotteries might engender

<sup>&</sup>lt;sup>22</sup>Interview, Daniel Chillo, Secretary of Public Revenues, Tigre, Bs As, Argentina.

<sup>&</sup>lt;sup>23</sup>Interview, Geraldo Cruz, current Diputado from San Pãolo and former mayor of Embu das Artes; translation ours.

<sup>&</sup>lt;sup>24</sup>Previous studies suggest that social incentives and peer effects may be an effective way to improve compliance. For example, Chetty, Mobarak and Singhal (2014) study Bangladeshi firms and find that the threat of exposing information about all firms' tax payment behavior led to a positive behavioural response and an increase in tax compliance.

<sup>&</sup>lt;sup>25</sup>Interview, Daniel Chillo, Secretario de Ingresos Publicos, Tigre, Bs As, Argentina.

<sup>&</sup>lt;sup>26</sup>Carlos Maisterrena, Sub-Director of Fiscal Administration, Ciudad de Paraná, Entre Ríos, Argentina.

support for incumbents and therefore greater compliance. To explain non-payment, one interviewee noted that "compliance has a lot to do with the vote intention of the incumbent government. If you have a high level of compliance that implies people will support the incumbent in the election. Those who do not pay are those who do not support."<sup>27</sup> Thus, theory as well as our field research suggest that lotteries may induce greater tax payments not only by generating material rewards to compliance but also by shifting political attitudes, for example, attitudes about the equity and fairness of the tax system.

In our empirical work, we probe these possibilities using household surveys, especially survey experiments embedded in our instrument. However, we must first assess whether winning lotteries does induce greater future compliance among those who are already good taxpayers; and whether knowledge of lotteries can cause bad taxpayers to comply at greater rates. In the next two sections, we describe our empirical strategy and outcome measures, before turning to our results.

## **3** Empirical Strategy and Design

To select taxpayers for holidays, the government uses the results of Uruguay's National Lottery, which posts online five random digits that indicate winning lottery numbers. Taxpayers whose four-digit IDs correspond to the final four digits of the winning National Lottery numbers are selected as as provisional winners.<sup>28</sup> For head and sewage taxes, the municipality grants such holidays six times a year (February, April, June, August, October, and December); for vehicle and property taxes, rebates are given three times a year (March, July, and November), in correspondence with the due dates for the respective taxes. Because two lotteries are held in the interim between property and vehicle tax payments, for those taxes the probability that any taxpayer wins a year free of taxes in any tax period is 1/5,000.<sup>29</sup> However, some taxpayers who would have won a tax lottery are not good taxpayers or physical persons or do not present themselves to the city government after being notified. The

<sup>&</sup>lt;sup>27</sup>Interview, Daniel Chillo, Secretary of Public Revenues, Tigre, Bs As, Argentina.

<sup>&</sup>lt;sup>28</sup>The randomization occurs through the selection from balls from an urn, as described in Spanish at http://www.loteria.gub.uy/Juego\_Loteria.php. For an example of posted lottery results, see http://www.loteria.gub.uy/ver\_resultados.php?vdia=21&vmes=6&vano=2013. Winning taxpayer numbers are posted at http://www.montevideo.gub.uy/sorteosBP/pages/sorteosBuenosPagadores.xhtml. Accessed May 2, 2015.

<sup>&</sup>lt;sup>29</sup>The probability of winning one lottery is 1/10,000, since winning numbers are selected at random from all four-digit IDs. The probability of winning both lotteries is vanishingly small, so the probability of winning at least one is essentially 1/5,000.

municipal bureaucracy first screens winning account numbers to identify eligible winners–i.e., "good taxpayers" whose tax accounts who owe no tax debts and have paid on time in the previous year.<sup>30</sup> To identify physical persons, the municipality then sends a letter to eligible winners indicating that they should come to City Hall to register for a year free of tax payments.

#### **3.1** Natural experiment: the effect of winning a tax holiday

The design of the lottery allows us readily to estimate the effects of winning a tax holiday, among good taxpayers. In particular, we use a time-series panel of administrative data (2004-2013) to assess the effects of winning the lottery on subsequent tax payments, comparing the payment history at t + 1, t + 2,  $t + 3 \dots$  of lottery winners to a control group of eligible non-winners, where t is the year (or portion of year) in which winners won the lottery.

Random assignment of the tax holiday treatment among eligible taxpayers gives us a strong basis for causal assessment. However, constructing appropriate treatment and especially control groups requires some care. For the treatment group, we received tax payment data for all taxpayers with winning tax account numbers since 2004. Although we use data on eligible winners to estimate treatment effects, we also requested data on ineligible taxpayers with winning numbers ("faux winners"), which we use for placebo tests. For the control group, note first that the right counterfactual group for winners of a particular lottery are taxpayers who were eligible to win as of the date of *that* lottery, based on being current on their tax payments over the previous year—but who did not win. (If we were instead to use a control group consisting of currently eligible taxpayers today, the treatment group would include only taxpayers who were eligible to win as of the date of each lottery, while the control group would include a mix of taxpayers who were eligible and ineligible as of each lottery's date. This asymmetry would risk bias, if potential outcomes are related to taxpayer type). To construct the right counterfactual group, we thus randomly generated a single four-digit number for *each* lottery since 2004. The municipality then located all taxpayer accounts with IDs ending in these numbers, then identified which of these selected taxpayers were eligible to win the corresponding lottery, based

<sup>&</sup>lt;sup>30</sup>Our use of the term "good taxpayers" should not be understood in a normative sense: this is precisely the term the municipality uses, e.g., its lottery is publicly called "Lottery for Good Taxpayers." One might accuse us of "seeing like a state" by adopting this term—but indeed this is exactly how the state "sees" in this context.

on their tax compliance over the previous year at the time the lottery took place. We use the eligible non-winners to estimate treatment effects and retain the ineligible non-winners for placebo tests. Our procedure for constructing the control group thus mimics the random process that created the treatment group of lottery winners. In addition, both the treatment and control groups are random samples from the population of good taxpayers in Montevideo, as of the date of each lottery.

Table 3.1 depicts the number of lottery winners and non-winners, distinguishing between good and bad taxpayers as well as the type of tax. There are 3,189 lottery winners across all four types of taxes and 3,174 eligible non-winners, for a total N=6,363 in the natural experiment. In addition, we have data on 6,150 bad taxpayers whose taxpayer IDs would have made them eligible to win a tax holiday, were they up-to-date on their payments, and 6,108 bad taxpayers whose taxpayer IDs match those in our control group. Note that the treatment and control groups are approximately the same size, which is typically the most efficient design conditional on the overall size of the study group.<sup>31</sup> Here we in fact have a series of mini-natural experiments, in which each lottery generates a treatment group of winners and a control group of non-winners. Thus, the random assignment is effectively blocked by individual lottery; however, the probability of winning is the same in every block. Note also that our procedure naturally distributes the study group across the four types of taxes in proportion to the prevalence of winners of lotteries for each tax, as well as good and bad taxpayers. Our non-winning numbers also give us a random sample of taxpayer accounts that we can use to characterize features of the taxpaying population; for example, we combine good taxpayers (N=1,354) and bad taxpayers (1,225) to obtain the simple random sample of 2,579 property tax accounts used in Figure 1.

The groups generated by the randomization are spread throughout the municipality in similar ways (see Appendix, Figures 26 and 27), and randomization generates exchangeability between the groups. Table 3.2 reports formal balance tests on pre-treatment covariates, such as previous tax compliance. (Here, to generate higher-powered tests, we use data on both good and bad taxpayers, since both types are randomized to the winning and non-winning lottery numbers). None of the individual *p*-values for the differences of means approach nominal significance levels. Table 3.3 also shows a placebo outcome test: the "effect" of assignment to a winning number for bad taxpayers—who were not eligible for and

<sup>&</sup>lt;sup>31</sup>Due to our sampling method, the size of the treatment and control groups are random variables; however, this will not lead to bias in treatment effect estimators due to independence of the denominator and the ratio of the numerator to the denominator, in estimators of treatment effects such as the average causal effect.

Tax	Taxpayer Type	Lottery Non-Winners	Lottery Winners
Property	Good Taxpayer	1354	1339
Vehicle	Good Taxpayer	375	391
Sewage	Good Taxpayer	404	452
Head	Good Taxpayer	1041	1007
Property	Bad Taxpayer	1225	1211
Vehicle	Bad Taxpayer	1924	1899
Sewage	Bad Taxpayer	939	915
Head	Bad Taxpayer	2062	2083
All Taxes	<b>Good Taxpayer</b>	3174	3189
All Taxes	Bad Taxpayer	6150	6108

Table 3.1: Natural Experiment: Sample Sizes

The table depicts the sample size of lottery winners and non-winners in the natural experiment. Rows used to estimate treatment effects are in **bold**; non-bolded rows are used for placebo tests.

did not in fact receive any benefit from winning the lottery. We return to such tests of the design in

Section 4.

Table 3.2: Balance Tests on Pre-Treatment Covariates: Good Taxpayers in Natural Experiment

	Control Mean	Difference	SE_Diff	N	p-value
Good Taxpayer in $t = 0$	0.340	0.003	0.007	18621	0.708
Good Taxpayer in $t = -1$	0.347	0.001	0.007	18309	0.934
Good Taxpayer in $t = -2$	0.345	0.002	0.007	17991	0.823
Good Taxpayer in $t = -3$	0.346	0.002	0.007	17670	0.769
Good Taxpayer in $t = -4$	0.345	0.005	0.007	17350	0.514
Retiree	0.008	-0.001	0.002	5129	0.655
2004 Property Value (pesos)	1,259,241.960	-199,730.648	164,141.207	13,462	0.224

#### 3.2 Field experiment: positive vs. negative incentives

The comparison of lottery winners and losers likely underestimates potential program impact, because the lottery may induce some bad taxpayers to bring their accounts up to date to gain eligibility for the lottery. Moreover, the lottery has apparently not been effectively advertised by the municipal government, despite its fairly longstanding existence since 2004. In our household survey described later, only 8% of respondents identified the lottery as a municipal policy that rewards good taxpayers, while only

	Control Mean	Difference	SE_Diff	Ν	p-value
Bad Taxpayer $t = 1$	0.102	0.005	0.005	11803	0.246
Bad Taxpayer $t = 2$	0.116	0.000	0.005	11052	0.970
Bad Taxpayer $t = 3$	0.122	0.006	0.007	7371	0.425
Bad Taxpayer $t = 4$	0.128	0.011	0.008	5998	0.171
Bad Taxpayer $t = 5$	0.130	0.014	0.009	4652	0.141
Bad Taxpayer $t = 1 - 5$	0.114	0.003	0.005	10254	0.597

Table 3.3: Placebo Tests on Post-Treatment Variables: Bad Taxpayers in Natural Experiment

Here, "t = 1" refers to the first date of payment at which we can compare treatment and control groups in a symmetric way. Depending on the tax, this date is up to 2 years after the relevant lottery, given that taxes are exonerated for one year, and for some taxes or taxpayers the exoneration does not take place immediately.

5% of survey respondents know someone who has won the lottery.<sup>32</sup> This raises a series of questions. If Montevideo—or another municipal government—were to launch a campaign to inform citizens about the existence of the rebate lottery, what sorts of messages would be most effective in boosting tax payments? And would it be more effective to emphasize the benefits of compliance, such as eligibility for the tax holiday lottery, or should the municipality instead emphasize punishments for non-compliance? And finally, what mechanisms may explain how benefits or punishments influence compliance?

To answer these questions, and to probe basic motivations for paying taxes, we use a field experiment in which we provide varied information to a random sample of taxpaying households. Unlike the natural experiment—which is limited to a comparison among "good" taxpayers, i.e., those who won the lottery and those who were eligible to win the lottery but did not win a tax holiday—the field experiment allows us to study impacts for both good and "bad" taxpayers, i.e., those who were not eligible for the tax holiday lottery as of the time of our field experimental interventions. Moreover, for both types of taxpayers, our informational experiments allow us a unique opportunity to compare the effects of positive incentives provided by the lottery to the effects of negative incentives such as punishments for non-payment—and to track the impact of these messages using actual tax payment data.

To create our study group for the field experiment, we worked with the municipal bureaucracy to draw a random sample of administrative tax payment records. The municipal employee in the infor-

<sup>&</sup>lt;sup>32</sup>These percentages are drawn from the placebo control group in our field experiment, who were reminded that tax bills are due but were not informed about the existence of the lottery (N=412).

mation technology division of the tax office with whom we worked set a seed, sequentially generated random numbers of the same length as taxpayer account ID numbers, and then drew the tax records corresponding to those account numbers, aiming to generate a stratified random sample with equal numbers of good and bad taxpayers. (Note that that the average value of the "good taxpayer" variable is around 53%, per Figure (1); in point of fact, we ended up with a somewhat larger group of good taxpayers).<sup>33</sup> Here, a good taxpayer is one who was eligible for the tax holiday lottery as of March 2014, according to the criteria discussed above. We also filtered out certain tax payers who pay their property tax bills for the whole fiscal year in March or November (and thus would not conceivably be influenced by our field experimental intervention in June-July 2014), and taxpayers who are not in fact issued bills. Due to these restrictions, the population from which our household survey and administrative data samples are drawn should be conceptualized as "all taxpaying households with bills due in July 2014." We verified that none of our sampled taxpayers had actually won a past lottery for exoneration of payment, since our goal was assess the effect of informing taxpayers about the possibility of exoneration.<sup>34</sup>

One might imagine that bad taxpayers are concentrated in poorer, outlying areas of Montevideo. In fact, our random sample of taxpayer accounts suggests otherwise: per Figure 2, good and bad taxpayers are evenly spread throughout the city. By this indicator, socioeconomic status or property value is not systematically related to eligibility for the tax holiday lottery.

<sup>&</sup>lt;sup>33</sup>The group of sampled accounts for bad taxpayers had higher rates of taxpayers with invalid addresses or due dates, to whom the municipality did not in fact issue bills.

<sup>&</sup>lt;sup>34</sup>Our flyers/informational treatments were generated, addressed, and then distributed by a company we hired beginning on June 27, 2014 (Phase I). On July 7, we received from the postal service a list of addresses where these flyers were not received (so we could replace these addresses for purposes of the household survey). We began the Phase II (A and B) mailings of flyers on July 5, targeting households with intermediate tax due dates for Phase II.A and the latest tax due dates for Phase II.B. We can be confident that the households in our study group received our informational treatments before the due date for paying taxes—and in most cases, before the physical receipt of tax bills. Mailing of tax bills by the municipality began on July 1 and continues until July 21 in a staggered fashion, according to which different zones of the city receive the tax bill at different times. The due date for tax payments is also staggered and follows the order of delivery of tax bills, with the due data approximately 8 days after the receipt of bills. We were not initially aware of how the staggering would occur, and the city zones used by the municipality do not correspond to postal codes; thus, we did not time the mailing of our informational treatments to coincide exactly with the arrival of the tax bill. However, all households in the Phase I mailings of informational treatments should have received flyers before July 8, the earliest possible due date. Moreover, for Phases II.A and II.B, we gathered information on due dates for each household from the municipality and grouped households into Phases II.A and II.B accordingly, to ensure that flyers would arrive in advance of due dates and (usually) tax bills. The greater risk is that in some cases (especially Phase I mailings) our flyers could arrive several days or even weeks in advance of tax bills and due dates; however, have data on the date at which bills were received and the due date for payments.



Figure 2: Field experiment: property plots of winning account numbers (treatment and control groups) The figure depicts good (green, N= 14,784) and bad (blue, N=13,862) taxpayers in the field experiment.

We then randomized these taxpayers with equal probability to one of six treatment groups, as depicted in Table 3.4. We collaborated with the municipal government to design and mail flyers printed with different messages, according to treatment assignment. Figures 9-14 depict the flyers and show their Spanish-language messages; Figure 15 shows the back of the flyers, which displays the municipal logo.<sup>35</sup> Our baseline reminder reads as follows:

#### 1. Reminder:

Dear neighbor:

We want to remind you that **the second payment of property taxes is due in July**. If you have not received your bill, you can obtain a duplicate copy on our web site (www.montevideo.gub.uy).

To this baseline reminder—a kind of placebo control condition—we added various messages regarding the benefits of compliance—i.e., informing taxpayers of the existence of the tax holiday

<sup>&</sup>lt;sup>35</sup>The design of the flyers and the messages printed on them were authorized by the municipal government, and our experimental protocol was approved by Berkeley's institutional review board.

Treatment condition	Sample of eligibles	Sample of ineligibles
	(Good taxpayers)	(Bad taxpayers)
0. Control	Admin. Data, N=7,243	Admin. Data, N=3,412
1. Reminder	Admin. Data, N=1,532	Admin. Data, N=2,080
	(+ Surveys, N=386)	(+ Surveys, N=353)
2A. Reminder + Lottery/Individual	Admin. Data, N=767	Admin. Data, N=1,050
Reward	(+ Surveys, N=181)	(+ Surveys, N=166)
2B. Reminder + Lottery/Individual	Admin. Data, N=751	Admin. Data, N=1,043
<b>Reward + Probability of</b>	(+ Surveys, N=197)	(+ Surveys, N=141)
Winning		
3. Reminder + Individual	Admin. Data, N=1,465	Admin. Data, N=2,109
Punishment	(+ Surveys, N=377)	(+ Surveys, N=343)
4. Reminder + Lottery	N=1,519 (Admin. Only)	N=2,057 (Admin. Only)
+ Social Benefit*		
5. Reminder + Social	N=1,507 (Admin. Only)	N=2,111 (Admin. Only)
Punishment*		
TOTAL N	Admin. Data, N=14,784	Admin. Data, N=13,862
	(+ Surveys, N=3,000)	(+ Surveys, N=3,000)

Table 3.4: Field Experiment: Treatment Conditions and Sample Sizes

Total N=6,000 (Survey data); N=28,646 (Administrative data). \* For these conditions, only administrative outcome data were gathered.

lottery for good taxpayers—or punishments for non-compliance. t.<sup>36</sup> For example, our second

condition adds language regarding the existence of the lottery and emphasizes the individual reward

the lottery offers:

#### 2A. Reminder + existence of lottery + individual reward

The municipal government of Montevideo wants to reward good taxpayers. **If you pay on time, you will be automatically entered in a lottery to win a year free of property tax payments.** Lotteries occur every other month of the year in conjunction with the National Lottery. The winners will be duly informed and the results of the lottery will be published on the web site of the city government. **You can be the next winner!** 

A variant of this treatment adds to 2A the correct probability of winning the lottery:

#### 2B. Reminder + existence of lottery + probability of winning + individual reward:

#### In each lottery, 1 of every 5,000 households receives this benefit.

<sup>&</sup>lt;sup>36</sup>Several studies to date find a positive result for reducing tax evasion by increasing the rates or salience of penalties and audits. See for example Slemrod, Blumenthal and Christian (2001); Kleven et al. (2011); Castro and Scartascini (2015).

However, as specified in our pre-analysis plan, our analyses often pool 2A and 2B into a single "existence of lottery + individual reward" condition.

Next, our "individual punishment" condition repeats the baseline reminder in 1, then adds language about fines and charges for non-payment:

#### 3. Reminder + Individual punishment:

Those who do not pay on time may be subject to fines and charges. The municipal government of Montevideo may take legal and administrative actions to enforce the rules where applicable. **Pay on time, avoid fines and charges!** 

Finally, we have two conditions that prime the *social* benefits of the lottery as well as the social justification for punishing non-compliance, respectively. The social benefit condition repeats 2A and then adds the following:

#### 4. Reminder + existence of lottery + social benefit:

The municipal government of Montevideo conducts this lottery to recognize good taxpayers for their contribution to constructing a city that is more just and better for all.

The social punishment condition repeats 3 and then adds the following text:

#### 5. Reminder + social punishment:

## Fines and charges are a sanction to those who do not pay their taxes and do not contribute to constructing a city that is more just and better for all.

The experimental realism of our treatments is very high: when folded for mailing, the municipal logo is visible, and in fact the flyers appear identical to municipal tax bills before being opened. This is useful because the municipality sometimes prints messages to taxpayers on the inside of tax bills, and an informational campaign about the lottery would likely make use of this medium.<sup>37</sup> The experience of receiving our flyers stamped with the municipal government's logo would thus be similar to the experience of receiving a tax bill on which the municipality prints encouragements to pay taxes. This maximizes the connection between our treatments and the potential policy intervention, the impact of

which we would like to assess.

<sup>&</sup>lt;sup>37</sup>For our experiment, we opted to send separate flyers that look like tax bills due to logistical considerations: rigorously ensuring that messages printed on each actual tax bill would follow treatment assignment proved infeasible. Moreover, we worried that the impact of the messages could be diluted by other messages included on the bills. Finally, the space on tax bills was too limited to include routine messages from the municipality as well as our own treatments.

Figure 3 shows a visual depiction of balance across our treatment groups on a highly prognostic covariate, the pre-treatment value of which is one of our key outcome variables: whether the tax bill was paid on time, at each due date between March 2009 and November 2013.<sup>38</sup> The plots distinguish between bad taxpayers (those ineligible to win the tax holiday lottery as of March 2014) and good (eligible) taxpayers. Several features of these plots are noteworthy. First, all good taxpayers had paid their taxes on time in July and November 2013-as required to be registered as good taxpayers in March 2014. Thus, the proportion who paid on time reaches 1.0 in those periods (right panel), just as it should. Second, note that those who were eligible in March 2014 tend to be "good" taxpayers more generally—on average, these taxpayers paid their taxes promptly over 90% of the time across the entire period. In contrast, the "bad" taxpayers paid on time between 45% and 50% of the time on average, though this dips somewhat in the most recent period (left panel). This suggests the existence of a structural category of "bad taxpayer," something we explore in more detail elsewhere. Finally, and most importantly, notice that the trend in both graphs is very similar across all six treatment conditions and the control group. This suggests that randomization worked: it created equivalence between the treatment groups on this important pre-treatment covariate, and very likely balanced other observable and unobservable variables as well. We show a similar plot in the online online Appendix for the variable measuring accumulated missed payments.

<sup>&</sup>lt;sup>38</sup>The data for March 2014 are complicated because of the timing at which tax eligibility for that tax holiday lottery is calculated for some taxes; that date is therefore omitted from the plot. For more details, see the amendment to our pre-analysis plan.



Figure 3: Pre-Treatment Balance: Paid Bill On Time

The plots show the average value of "Paid Bill On Time," which records whether taxpayers paid the property tax bill on time, for every tax due date between January 2009 and July 2013, inclusive. Property tax bills are paid in March, July, and November. Data are presented separately for "bad taxpayers" (left panel) and "good taxpayers" (right panel) in our study group. The latter are eligible for a tax holiday lottery as of March 2014, based on being up-to-date on tax payments in the most recent periods.

#### **3.3** Survey experiment: effects on attitudes

We initially intended to trace the impact of our flyers on attitudes using household survey data, comparing survey responses according to treatment assignment in our field experiment. However, this provided infeasible. We hired a reputable Uruguayan firm, CIFRA; the survey was fielded beginning on July 15, 2014, and we believed it would continue through approximately August 20. The survey firm intended to locate physical taxpayers using addresses in taxpayer accounts we obtained by the municipality. There were several difficulties, especially that the taxpayer accounts included both physical persons as well as firms, and that the mailing address associated with an account did not always correspond to the physical address of the property on which taxes are paid. As of July 25, nine days after initiating fieldwork, the firm had only visited 300 addresses (and only completed 47 interviews). We then reorganized the data by geocoding all of the addresses using QGIS together with further municipal data on the location of properties; however, as of December 11, 2014, CIFRA had completed only 2,349 interviews. Given the serious time delay between the receipt of flyers and measurement of household survey data, we therefore amended our pre-analysis plan to focus attention on survey experimental outcomes rather than the estimation of treatment effects from the flyer experiment.

We therefore use experiments that we also embedded in our survey instrument to evaluate several hypotheses about how lotteries shape political attitudes and attitudes towards the equity and fairness of the tax system. In particular, we manipulate the description of both rewards and punishments to emphasize their individual or social rationale. These survey experimental prompts are intended to capture the language and meaning of the flyers to which households were randomly assigned in the field experiment. We briefly summarize the prompts here:

- Individual punishment: "Montevideo's City Hall applies fines and charges to those who do not pay their taxes on time. These sanctions can be very costly for those who do not pay their taxes."
- Social punishment: "Montevideo's City Hall applies fines and charges to those who do not pay their taxes on time. The fines and charges are a sanction for those who do not contribute to the construction of a city that is more just and better for all."
- Individual benefit: "Montevideo's City Hall raffles in connection with the National Lottery the exoneration of municipal taxes for one year. This policy rewards individually those who are up-to-date on their taxes."
- Social benefit: "Montevideo's City Hall raffles in connection with the National Lottery the exoneration of municipal taxes for one year. The City Hall conducts this lottery to recognize good taxpayers for their contribution to a city that is more just and better for all."
- **Discretionary benefit**: "At times Montevideo's City Hall chooses people who are up to date with the payment of municipal taxes, and grants them one year of exoneration of payment of those taxes."

These prompts were randomly assigned to respondents, with two prompts per instrument. (Prompt order was randomized).

For analysis of the survey experimental results, we registered the following outcomes, where respondents are asked for their degree of agreement on a 0-10 scale with the following statements (some of these were asked only to respondents assigned to certain treatment conditions):

- 1. "People only pay their taxes on time when there are substantial fines and charges";
- 2. "In Montevideo, punishments don't apply to the privileged"; and
- 3. "Fines and charges for bad taxpayers are pointless."<sup>39</sup>
- 4. "Policies that reward good taxpayers are a waste of money";
- 5. "In Montevideo, benefits for good taxpayers always go to the same people."<sup>40</sup>
- 6. "In general, the municipal government does a good job"; and
- 7. "In Montevideo, it is worth it to be up to date on ones taxes."<sup>41</sup>

We also asked respondents, "How would you classify the taxes that the municipal government charges, in general: very just, fairly just, a little just, or not just at all?"<sup>42</sup>

## **4** Results

#### 4.1 Winning Inhibits Compliance

Does winning a tax holiday affect future compliance? Figure 4 presents an initial look, pooling across all four types of taxes. The horizontal axis of each plot shows the time elapsed before or after the lottery that we used to select a particular winning number, or to sample eligible non-winning numbers. Thus, for each taxpayer, we center the date (lottery period) at which the taxpayer "won" or "lost" the lottery at t = 0. Time is measured in bimonthly increments, i.e., at each new lottery (recall that for property and vehicle taxes, lotteries occur every two months but prizes are awarded every four months, since payments are triennial). For time periods less than 0, the plots therefore show pre-treatment values; for those greater than zero, they show post-treatment values. Tax compliance behavior of taxpayers with a winning lottery number is shown with red circles; that of taxpayers with a comparable non-winning number (our control group) is shown with blue triangles. The left panel shows "bad" taxpayers—those

<sup>&</sup>lt;sup>39</sup>"Las multas y recargos a malos paradores no sirven para nada."

<sup>&</sup>lt;sup>40</sup>"En Montevideo, los beneficios para buenos pagadores se los llevan los mismos de siempre."

<sup>&</sup>lt;sup>41</sup>"En Montevideo, vale la pena estar al día con los impuestos."

<sup>&</sup>lt;sup>42</sup>"Cómo clasificara los impuestos que cobra la Intendencia de Montevideo en general: muy justos, bastante justos, poco justos o nada justos"

who were ineligible to win at the date of the relevant lottery, based on not being up-to-date on taxes over the previous year. The proportion of good taxpayers on the vertical axis is therefore mechanically forced to 0 at the date of the relevant lottery. The right panel shows "good" taxpayers, the proportion of whom is mechanically 1 at t = 0—since good taxpayers are defined as those who were eligible to win at the date of the relevant lottery. Note that status as a "good" or "bad" taxpayer is not a fixed attribute—this is simply a variable that records eligibility at a particular point in time (though of course it is related to overall propensity to comply). The vertical bars around the proportion of good taxpayers at each time period are 95% confidence intervals.<sup>43</sup>

Figure 4 combines a graphical balance test (pre-treatment values in the left and right panels); a placebo outcome test (post-treatment values in the left panel); and graphical treatment effects (post-treatment values in the right panel). Focusing first on the pre-treatment values in the left and right panels, we have a graphical balance test. Indeed, compliance behavior before the relevant lottery took place should be identical for taxpayers with winning lottery numbers and those in our control group of "losing" lottery numbers—because assignment of a winning number is randomized. It is therefore reassuring that the red and blue plots are indistinguishable prior to time 0, as already suggested by our formal balance tests.

Next, post-treatment values in the left panel of Figure 4 provide a graphical test on a placebo outcome, akin to the formal tests displayed above in Table 3.3. Note that the bad taxpayers in the left panel did not actually "win" the lottery: their tax account IDs simply would have made them eligible for a tax holiday, had they been good taxpayers. (Meanwhile, bad taxpayers who "lost" the lottery in our study group did not really lose anything–they are simply a random sample of similarly ineligible taxpayers in the taxpaying population). We would therefore expect the tax compliance behavior of these two groups of taxpayers to be identical in the post-treatment period, and indeed they are. This also provides evidence for an exclusion restriction: having a winning lottery number does not independently influence tax compliance behavior.

Finally, the post-treatment values in the right panel of Figure 4 depict treatment effects—beginning at the right-hand edge of the grey vertical strip, which measures the point at which all winning taxpayers

<sup>&</sup>lt;sup>43</sup>These are computed for each time period separately: we estimate the mean and standard error for the groups with winning and losing numbers at each time period, and use normal approximations for the confidence intervals (since the *N*s are large).

had tax obligations to pay after the holiday. (This is approximately two years—12 bimonthly payment periods—after the date at which they won the lottery. While exoneration lasts a year, it often does not begin immediately after the lottery, as taxpayers need to present themselves to claim the exoneration, and municipal record-keeping also often occurs with a lag. To be certain we are not comparing winners and losers who each owe taxes, we begin the comparison period after 12 bimonthly payment periods). We estimate the treatment effect of winning the lottery by comparing post-treatment payment behavior of good taxpayers with winning lottery numbers (red circles) to our control group of eligible taxpayers (blue triangles).



Figure 4: Natural Experiment: Balance, Placebo Outcomes, and Treatment Effects

That the triangles appear above the circles and the confidence intervals do not overlap indicates

a *negative* effect of winning the lottery on tax compliance. Indeed, lottery winners are less likely to be good taxpayers after winning, compared to a comparable group of non-winning eligible taxpayers. Table 4.5 presents formal statistical tests. The first five rows test the effect of winning separately, for t = 1 through t = 5 (where t = 1 is the first year when all eligible taxpayers were required to pay taxes again, i.e., the right-hand edge of the grey vertical strip in Figure 5, and t = 2 is then one year later, etc.); the sixth row pools those five years of payments, while the final four rows present estimates that are pooled across time but broken down by type of tax. While there is some year-on-year variation in effect size, the negative effect of winning on compliance appears to last for around three years, and is around 3 percentage points in size.

	Control Mean	Effect	SE Effect	Ν	p-value
Payment Year 1	0.704	-0.035	0.012	5021.000	0.003
Payment Year 2	0.660	-0.023	0.013	4361.000	0.088
Payment Year 3	0.604	-0.030	0.015	3775.000	0.047
Payment Year 4	0.561	-0.011	0.017	3035.000	0.528
Payment Year 5	0.556	-0.028	0.019	2347.000	0.144
Payment Years 1-5	0.642	-0.029	0.011	5021.000	0.010
Property, 1-5	0.790	-0.031	0.015	2024.000	0.034
Vehicle, 1-5	0.260	-0.013	0.020	766.000	0.520
Sewage, 1-5	0.626	-0.051	0.030	653.000	0.091
Head, 1-5	0.639	-0.010	0.020	1578.000	0.599

Table 4.5: Natural Experiment: Tests for Treatment Effects

#### 4.2 Is habit the mechanism?

What explains these results? One possibility is behavioral: winning the lottery could break the habit of payment and therefore lead winners to pay less reliably than winners, at least for some period of time after winning. Indeed, one of the distinctive features of the tax holiday is exactly that it interrupts payment, especially for the property tax. Good students who are given a year off of homework may be less diligent when made to do homework again. Is this the mechanism accounting for our negative results?

One piece of suggestive evidence comes from variation in effects across types of taxes, shown in Figure 5 (now only for good taxpayers, to focus on treatment effects) and rows 7-10 of Table 4.5. For sewage and especially property taxes, the patterns look quite similar to the overall effects for good taxpayers: starting from the right-hand side of the grey vertical strip in Figure 5, the proportion of good taxpayers is lower for those with winning lottery numbers than those with losing lottery numbers. Again, the placebo tests appear as they should for each tax: there is no discernible difference between bad taxpayers with winning and losing lottery numbers. Interestingly, however, the effects are not present for the head and especially the vehicle tax: for those taxes, winning the lottery does not appear to depress tax payment.

The null results for the vehicle tax may provide evidence consistent with an interpretation oriented towards habit. With the vehicle tax, unlike the four other taxes we consider here, winning taxpayers typically continue to pay the taxes in the periods immediately following the winning of the lottery i.e., in the payment periods included in the grey vertical strip. The reason is that with the vehicle tax, payment is exonerated retroactively, so that the *previous* year's payments are forgiven. Some taxpayers take the windfall as a refund, others take it as a credit against future payments. But even those who take credits often owe vehicle fees within the following year—because the vehicle tax is often increased annually, or because of inflation of the nominal value of the payment (as opposed to the nominal value of the credit at the time the lottery was won).

The evidence from cross-tax variation is only suggestive (and we are doing some additional work, for instance, to identify taxpayers in our study group who pay both vehicle and property tax, in order to eliminate one possible source of heterogeneity and compare treatment effects by tax type within this group). However, in conjunction with the municipality, we are designing ongoing experimental interventions that may allow us to assess this possibility. For example, among current tax holiday winners whose tax obligations will soon resume, we plan to assess whether a randomly assigned reminder to start paying taxes again—or an encouragement to enroll in an automatic debit plan—can boost compliance. We would welcome suggestions on further approaches to evaluating the habit idea.



Another possible mechanism is misperception: perhaps the lottery provides an incentive to pay taxes on time, but winners believe they cannot win again. Some evidence from our household survey

is consistent with this. For example, after describing the lottery, we asked respondents what they think the chances of winning the lottery are for any taxpayer; we then asked whether the chance of winning again would be more, the same, or less for someone who had already won the lottery. Though the probabilities are independent, 2% of respondents said the chance of winning again was larger than the original probability and 55% said it is the same, but 42% answered that the chances of winning again are lower than the initial chance of winning. Yet, militating against this interpretation is the evidence we present next, from our field experiment, that calls into question whether the lottery does indeed create an incentive to pay taxes on time.<sup>44</sup>

#### 4.3 Informational Interventions: Positive vs. Negative Incentives

If winning the lottery does not increase and may even decrease tax compliance among good taxpayers, what does knowledge of the lottery do to bad taxpayers? Recall that only 8% of survey respondents identified the lottery as a municipal policy that rewards good taxpayers. Thus, informing taxpayers about the lottery policy may well be a good way to influence tax-payment behavior.<sup>45</sup>

Figure 6 provides a first assessment. This figure compares proportions in the six treatment conditions to the control group that received no flyers, on two outcome variables: Intended Compliance (whether the account holder accessed the Web account, for example, to print a duplicate bill); and whether the account holder Paid Bill On Time in July 2014, the first payment period following our intervention. Note that because the comparison between the treatment conditions is our main strategy for assessing the effects of positive vs. negative incentives, we did not pre-register the analysis in Figure 6. It is nonetheless instructive because it clearly suggests that our flyers had an impact. For both good and bad taxpayers, nearly all the types of flyers elevated Intended Compliance, relative to the control group.<sup>46</sup> Interestingly, however, note that for good tax payers the point estimate is highest for the simple reminder condition. We believe in retrospect that the volume of text on the other flyers could have

<sup>&</sup>lt;sup>44</sup>Also potentially militating against this interpretation is that 84% of the respondents underestimated the chances of winning the lottery: 64% answered 1 out of 100,000 won and 20% claimed it was 1 out of 10,000, in response to a closed-ended question. Only 6.5% guessed the right chance of 1/5,000, whereas 10% overestimated the probability.

<sup>&</sup>lt;sup>45</sup>In our first pre-analysis plan, we indicated that we would also conduct instrumental variables analysis using the rate of knowledge in the control group in the denominator of a Wald estimator of the effect of treatment on Compliers. This analysis is pending.

<sup>&</sup>lt;sup>46</sup>The apparent exceptions, at least according to the nominal significance levels, are the individual reward condition among the bad taxpayers and the social reward condition among the good taxpayers.

distracted from the simple, powerful message reminding taxpayers that their account is due and that duplicate copies could be obtained on the web, in the Reminder condition. We return to discuss that interpretation later.



Figure 6: Treatment effects, good and bad taxpayers, relative to control group

The right-hand panel of Figure 6, meanwhile, suggests that our flyers also boosted actual compliance but here only among bad taxpayers, and the estimates are only nominally significant for the punishment conditions. The effect estimate of 4 percentage points is similar to that estimated by Castro and Scartascini (2015), in their study of the effects of punishment threats on tax compliance in Junín, Argentina. Indeed, when we pool the reward and punishment conditions together (combining the "individual" and "social" treatments), we see that there as well the punishment condition is significant at nominal levels (Figure 7). These estimates are suggestive that our flyers collectively had an effect on both intended and actual compliance.





However, what is most striking in the figures is that the simple reminder appears to have a large effect, especially on intended compliance by good taxpayers; and indeed, there does not appear to be any difference between the reward or punishment conditions and the simple reminder. Figure 8 confirms this impression. This pre-registered comparison shows that there is no positive impact of information on either punishments or rewards on intended or actual compliance, relative to the reminder condition. And indeed, the nominal significance test (that is, without yet adjusting for multiple comparisons) suggests that the pooled reward condition actually drives *down* compliance, relative to the simple reminder. Table 8 shows the means, standard errors, and *p*-values for these comparisons.





Table 4.6: Rewards and Punishments: Pooling Individual and Social Conditions

Treatment	Outcome	Taxpayer Type	Reminder	Effect	SE	Ν	p-value
			Mean				
Pooled Reward	Paid Bill	good	0.939	0.000	0.007	4567	0.985
Pooled Reward	Paid Bill	bad	0.470	-0.006	0.013	6165	0.668
Pooled Reward	Accessed Web	good	0.223	-0.066	0.013	4569	0.000
Pooled Reward	Accessed Web	bad	0.150	-0.020	0.009	6230	0.034
Pooled Punishment	Paid Bill	good	0.939	-0.006	0.008	4501	0.464
Pooled Punishment	Paid Bill	bad	0.470	0.015	0.013	6231	0.277
Pooled Punishment	Accessed Web	good	0.223	-0.026	0.013	4504	0.044
Pooled Punishment	Accessed Web	bad	0.150	-0.005	0.010	6300	0.619

The table reports treatment effects for good and bad taxpayers, pooling the individual benefit and social benefit conditions ("Pooled Reward") and the individual punishment and social punishment conditions ("Pooled Punishment"), all relative to the Reminder condition.

A final topic for the field experiment relates to heterogeneous effects. In particular, we hypothesized that effects may be largest for marginal taxpayers—those "good taxpayers at risk," who have owed payments in the past, or "salvageable bad taxpayers," who were ineligible for tax rebates at the time

of our intervention but have never owed more than a year of taxes. Such taxpayers may be especially responsive to our experimental prompts. The analysis in Table 6.9 shows estimated effects, standard errors, and *p*-values for these taxpayers. The evidence suggests that treatment effects are no stronger for this group.

Treatment	Outcome	Taxpayer Type	Reminder	Effect	SE	Ν	p-value
			Mean				
Pooled Reward	Paid Bill	at risk	0.891	0.007	0.019	1165	0.712
Pooled Reward	Paid Bill	salvageable	0.592	-0.013	0.015	4701	0.381
Pooled Reward	Accessed Web	at risk	0.219	-0.053	0.025	1165	0.033
Pooled Reward	Accessed Web	salvageable	0.187	-0.024	0.012	4736	0.04
Pooled Punishment	Paid Bill	at risk	0.891	0.021	0.019	1125	0.277
Pooled Punishment	Paid Bill	salvageable	0.592	0.002	0.015	4806	0.89
Pooled Punishment	Accessed Web	at risk	0.219	-0.033	0.025	1125	0.193
Pooled Punishment	Accessed Web	salvageable	0.187	-0.013	0.012	4849	0.272

Table 4.7: Heterogeneous effects for "marginal" taxpayers

One final wrinkle about our study group is important to note: the list of addresses provided by the municipality includes non-physical persons (e.g., corporations) as well as physical persons living in households. The municipality does not have a ready way to distinguish these persons, which is why it must screen lottery-winning taxpayer accounts before awarding a year free of tax payments. Using the random sample of study group households included in our household survey, we can estimate the rate of physical persons as a proportion of the taxpayer accounts in our study group. (However, we discuss later some difficulties with our survey logistics). This also allows us to conduct a type of instrumental-variables analysis, in which physical persons are treated as "Compliers" who could have received the treatment. We return to such issues of analysis in section 4.

### 4.4 Survey Experiment

Finally, we turn to the survey experiment. Recall that our motive here is to assess whether exposure to the prompts and messages on our flyers shapes attitudes towards the equity and fairness of the tax system, as well as broader political attitudes. We designed these as intermediate outcomes that may help us assess mechanisms through which lotteries might influence compliance, yet we found only weak effects of our flyers on tax payment. Still, it is possible that the treatments provided by the field

(or even natural) experiments are too weak and that other informational campaigns regarding these policies could in principle influence attitudes.



However, our evidence from the survey experiments is also largely consistent with null effects with one important exception. Figure 28 shows the means and confidence intervals for our experiments comparing the lottery (social and individual benefit) to the discretionary benefit condition. (Figure 28 in the Appendix shows effects for social vs. individual rewards; and pooled punishment vs. pooled rewards). For the most part, comparisons across these conditions show few significant difference. Yet, as the bottom-left two panels of the figures show, there is indeed evidence that lotteries are viewed as less discretionary: exposure to the lottery treatment rather than the discretion treatment significantly boosts the propensity to say that it is worth it to be up to date on taxes, and significantly diminishes the tendency to say that rewards "go to the same people as always." This is interesting, in light of the fact that comparing the discretionary to lottery treatments is intended precisely to measure the effects on such perceptions of transparency and equity.<sup>47</sup>

## 5 Conclusion

Municipal governments throughout Latin America, including the capital city of Uruguay, have developed positive incentive schemes such as randomized lotteries designed to boost tax compliance. The theory that they should have such effects is fairly plausible. Enforcing tax compliance through sanctions for non-payment is notoriously difficult in the developing world, and even when monitoring problems are minimal—as in the case of the taxes we study in this paper—legal, institutional, and political forces can militate against strong enforcement. Rewards such as tax holidays for good taxpayers seem to offer a promising alternative. Even if the *expected* material return to taxpayers may be low or negative, the behavioral impact of a promised reward can be powerful (as lotteries and games of chance in other settings suggest). Moreover, the transparency of the lottery process and the recognition of good taxpaying behavior it promises may have strong effects on taxpaying behavior for non-pecuniary reasons.

Our evidence presented in this paper suggests otherwise. Using a strong and high-powered design that combines natural, field, and survey experiments, and leveraging our unusual access to individual tax payment records, we find little evidence that information about rewards—or rewards themselves— boost compliance. Indeed, a simple reminder that taxes are due has a stronger effect on intended compliance measured as access to a duplicate online copy of taxpayers' bill than either information about punishments or rewards. Punishments sometimes appear more effective than rewards yet they, too, often add little to a simple reminder. It may be that a reminder is a kind of threatened punishment, especially to a bad taxpayer. Yet the addition of the reminder condition is valuable, given past findings in similar studies without such a placebo group that threats of punishment can boost compliance, relative to a control group that receives no flyers.<sup>48</sup>

These results should not condemn efforts to build tax compliance through positive incentives. More

<sup>&</sup>lt;sup>47</sup>This finding is related to claims on how individual perceptions on government spending can affect tax compliance. See for example Cowell and Gordon (1988).

<sup>&</sup>lt;sup>48</sup>See e.g. Castro and Scartascini (2015).

evidence is needed: some incentive programs in Latin America have features that Montevideo's lacks such as public recognition of good taxpaying behavior, or other ways of cultivating social pressures to pay taxes—and those could prove critical. Yet, strong evidence on the effects of such programs is often lacking. Our negative results do call into question their general power, and they underscore the difficulties in encouraging greater tax payment in a setting with persistent non-compliance.

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Figure 9: Text of informational intervention (Spanish): Reminder





## Estimado/a vecino/a:

Queremos recordarle que en el mes de **julio vence la segunda cuota de la Contribución Inmobiliaria.** Si todavía no recibió su factura, puede obtener un duplicado en nuestro sitio web (www.montevideo.gub.uy).

Por consultas: FONO TRIBUTOS 1950 3000



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Figure 10: Text of informational intervention (Spanish): Reminder + lottery + individual benefit





## Estimado/a vecino/a:

Queremos recordarle que en el mes de **julio vence la segunda cuota de la Contribución Inmobiliaria.** Si todavía no recibió su factura, puede obtener un duplicado en nuestro sitio web (www.montevideo.gub.uy).

La Intendencia de Montevideo quiere premiar a los buenos pagadores. Si usted paga en fecha participará automáticamente de un sorteo por la exoneración de un año de Contribución Inmobiliaria.

Los sorteos se realizan todos los meses pares del año junto con la Lotería Nacional. Los beneficiados serán debidamente informados y se publicarán los resultados en el sitio web de la Intendencia.

## ¡Usted puede ser el próximo!

Por consultas:

FONO TRIBUTOS **1950 3000** 





Figure 11: Text of informational intervention (Spanish): Reminder + lottery + individual benefit + probability of winning







## Estimado/a vecino/a:

Queremos recordarle que en el mes de **julio vence la segunda cuota de la Contribución Inmobiliaria.** Si todavía no recibió su factura, puede obtener un duplicado en nuestro sitio web (www.montevideo.gub.uy).

La Intendencia de Montevideo quiere premiar a los buenos pagadores. **Si usted paga en fecha participará automáticamente de un sorteo por la exoneración de un año de Contribución Inmobiliaria.** 

#### En cada sorteo, 1 de cada 5.000 hogares recibe este beneficio.

Los sorteos se realizan todos los meses pares del año junto con la Lotería Nacional. Los beneficiados serán debidamente informados y se publicarán los resultados en el sitio web de la Intendencia.

#### ¡Usted puede ser el próximo!

Por consultas:

# FONO TRIBUTOS **1950 3000**



Figure 12: Text of informational intervention (Spanish): Reminder + individual punishment

Quienes no paguen en fecha podrían estar sujetos a multas y recargos. La Intendencia de Montevideo podría tomar acciones administrativas y legales para hacer cumplir la normativa en los casos que correspondan.

# ¡Pague en fecha, evite multas y recargos!

Figure 13: Text of informational intervention (Spanish): Reminder + lottery + social benefit





## Estimado/a vecino/a:

Queremos recordarle que en el mes de **julio vence la segunda cuota de la Contribución Inmobiliaria.** Si todavía no recibió su factura, puede obtener un duplicado en nuestro sitio web (www.montevideo.gub.uy).

La Intendencia de Montevideo quiere premiar a los buenos pagadores. **Si usted paga en fecha participará automáticamente de un sorteo por la exoneración de un año de Contribución Inmobiliaria.** 

Los sorteos se realizan todos los meses pares del año junto con la Lotería Nacional. Los beneficiados serán debidamente informados y se publicarán los resultados en el sitio web de la Intendencia.

La Intendencia de Montevideo realiza este sorteo para reconocer a los buenos pagadores por su contribución a la construcción de una ciudad más justa y mejor para todos/as.

46

Por consultas:

FONO TRIBUTOS **1950 3000** 

Figure 14: Text of informational intervention (Spanish): Reminder + social punishment



## Estimado/a vecino/a:

Queremos recordarle que en el mes de **julio vence la segunda cuota de la Contribución Inmobiliaria.** Si todavía no recibió su factura, puede obtener un duplicado en nuestro sitio web (www.montevideo.gub.uy).

Quienes no paguen en fecha podrían estar sujetos a multas y recargos. La Intendencia de Montevideo podría tomar acciones administrativas y legales para hacer cumplir la normativa en los casos que corresponda.

Las multas y recargos son una sanción para quienes no pagan sus impuestos y no contribuyen a la construcción de una ciudad más justa y mejor para todos/as.

Por consultas:

FONO TRIBUTOS **1950 3000** 

Figure 15: Informational intervention: Reverse side of flyers with municipal logo

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La no recepción en tiempo y forma por parte del contribuyente de las facturas solicitadas, no eximen de la obligación de pago en los plazos legales.

## 6 Online Appendix: Dunning et al. (2015)

#### 6.1 Power calculations

In this sub-section and the associated Figures 16-19, we present formal justification for the sample sizes in our natural and field experiments (Tables 3.1 and 3.4). For several of our power analyses, we take as a benchmark the informational experiment of Castro and Scartascini (2015), who estimate effects of informational treatments on tax compliance of over 4 percentage points using *negative* incentives for compliance (reminding taxpayers of fines for non-compliance, as in our Individual Sanction treatment). However, we calculate the probability of rejecting the null hypothesis of no effect, given various true effect sizes. This effect could be, e.g., the difference in subsequent tax compliance rates for lottery winners and eligible non-winners, or differences in various graded outcomes measured through administrative or survey data.

**Binary outcomes (e.g. tax compliance)**: There are N units with  $n_T$  units assigned to treatment and  $n_C = N - n_T$  to control; we assume equal numbers assigned to treatment and control ( $n_C = n_T$ ), as in our natural experiment per Table 3.1 and as in most of of the pairwise comparisons in Table 3.4 for the field experiment. We suppose average tax compliance is around 70%, thus the variance of this binary outcome is  $0.7 \times (1 - 0.7)$ , pooling across treatment and control groups. Thus, the standard error for the difference of tax compliance rates across treatment and control groups is<sup>49</sup>

$$\sqrt{\frac{0.7 \times 0.3}{n_T} + \frac{0.7 \times 0.3}{n_C}},\tag{4}$$

or, using  $n_T = n_C = \frac{N}{2}$ ,

$$SE = \frac{2\sqrt{0.7 \times 0.3}}{\sqrt{N}}.$$
(5)

For each effect size, we calculate power under a two-tailed test as

$$1 - \Phi(2 - \frac{\text{effect}}{\text{SE}}), \tag{6}$$

where  $\Phi$  is the normal cumulative distribution function, SE is given by equation (5), and **effect** is the true effect size.<sup>50</sup> Equation (6) gives the approximate area above the normal curve centered over **effect** that is more than two standard errors away from 0, the effect size under the null hypothesis.<sup>51</sup> For a one-tailed test, we use 1.65 in place of 2 in equation 6; a one-tailed test is more appropriate for many of our unidirectional hypotheses discussed in Section 4 (e.g., knowledge of the lottery increases tax compliance, among lottery losers or bad taxpayers, but does not decrease it).

Figures 16 and 17 show power for two-tailed and one-tailed tests, respectively, assuming true effects of 4, 6, 8, and 10 percentage points, e.g., **effect**  $\in$  {0.04, 0.06, 0.08, 0.10}. In each figure, the vertical line shows the study group size, pooled across treatment and control groups, that is needed for 80% power given each effect size. For N = 2,000, we have slightly more than 80% power given a true effect size of 6 percentage points, using a two-tailed test; for a one-tailed test, we have 80% power against a

<sup>&</sup>lt;sup>49</sup>We use the "conservative" formula for the standard error in randomized experiments, which is the same as for the difference of proportions of two independent samples; for formal justification, see online Appendix notes 31, 33 of David Freedman, Roger Pisani, and Roger Purves, 2007, *Statistics*, W.W. Norton Co., 4th edition.

 $<sup>^{50}</sup>$ We switch the signs in (6) to give the area *greater* than two standard errors above zero.

<sup>&</sup>lt;sup>51</sup>To be conservative, here we use 2 in place of 1.96, though we can rely on the central limit theorems and use normal approximations for most hypothesis tests; with smaller n, one might want to use the *t*-distribution or permutation tests.

true effect size of 5 percentage points (N = 2,000). With a one-tailed test, we also have 80% power for an effect size of 6 percentage points when N = 1,500. These calculations suggest reasonable power to measure moderate effects with binary outcomes, using our survey data. However, to measure the binary outcome of tax compliance, we will use cheaper administrative data and thus a larger N, so our power will be substantially greater.

**Graded outcomes:** Power is greater with graded measures rather than binary outcomes. Our household survey will measure attitudes towards the tax system, often using scales instead of binary outcomes (e.g., degree of agreement with statements about the fairness of the tax system); and we also construct graded measures of indebtedness, as discussed in Section **??**. In Figures 18 and 19, we measure effect sizes in relation to the unknown standard deviation of this outcome variable. Thus, for pooled N = 2,000, we will have power of just over 80% against a true effect size of 0.13 standard deviations (two-tailed test).

Estimating control-group parameters: One important role of the household survey is to allow us to estimate the proportion of taxpayers in Montevideo who are uninformed about the existence of the lottery. It is critical that we estimate this proportion precisely, as this estimated proportion is the denominator in some of our instrumental-variables analyses. We have several sources of data to estimate the proportion of uninformed taxpayers: (1) survey data on the Reminder group in our field experiment (N = 2,000, pooling across eligibles and ineligibles); (2) survey data on the "individual sanction" group in our field experiment, who we also do not inform of the existence of the lottery (N = 2,000, pooling across eligibles and ineligibles); and (3) survey data on the control group in the natural experiment (N = 1,000).<sup>52</sup>. If 50% of good taxpayers who have never won the lottery are unaware of its existence, the standard error for our estimate of this population percentage is 0.79% pooling across good and bad taxpayers and using data from (1) and (2) (N = 4,000). (We will need to weight the estimates when pooling, to account for our oversampling of ineligible taxpayers). With N = 5,000, the standard error is 0.71%.

**Justification for sample size**. These power calculations justify our sample size for the household surveys, as depicted in Tables 3.4 and 3.1. Our power is about 80% against effect sizes for tax compliance comparable to those estimated in previous research, in the case of negative incentives. Our sample size gives us similar power against movements of around 0.15 standard deviations in attitudinal dependent variables measured as scales. Finally, our sample of households who have not won the lottery allows us to estimate the proportion of taxpayers who are uninformed about the lottery with fairly good precision; these estimates are important for assessing overall program impact as well as the likely effects of more effectively promoting knowledge of the tax rebate lottery.

With respect to the administrative data collection, our sample size balances our desire for more data against the cost in time and effort to the municipality.<sup>53</sup> One issue is that some of the tax payer records are for juridical not physical persons (i.e., they are companies). In our surveys, we have the ability to filter juridical persons, ultimately by visiting households; this will allow us to assess the overall proportion of physical persons in the population of taxpayers. However, this will certainly diminish our true power, relative to these calculations. We therefore want to err as much as possible on the side of a large sample size for the administrative data.

<sup>&</sup>lt;sup>52</sup>The latter group is a random sample of the population of eligible taxpayers, but eligibility is as of the date of different lotteries, so this is a stratified random sample where we do not know the probability of being in each strata; we may therefore opt to use only (1) and (2) to estimate the proportion of uninformed taxpayers.

<sup>&</sup>lt;sup>53</sup>Supplying data is not costless for the municipality, as will involve manual extraction of records from municipal databases using the four-digit IDs we generate, and the municipality will only grant access to a sample of the data.







proportions. Vertical line shows the size required for 80% power.







Plots show statistical power as a function of study size for different effect sizes (binary outcome, e.g tax compliance). Effects are differences of proportions. Vertical line shows the size required for 80% power.







Circles show statistical power as a function of study size for different effect sizes (graded outcome, e.g attitude scales). Effect sizes are expressed in standard deviations, e.g. 0.1 of one SD. Vertical line shows the size required for 80% power.







Circles show statistical power as a function of study size for different effect sizes (graded outcome, e.g attitude scales). Effect sizes are expressed in standard deviations, e.g. 0.1 of one SD. Vertical line shows the size required for 80% power.

## 6.2 Field Experiment: Balance



Figure 20: Balance: Accumulated Missed Payments

### 6.3 Additional results



Figure 21: Components of the Web Access Indicator

good taxpayer bad taxpayer



#### Figure 22: Outcome levels, good taxpayer



#### Figure 23: Outcome levels, bad taxpayer



Figure 24: Difference in differences analysis



Figure 25: Treatment effects relative to reminder group (Difference-in-Differences analysis)

Table 6.8: Rewards and Punishments: Pooling Individual and Social Conditions

Treatment	Outcome	Taxpayer Type	Control	Effect	SE	Ν	p-value
			Mean				
Pooled Reward	Paid Bill	good	0.94	-0.00	0.01	10274	0.92
Pooled Reward	Paid Bill	bad	0.45	0.02	0.01	7477	0.11
Pooled Reward	Accessed Web	good	0.14	0.02	0.01	10280	0.02
Pooled Reward	Accessed Web	bad	0.11	0.02	0.01	7562	0.01
Pooled Punishment	Paid Bill	good	0.94	-0.01	0.01	10208	0.24
Pooled Punishment	Paid Bill	bad	0.45	0.04	0.01	7543	0.00
Pooled Punishment	Accessed Web	good	0.14	0.06	0.01	10215	0.00
Pooled Punishment	Accessed Web	bad	0.11	0.04	0.01	7632	0.00
Reminder	Paid Bill	good	0.94	-0.00	0.01	8769	0.92
Reminder	Paid Bill	bad	0.45	0.02	0.01	5418	0.08
Reminder	Accessed Web	good	0.14	0.08	0.01	8775	0.00
Reminder	Accessed Web	bad	0.11	0.04	0.01	5492	0.00

The table reports treatment effects for good and bad taxpayers, pooling the individual benefit and social benefit conditions ("Pooled Reward") and the individual punishment and social punishment conditions ("Pooled Punishment").

Treatment	Outcome	Taxpayer Type	Control	Effect	SE	N	p-value
			Mean				
Pooled Reward	Paid Bill	at risk	0.90	-0.005	0.013	2992	0.663
Pooled Reward	Paid Bill	salvageable	0.56	0.019	0.013	5703	0.143
Pooled Reward	Accessed Web	at risk	0.13	0.036	0.015	2994	0.019
Pooled Reward	Accessed Web	salvageable	0.13	0.029	0.009	5753	0.002
Pooled Punishment	Paid Bill	at risk	0.90	0.008	0.012	2952	0.511
Pooled Punishment	Paid Bill	salvageable	0.56	0.035	0.013	5808	0.008
Pooled Punishment	Accessed Web	at risk	0.13	0.056	0.016	2954	0.001
Pooled Punishment	Accessed Web	salvageable	0.13	0.040	0.009	5866	0.000
Reminder	Paid Bill	at risk	0.56	0.03	0.02	4118	0.04
Reminder	Paid Bill	salvageable	0.90	-0.01	0.02	2613	0.46
Reminder	Accessed Web	at risk	0.13	0.05	0.01	4169	0.00
Reminder	Accessed Web	salvageable	0.13	0.09	0.02	2615	0.00

Table 6.9: Heterogeneous effects for "marginal" taxpayers

## 6.4 Survey Experiment

	Mean	Mean	Difference	SE Diff	t-stat	Ν	df	p-value	FDR	Bonferroni
	Individual	Social							reject	reject
	Benefits	Benefits								
Worth it to be up to date	7.54	7.84	-0.31	0.14	-2.16	1685	1246.56	0.03	do not reject	do not reject
Rewards are waste of money	3.09	3.12	-0.03	0.19	-0.18	1664	1152.15	0.86	do not reject	do not reject

	Mean	Mean	Difference	SE Diff	t-stat	Ν	df	p-value	FDR
	Discretionary	Lottery							reject
	Rewards	Rewards							
Rewards go to the same people as always	6.09	5.03	1.05	0.22	4.81	1542	691.02	0.00	reject null
Worth it to be up to date	7.17	7.64	-0.48	0.15	-3.16	2266	902.40	0.00	reject null
Mun. taxes are just	2.75	2.71	0.04	0.04	1.01	2291	1021.98	0.16	do not reject
Mun.gov. does a good job	4.49	4.66	-0.17	0.14	-1.19	2313	1037.66	0.23	do not reject
Rewards are waste of money	3.08	3.10	-0.02	0.18	-0.12	2234	979.39	0.90	do not reject

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	Mean	Mean	Difference	SE Diff	t-stat	N	df	p-value	FDR	Bonferroni
	Positive	Negative							reject	reject
	Incentives	Incentives								
Worth it to be up to date	7.64	7.85	-0.21	0.09	-2.33	3958	3606.11	0.02	do not reject	do not reject
Mun. gov. does a good job	4.66	4.76	-0.10	0.10	-1.04	4008	3727.03	0.15	do not reject	do not reject
Mun. taxes are just	2.71	2.70	0.01	0.02	0.35	3993	3675.76	0.36	do not reject	do not reject



Figure 26: Natural experiment: property plots of winning account numbers (treatment group)



Figure 27: Natural experiment: property plots of winning account numbers (control group)



#### Figure 28: Survey experiment: all results