

# Is Paying Taxes Habit Forming?

## Experimental Evidence from Uruguay

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## Abstract

Political participation involves routinized, repeated behaviors—and repetition may itself breed habits of citizenship that exert a causal influence on behavior. This implies the possibility of virtuous or vicious cycles in civic participation. Yet, it is usually extremely difficult to separate habit from confounding explanations for repeated behaviors. We study a policy in Montevideo, Uruguay that randomly assigns tax holidays, or year-long interruptions of payments, to punctual taxpayers; the program is designed both to reward and induce tax compliance, a critical aspect of citizen-state interaction and a key facet of state capacity. We find that far from fostering compliance, the tax holiday inhibits it: winning the lottery results in a 3 percentage-point reduction in compliance after the end of the holiday, an effect that lasts for up to three years. We use field and survey experiments to disentangle informational mechanisms from the effects of habit disruption. Our findings have both social-scientific and policy implications.

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

# 1 Introduction

Political participation often involves routinized, repeated interactions between citizens and the state. In realms as distinct as voting, dealings with police, or payment of taxes, repetition of a behavior may breed habits of citizenship—and these habits may themselves exert a causal influence on behavior. Recent research on voting, for example, has demonstrated the powerful force of habit. Gerber, Green and Shachar (2003) show that voters randomly assigned to receive a get-out-the-vote message in one election were significantly more likely to vote in the next election as well. Meredith (2009) finds that eligibility to vote in a past presidential election—as determined by whether a voter was just over or just under 18 years old—increases the probability of participation in the subsequent election. If voting is partly habitual, interruptions of habit can also have substantial consequences. For example, disruptions caused by residential mobility and re-registration requirements may account for turnout disparities between young and old voters in the United States (Aldrich, Montgomery and Wood, 2011; Ansolabehere, Hersh and Shepsle, 2012). The power of habit also implies the possibility of virtuous or vicious cycles in civic participation—and raises the question of how participatory habits are formed and how they may be disrupted.

Consider the case of tax compliance—a critical aspect of citizen-state interaction and a key facet of state capacity. Scholars often attribute low rates of compliance, especially in developing countries, to weak monitoring capacity and the inability of states to penetrate society and compel tax payment.<sup>1</sup> Yet habit could be a contributor to persistently high or low compliance, in developed or developing countries alike. In data we gathered for this project, we found considerable non-compliance with municipal property tax bills in Montevideo, the capital city of Uruguay—a Latin American country thought to possess a relatively capable and efficacious state. For example, the average property tax account in Montevideo owed four past due tax payments in 2015. Only around 70 percent of tax bills are paid on time, and the municipality classifies just around 50 percent of property taxpayers as “good taxpayers” at any point in time, based on having paid taxes on time over the previous year.<sup>2</sup> For other types of municipal taxes, and in many other countries, compliance rates are even lower. Weak monitoring capacity alone cannot explain these patterns: with municipal property taxes, the state knows

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<sup>1</sup>See e.g. Besley and Persson (2009) or Mann (1984).

<sup>2</sup>The data are from Figure 1, discussed later.

with certainty the amount of tax owed on the basis of appraised property values, yet still fails to induce prompt compliance among substantial numbers of taxpayers.

Habit, by contrast, may play a role in sustaining poor compliance. While evidence we present later suggests that some taxpayers move in and out of the “good taxpayer” category over time, there is also substantial structural stability, with many “good taxpayers” remaining compliant year after year while similarly situated people are repeatedly non-compliant. The difficult question, of course, is whether habit has any causal effect, or whether unobserved attributes account for all of the difference between compliant and non-compliant taxpayers. Empirical research on habit is not as advanced for taxes as it is for voting, perhaps because it is exceedingly difficult to randomly assign past compliance. Yet, understanding habit’s impact on tax payment is important for both social-scientific and policy purposes.

We show in this article that states—and social scientists—ignore the importance of habit at their peril. Faced with the reality of low compliance, and the apparent difficulty of using negative incentives such as threats of punishment to elicit payments, many municipalities in developing countries have experimented with rewards programs that offer positive incentives for prompt tax payment. For example, upwards of 25 percent of municipalities in Brazil, Colombia, Ecuador, and Uruguay, and many localities in Argentina, Peru, and Mexico, now offer rewards programs such as prize lotteries to good taxpayers.<sup>3</sup> Montevideo was one of the earliest innovators of such policy in Latin America: since 2004, and across four types of municipal taxes (property, vehicle, sewage, and head), the municipality has raffled a year free of tax payments to eligible good taxpayers who have been punctual compliers over the previous year. These tax holidays both reward and may incentivize future compliance by good and bad taxpayers alike; they may also boost perceptions that the tax system is equitable and transparent.

However, such holidays also interrupt the habit of tax payment. Indeed, the lotteries provide a rare form of randomization that allows study of the impact of disrupting tax compliance. Using access to a detailed panel of administrative records on individual tax payment as well as household survey data, we compare lottery winners to a randomly selected control group of eligible non-winners (study group N=6,363). This natural experiment allows us to assess how winning a tax holiday influences subsequent tax compliance, using unobtrusive outcomes that are not subject to social desirability bias from self-reports and that are measured over a long time period (2000-2014). Placebo outcome tests

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<sup>3</sup>See Section 2.

with data on ineligible “winners” as well as balance tests on pre-treatment covariates validate key assumptions of our design.

We find that far from promoting tax compliance, Montevideo’s tax holiday inhibits it: among eligible good taxpayers, winning the lottery results in a 3-point reduction in the proportion of good taxpayers, an effect that lasts for up to three years. The program is therefore a revenue loser, not only because of the tax receipts foregone during the tax holiday, but because winners are worse taxpayers after the end of the holiday than they otherwise would have been. Estimates of complier average causal effects suggest a much larger impact on those who actually receive the exoneration. Our interpretation that this is due to interruption of habit is bolstered by the fact that we do not find effects for some municipal taxes, such as vehicle, for which winners typically continue to pay some small amount of tax and for which the payment habit is thus not in fact interrupted. We also find no effect on taxpayers signed up for automatic debits, whose payments mechanically restart after the conclusion of the tax holidays; only for the majority of manual taxpayers does the tax holiday have a negative effect.

The main alternative explanations for our findings are informational. Our survey data suggest that the municipal government has poorly advertised the tax holiday: only about 8% of Montevidean taxpayers can identify the tax lottery as a municipal policy that rewards good taxpayers. Along with disrupting the payment habit, winning the lottery thus also informs many winners about the existence of the rewards program. To explain the negative impact of winning on subsequent compliance, taxpayers would have to interpret that information as a negative signal about municipal capacity or the payment behavior of other taxpayers. Alternately, winners might falsely believe that their probability of winning the lottery a second time is lowered, which might lower future compliance if the lottery indeed has a positive incentive effect. Winning the lottery might also lead to income or substitution effects, in which taxpayers spend the additional revenue gained from the tax holiday on consumption goods in a way that makes it more difficult to resume good taxpaying once the holiday expires. Finally, the tax holiday could have a behavioral effect by breaking a taboo against non-compliance among good taxpayers.

To assess these alternative explanations, we also conducted a field experiment with a series of informational interventions. In collaboration with the municipal government, we mailed flyers stamped with the municipal logo—which appear very much like tax bills themselves—reminding taxpayers of an impending due date for taxes and providing additional experimentally varied messages. Along with

a pure control group that received no flyers, we assigned households to receive a mere reminder (the placebo control group); a reminder plus information about the existence of the lottery (which in turn primed either individual or social benefits of the lottery); and a reminder plus information about fines and sanctions for non-payment (again priming the individual or social rationale for punishment). We then tracked the impact of our treatments using administrative data on ex-post tax payment and other behavior, including whether taxpayers logged in to their online accounts, which we take as a measure of intended compliance. This large field experiment (N=28,646 households) accomplishes several objectives. Most importantly, by providing information about the lottery without actually awarding the tax holiday, our interventions allow us to separate informational effects from the impact of habit disruption. The experiment also allows us to compare the effects of the putative positive incentives provided by the lottery to the effects of more typical negative incentives such as punishments for non-compliance; and because we included “bad” as well as “good” taxpayers in the experiment, it also allows assessment of the broader impact of the program, which might cause ineligible “bad” taxpayers to bring their tax accounts up to date to gain eligibility for the tax holiday. Finally, we also use survey experiments embedded in our household survey to assess the impact 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. We registered a pre-analysis plan covering our natural and field experiments and subsequently posted two amendments before receiving outcome data.<sup>4</sup>

We find little evidence that informational explanations or substitution effects can account for our results. Our informational interventions as a whole did succeed in boosting access to online tax bills, our measure of intended compliance; among bad taxpayers, the message about punishments boosted actual tax compliance by around 4 percentage points, relative to a pure 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 to a placebo control group that taxes are due. Thus, while many taxpayers do perceive that winning the lottery produces a lower probability of winning again, our evidence is not consistent with a powerful positive incentive effect of the lottery, which is necessary for misperception to explain the negative effect of

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<sup>4</sup>See <https://www.socialscienceregistry.org/trials/452> and <http://egap.org/design-registration/registered-designs/> (design 84: 20140723). Complete results produced using the code in our registered mock analysis will be available in an online Appendix.

winning on compliance. We also find some evidence from our survey experiment that information about the lottery positively impacts attitudes towards the fairness of the tax system. It is therefore unlikely that information about the lottery acts as a negative signal about the fiscal system. We also emphasize that the effects of habit interruptions, while persistent, may eventually decay (see Section 2) and indeed this is what we find in the data; alternative explanations such as the breaking of a taboo against non-compliance might suggest more permanent effects. We discuss the evidence against other alternative explanations further in Section 4, where we conclude that the disruption of payment itself is consequential and that the effect on habit is the most likely mechanism.

Our study makes several contributions to the study of the habitual bases of political participation, as well as the causes of tax compliance. In settings where taxpayers face a low probability of punishment for non-compliance, the key question may not be why people do not pay their taxes—but why any taxpayers do comply. Our study provides some insight into this important puzzle, similar in its essence to the paradox of voting.<sup>5</sup> While expressive or material benefits of paying taxes could play some role in sustaining compliance, the simple repetition of compliant behavior can itself breed future payment. This also suggests that policies that are inattentive to habit—such as Montevideo’s temporary exoneration of tax obligations as a reward for good compliance behavior—can have perverse consequences. For policy makers, our findings suggest the possible wisdom of raffling up-front rewards or finding other ways to foster the habit of compliance, other than tax holidays. We return to these interpretations in the conclusion, after describing the theory that motivates our study and discussing the background and context of the policy innovation we assess (Section 2); describing our empirical strategy, including our natural and field experimental designs, and presenting our main results (Section 3); and discussing alternative explanations as well as the evidence for the habit mechanism (Section 4).

## **2 Why do people comply? Taxes, incentives, and habit**

Why do people comply with taxes? In this section, we suggest that a standard decision-theoretic model emphasizing the costs of evasion often cannot account for observed compliance behavior. Augmenting this standard framework to include both material and expressive benefits of tax payment holds explana-

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<sup>5</sup>E.g., Downs (1957).

tory promise and suggests that rewards programs like the one we study could conceivably have positive effects on compliance. However, it also leaves important features of tax compliance unexplained. We argue that attention to habit may elucidate several features of our historical data on tax payment, and a focus on habit motivates the interpretation of our empirical results.

In a standard formalization of the compliance problem, taxpayers compare the utility of evasion to the cost of punishment discounted by its probability.<sup>6</sup> For example, 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  $z - pc$ , where  $c$  is the penalty for nonpayment and  $p$  the probability of punishment. In the context of the municipal taxes we study in this paper, 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. To explain systematic non-compliance, researchers often focus on the low value of  $p$ , due to the difficulty of sanctioning tax evasion.<sup>7</sup> In developing countries in particular, states may have limited capacity—what Mann calls “infrastructural power”—to penetrate society and monitor and discipline non-compliers.<sup>8</sup>

Even where administrative capacity is strong and monitoring problems are negligible, however, enforcing punishments for non-payment can pose challenges. In the case of municipal property taxes, tax bureaucrats know appraised values and can identify taxpayers' obligations with certainty.<sup>9</sup> Enforcement becomes an apparently easier problem, that of cajoling taxpayers to fulfill their known tax obligations promptly.<sup>10</sup> Non-compliance is nonetheless endemic. A simple random sample of taxpayer accounts in Montevideo, for which we have payment data from 2000-2014, shows that the municipal government has classified just 50% taxpayers as good taxpayers at any moment in time, based on being current on payments over the previous year (right panel of Figure 1); the compliance rate per tax bill due is around 70% (middle panel), which results in growing average delinquency over time (left panel).

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<sup>6</sup>E.g., Allingham and Sandmo (1972).

<sup>7</sup>E.g. Bates and Lien (1985).

<sup>8</sup>Mann (1984), also Soifer (2008).

<sup>9</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>10</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.



Our interviews with officials in Latin American cities suggest a number of factors, beyond monitoring problems, that can undermine the enforcement of tax compliance through negative incentives. While many municipal governments can in principle seize and auction properties on which taxes are owed, in practice such takings are rare. At most, an embargo is placed on a property so that it cannot be privately sold until debts are cleared (this is enforced through the public notaries that must regulate any sale). The most typical outcome of a legal process against a debtor is renegotiation, in which delinquent taxpayers agree to a payment plan by which past debts are frozen and discounted monthly payments are made. A tax official in Tigre, Argentina notes “In the last 20 years, we have never auctioned either a commercial or residential property. In general, we end up with an agreement.”<sup>11</sup> Renegotiations do not of course result in full compliance: as another Argentine bureaucrat told us, “Payment agreements don’t fix the situation, they generate a new problem for you. Now the person doesn’t only have to pay the monthly rate but also the quota from the agreement.”<sup>12</sup> General amnesties for delinquent taxpayers, particularly in times of economic crisis, are also highly prevalent. For example, Montevideo’s city government issued 11 amnesties at different points between 1997 and 2013.<sup>13</sup> The reasons that tax penalties are not fully assessed likely stretch beyond inefficiencies in the judiciary or bureaucratic agencies. The political unpopularity of taxes can be especially salient in municipalities, where face-to-face negotiations with delinquent taxpayers is common and selective enforcement (or “forbearance,” see Holland 2014) may prevail. As one municipal official put it, for municipal governments “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>14</sup> Thus, even in what might seem like a best-case situation for inducing compliance—strong administrative capacity and limited monitoring problems—the probability of punishment for non-compliance can be very low.

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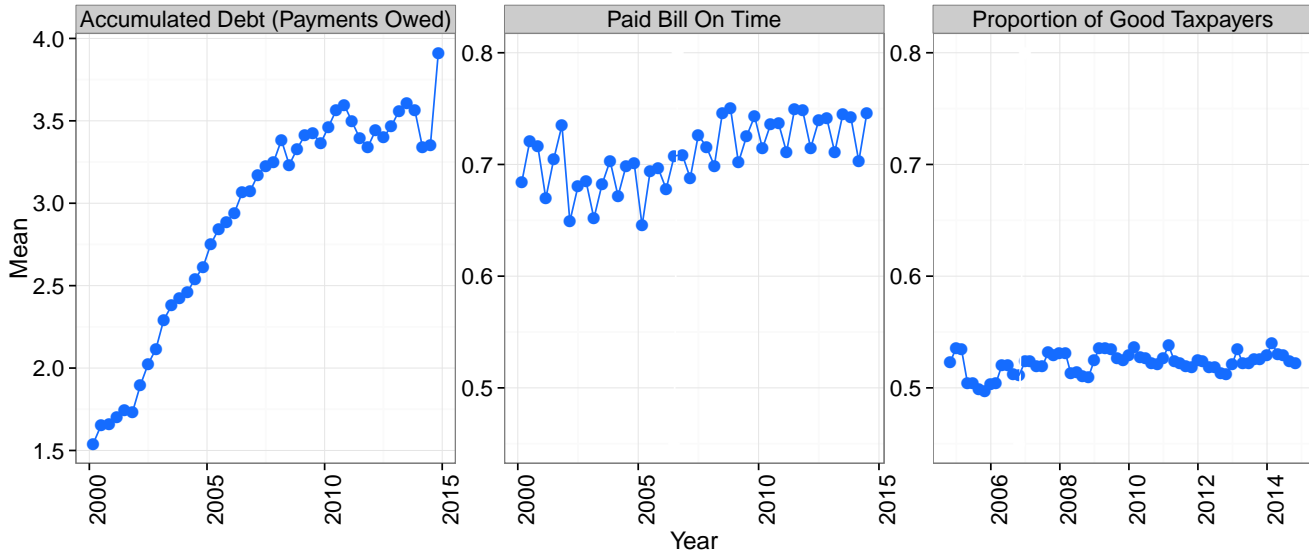
<sup>11</sup>Daniel Chillo, Secretario de Ingresos Públicos; all translations ours. Of course, negotiations happen in the shadow of legal proceedings. A politician in the state of São Paulo, Brazil says “many negotiations . . . happen before [court proceedings are initiated] . . . the majority of the legal actions of City Hall fall by the wayside because [negotiations] were successful.” Geraldo Cruz, current federal deputy from San Pãolo and former mayor of Embu das Artes).

<sup>12</sup>Carlos Maisterrena, Sub-Director de la Administración Fiscal Municipal, Ciudad de Paraná, Entre Ríos, Argentina.

<sup>13</sup>That includes amnesties in the years 1997 (vehicle, property, and head taxes), 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), 2010 (vehicle, property, head, and sewage), and 2013 (property, sewage, head).

<sup>14</sup>Chillo, footnote 16.

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 depicts average accumulated debt (payments owed) per account; the middle panel plots 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.

## 2.1 The benefits of compliance

If the standard framework highlights the difficulty of enforcing compliance through negative incentives, it also suggests that the provocative question may not be why some people don’t pay taxes—but why anyone does. For property taxpayers in many contexts, compliance may not appear fully rational, especially given the low probability of punishment. Notwithstanding substantial non-compliance, Figure 1 suggests that many people do nonetheless comply.

One natural possibility is that the material or expressive benefits of paying taxes induce compliance. If so, governments might therefore seek to boost the perceived benefits of payment. Latin American municipal governments from Salta, Argentina to Peruíbe, Brazil to Miraflores, Peru now raffle prizes—from televisions to new cars and houses to discounted payments—to taxpayers who pay their taxes on time. To assess the prevalence of such policies, we sought to identify the presence of prize programs

in every Latin American country. We took a 10% random sample of municipalities in all countries where we could find any evidence of such a program and then used the Web, interviews with municipal bureaucrats and politicians, and ancillary sources to code whether selected municipalities have a prize lottery for good taxpayers or offer some other sort of incentive, such as discounts for prompt compliance. As Figure 2 shows, 79% of Uruguayan municipalities offer such an incentive, along with an estimated 24% of Brazilian municipalities (134/558 in our sample); incentive programs are prevalent in Argentina, Bolivia, Colombia, Ecuador, Mexico, and Peru as well.<sup>15</sup> These programs thus constitute an important policy innovation in these developing countries. Rather than being punished for poor tax-paying behavior, citizens are therefore rewarded for their history of tax compliance.

Do such incentive programs induce greater compliance? Many interviewees believe that they do. As an official in Tigre, Argentina noted, “We have a compliance rate of 85% with the Municipal Service Tax today, whereas when we started [prize lotteries] in 2009, it was at 68%.”<sup>16</sup> While perhaps an example of “casual” rather than causal inference, the theory of change appears plausible: prizes may not only sustain compliance among good taxpayers but also induce bad taxpayers to bring their accounts up to date. As an Argentine official noted, his municipality’s 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>17</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 . . . were shown, a lot of municipalities adopted these policies.”<sup>18</sup> In Montevideo, the center-left government of the Frente Amplio initiated its lottery in the context of an amnesty for many delinquent taxpayers, following the economic crisis of 2002; it was in part intended to counteract perceived negative perceptions among citizens of forgiveness for

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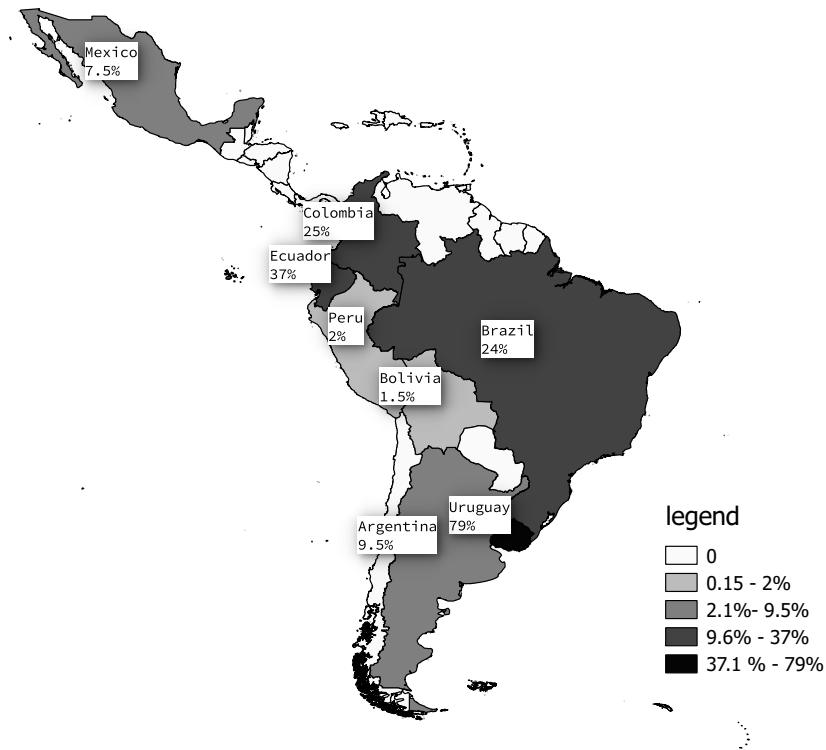
<sup>15</sup>The frequency of lotteries outside of Brazil is lower, and our 10% random sample offers a smaller *N* of municipalities, so inferences in other countries should be drawn with more caution. Nonetheless, our investigation suggests the prevalence of positive incentive programs for good taxpayers.

<sup>16</sup>Daniel Chillo, Secretario de Ingresos Publicos, Tigre, Bs As, Argentina; all translations ours.

<sup>17</sup>Carlos Maisterrena, Sub-Director de la Administración de Fiscal Municipal, Ciudad de Paraná, Entre Ríos, Argentina; translation ours.

<sup>18</sup>Beto Tricoli, Prefeito de Atibaia (2001-2008), Brazil.

Figure 2: Reward Programs for Good Taxpayers in Latin America



The figure shows the percentage of municipalities that offer reward programs for up-to-date taxpayers. Based on a 10% random sample of municipalities in Argentina, Bolivia, Brazil, Colombia, Ecuador, Mexico, and Peru, as well as a census of municipalities in Uruguay; in other Latin American countries, we identified no rewards programs using Web searches and ancillary sources.

non-compliance through amnesties.<sup>19</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>20</sup>

<sup>19</sup>For discussion, see [http://historico.elpais.com.uy/Suple/LaSemanaEnElPais/04/10/29/lasem\\_ciud\\_118264.asp](http://historico.elpais.com.uy/Suple/LaSemanaEnElPais/04/10/29/lasem_ciud_118264.asp); [http://www.montevideo.com.uy/notnoticias\\_66228\\_1.html](http://www.montevideo.com.uy/notnoticias_66228_1.html); and <http://www.180.com.uy/articulo/14284>. Accessed May 2, 2015.

<sup>20</sup>In October 2013, the municipality announced a renewed amnesty for certain bad taxpayers, underscoring the difficulties of

Yet, from the perspective of the standard framework, the material benefits of rewards programs alone appear unlikely to boost tax payment. Under Montevideo's lottery policy, good taxpayers win a year free of tax payments with probability  $1/5,000$  in any tax payment period. Thus, following the notation above, the expected utility of paying the full year's taxes this year is (without discounting)  $(1/5,000)z - z$ . Thus, in the case of full year's debt, a taxpayer has to pay a year's worth of taxes  $z$ —in order to gain, with probability  $1/5,000$ , a year free of tax payments  $z$  in the following year. For virtually all delinquent taxpayers, the expected monetary value of rectifying accounts is likely to be negative.<sup>21</sup> To be sure, casinos would make no money if gamblers did not take bets with negative expected values; yet this does not look like a promising deal for the taxpayer.<sup>22</sup>

In addition to providing a direct material benefit, however, prize lotteries such as Montevideo's may possibly affect non-material or expressive benefits of paying taxes, e.g. by influencing perceptions of the fairness and equity of the tax system. Several interviewees pointed to exactly these kinds of benefits. For example, some flagged the importance of prize lotteries' 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>23</sup> As one Brazilian mayor put it, "the best weapon [against non-compliance] is transparency."<sup>24</sup> The public nature of this lottery also underscores an additional theme, recognition.<sup>25</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

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cracking down on non-compliance. There have been amnesties in 2004, 2008, and 2010, among other years. See also URLs in previous note.

<sup>21</sup> 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. However, the *expected* value of winning any lottery is then US\$265/5,000, or about five US cents. We estimate these values from the control group in our natural experiment described below.

<sup>22</sup> For discussion of positive and negative incentives such as lotteries and state amnesties, see (Parle and Hirlinger, 1986; Loftus, 1985; Alm, McKee and Beck, 1990; Falkinger and Walther, 1991; Alm et al., 1990). According to (Smith and Stalans, 1991), the costs of implementing governing programs that give taxpayers positive material incentives to comply with tax laws may outweigh the benefits.

<sup>23</sup> Chillo, footnote 16.

<sup>24</sup> Geraldo Cruz, current Diputado from San Pãolo and former mayor of Embu das Artes; translation ours.

<sup>25</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.

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>26</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... 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>27</sup> To extend the standard framework for modeling compliance, let  $b$  therefore be an expressive benefit of paying taxes. Under Montevideo’s lottery program, the expected net utility of non-compliance is then  $z - (1/5,000)z - b - pc$ . Thus, tax payment occurs whenever  $pc > (\frac{4,999}{5,000})z - b$ , which is satisfied more easily the larger is  $b$ . It is possible that by boosting both the monetary and expressive benefits of paying taxes, the incentive programs cropping up throughout Latin American could induce greater tax compliance.

## 2.2 Habit as a cause of compliance

However, this extended framework is also incomplete—and leaves unexplained several key features of tax payment data that we explore empirically later. Suppose instead that the tendency to comply with taxes is also partly habitual.<sup>28</sup> For example, let  $\gamma_t = 1$  if a taxpayer fails to comply with taxes at time  $t$  and otherwise equals zero. A framework that accommodates habit formation gives the expected utility of choosing  $\gamma_t = 1$  as  $z - (1/5,000)z - b - pc + \theta\gamma_{t-1}$ . Here,  $\theta \in (0, 1)$  captures the sensitivity of current tax payment to past compliance behavior. Note that by a recursive argument, the “stock” of habit at time  $t$  is given by  $S_t = \sum_{i=1}^{\infty} \theta^i \gamma_{t-i}$ .<sup>29</sup> Thus, tax payment could therefore become nearly automated through behavioral repetition. Completing the argument, one could suppose that compliance is not a deterministic function of these parameters but is also affected by some mean-zero random noise  $v_t$ : thus,  $\gamma_t = 1$  if  $z - (1/5,000)z - b - pc + S_t + v_t > 0$ . Social psychologists have developed a specific understanding of “habit” as involving repetition of a response under similar conditions, so that the

<sup>26</sup>Chillo, footnote 16.

<sup>27</sup>Carlos Maisterrena, Sub-Director of Fiscal Administration, Ciudad de Paraná, Entre Ríos, Argentina.

<sup>28</sup>Our registered pre-analysis plan considered this possibility; see Dunning et al., July 23, 2014. “Pre-Analysis Plan–Positive vs. Negative Incentives for Compliance: Evaluating a Randomized Tax Holiday,” p. 7 and 37 (Mechanism 1B.2).

<sup>29</sup>Similar approaches to modeling the stock of habit is used in economics to study the equity premium puzzle.

response becomes automatically activated when those conditions occur (Wood and Neal 2007). We are not committed to a specific understanding of habit in terms of the role of contextual conditions (though these tend to remain constant in the context we study empirically). We simply note that in this formalization, the benefits of tax compliance today are positively related to tax compliance in the past.

This framework generates several implications consistent with the data on tax compliance we present below. First, behavioral repetition readily generates types of taxpayers who typically comply and those who do not. For taxpayers with large “stocks” of past non-compliance (high  $S_t$ ), non-compliance may usually be the preferred option; while those for whom  $S_t$  approaches zero are more likely to comply. To be sure, if compliance is a random variable, some types with high  $S_t$  might nonetheless comply on some occasions, depending on the realization of  $v_t$ . Yet habit can contribute to the emergence and persistence of “good” and “bad” taxpayers, in the language of the municipal policy we study empirically.

Second, exogenous changes to tax compliance behavior—such as interventions that switch past compliance,  $\gamma_{t-1} = 0$ , to non-compliance,  $\gamma_{t-1} = 1$ —can have lasting effects beyond period  $t$ . After all, compliance at time  $t + 1$  is a function of compliance at  $t$ ; but compliance at  $t$  is a function of compliance at  $t - 1$ . Disrupting the habit of tax compliance at  $t - 1$  may therefore imply less tax compliance at time  $t$ , which breeds lower compliance at  $t + 1$ ,  $t + 2$ , and so forth. Thus, there can be “knock-on” effects of past shocks to tax compliance that extend beyond the immediately following tax payment period.

Finally, however, there is naturally a “decay” in the effect of such habit disruptions. Consider a taxpayer who has always complied until time  $t - 1$  and therefore has stock  $S_t = 0$ . Since she has always complied, such a taxpayer is also likely to have parameter values ( $p$ ,  $c$ , or  $b$ ) that favor compliance.<sup>30</sup> Switching past compliance  $\gamma_{t-1} = 0$  to non-compliance,  $\gamma_{t-1} = 1$ , makes compliance less attractive at time  $t$  and can have knock-on effects at  $t + 1$  as described in the previous paragraph. However, for many realizations of  $v_t$ , such a taxpayer will again comply—which, given parameter values that favor compliance, will tend to foster continued compliance at  $t + 1$  and subsequent periods. These implications are important because they contrast with many alternative explanations for the empirical findings we present next.

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<sup>30</sup>For ease of presentation, we have not subscripted these parameters; yet it is natural that e.g. the expressive benefit of paying taxes  $b$  varies across taxpayers.

In Uruguay, the great majority of taxpayers pay their taxes in person at local kiosks, or “Red de Cobranzas.” Thus, a compliance habit can form from the repetition of a response (that is, tax payment) under similar conditions, involving the arrival of a tax bill and a trip to the local kiosk or to City Hall to pay taxes due. (A smaller proportion pay by automatic bank debit; we exploit this variation later to test conjectures about habit interruption). Winning the lottery could break the habit of payment and therefore lead winners to pay less reliably than winners for some period of time after winning. In terms of our formalization, by rewarding good taxpayers with a year’s interruption in their tax payment obligations, the tax holiday lottery that we study empirically switches past compliance,  $\gamma_{t-1} = 0$ , to non-compliance,  $\gamma_{t-1} = 1$ , and therefore also affects the stock of habit. Our framework thus suggests that such an incentive program could have quite perverse impacts on future compliance. We turn next to the empirical evidence for this conjecture.

### **3 The negative effect of tax holidays: a natural experiment**



To study the effects of the tax holiday, we use the design of the lottery as a natural experiment. The municipal government uses the results of Uruguay’s National Lottery to select taxpayers for holidays. Thus, account numbers the final four digits of which correspond to the winning number of the relevant National Lottery are selected as provisional winners.<sup>31</sup> In February 2009, for example, the winning National Lottery number ended in 8662. In this instance, the municipality first identified all taxpayer account numbers also ending in 8662, across four types of taxes—head (“Tasa General Municipal”), vehicle (“Patente de Rodados”), property (“Contribución Inmobiliaria”), and sewage (“Tarifa de Sanamiento”)—then screened in those taxpayers who owed no past taxes and had paid on time over the previous year. This screening process leaves the winning “good taxpayers” illustrated in Figure 3, from a screenshot off the municipality’s website. The municipality sends a letter to the addresses associated with each of these accounts indicating that winners should come to City Hall

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<sup>31</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](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](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.



Figure 3: The Tax Holiday Lottery: Selection of Winning Account Numbers

Institucional		Trámites y tributos		Tránsito y transporte		Servicios y sociedad		Empresas		Ciudad y cultura	
 											
<b>Sorteo Bimestral para Buenos Pagadores</b>											
<b>Listado de Posibles Cuentas Ganadoras</b>											
Sorteo Bimestral Febrero 2009 Terminación: 8662											
Tasa General Municipal			Patente de Rodados			Contribución Inmobiliaria			Tarifa de Saneamiento		
Cuenta Corriente	Cuenta Corriente	Identificación	Cuenta Corriente	Identificación	Cuenta Corriente	Cuenta Corriente	Identificación	Cuenta Corriente	Cuenta Corriente	Identificación	Cuenta Corriente
638662	1208662	305612/SAW9167	218662	564/401	2798662	668662	1318662	360356/SAH2587	258662	10282/301	2808662
678662	1328662	382765/SAQ2304	268662	14659/401	2878662	688662	1408662	463531/SAH7615	288662	21837/101	2898662
728662	2208662	904461/SAG7716	298662	26220/803	2908662	768662	3518662	1004335/SBC2701	308662	29734/502	3168662
798662	3618662	1016292/SBD1254	318662	31711/503		838662	3718662	1028575/SBD7490	328662	34080/601	
948662	3868662	1056616/SBE8813	338662	39851/4		1008662	3978662	1078082/SBF6388	418662	85343	
1018662			468662	114666		1018662			478662	121098/3	
1038662			508662	141019/1		1078662			538662	161294/202	
1098662			548662	168557		1118662			2028662	151499	
1738662			3778662	1712/202		2148662					

The figure shows a screenshot of winning taxpayer accounts in the February 2009 tax holiday lottery. Across four types of taxes—head (“Tasa General Municipal”), vehicle (“Patente de Rodados”), property (“Contribución Inmobiliaria”), and sewage (“Tarifa de Sanamiento”)—the figures shows all taxpayer accounts that end in “8662” and for which taxes have been paid promptly over the previous year. Source: <http://www.montevideo.gub.uy/sorteosBP/pages/consCuentasSorteadas.xhtml>, accessed April 4, 2016.

to claim their year-long tax holiday.<sup>32</sup>

To construct the study group for our natural experiment, we first identified all taxpayer accounts that were randomly selected in each lottery since 2004. Our treatment group consists of winning taxpayers who were eligible to claim the tax holiday: they were all “good taxpayers” in the year prior to the date of the lottery in which their account number was selected.<sup>33</sup> Constructing the appropriate control group requires some care: the right counterfactual consists not of currently eligible non-winners but rather sets of taxpayers who were eligible to win as each past lottery, but whose account numbers were not randomly selected in the tax holiday lottery.<sup>34</sup> To create the control group, we randomly generated a four-digit number, different from the winning number, for each lottery since 2004, then screened in all taxpayers whose accounts ended in these numbers and who were “good taxpayers” as of the date of the corresponding lottery. Our procedure therefore mimicked the random process that created the treatment group. We requested data for ineligible as well as eligible taxpayers with the selected account numbers. Although we only use data for eligible taxpayers to estimate treatment effects (since only those taxpayers could potentially win a tax holiday lottery), we exploit data on ineligible taxpayers for placebo tests. Our municipal partners provided a time-series panel of tax payment data (2000-2013) for all randomly selected account numbers.

Table 3.1 depicts the size of treatment and control groups (in bold font), distinguishing between the different types of taxes.<sup>35</sup> There are 3,189 eligible lottery winners across all four types of taxes (the treatment group) and 3,174 eligible non-winners (the control group), for a total N=6,363. In addition, we have payment data for 6,150 ineligible taxpayers whose account numbers would have made them

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<sup>32</sup>For head and sewage taxes, the municipality grants such holidays on a bimonthly basis six times a year (in February, April, June, August, October, and December); for vehicle and property taxes, it issues holidays three times a year ( =March, July, and November).

<sup>33</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., the page shown in Figure 3 is titled “Lottery for Good Taxpayers.”

<sup>34</sup>If we were instead to use currently eligible taxpayers as a control group, we would mix taxpayers who were eligible and ineligible as of the date of each past lottery. Since the treatment group only includes eligible winners as of the date of each lottery, this asymmetry would risk bias, if potential outcomes are related to eligibility status (which seems extremely plausible since the outcome is tax compliance).

<sup>35</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. Since winning numbers are selected at random from all four-digit IDs, the probability that a taxpayer account is selected in any one lottery is 1/10,000. Two lotteries are held in the interim between triennial property and vehicle tax payments, and since the probability of winning both lotteries is vanishingly small, the probability of winning at least one is essentially 1/5,000.

eligible for tax holidays, were they up-to-date on their payments; and 6,108 ineligible taxpayers whose account numbers match those in our control group. Note that the cells of Table 3.1 are themselves random samples from the population of eligible taxpaying accounts in Montevideo, as of the date of each lottery. We thus use these random samples to characterize features of the taxpaying population. For example, we combine good taxpayers (N=1,354) and bad taxpayers (1,225) who have not won the property tax lottery for the random sample of 2,579 property tax accounts used in Figure 1.

Table 3.1: **Natural Experiment: Sample Sizes**

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

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.

Figure 4 shows tax compliance behavior over time, pooling across all four types of taxes. The vertical axis measures the proportion of good taxpayers. For each taxpayer, we center the horizontal axis at  $t = 0$ , the date (lottery period) at which the taxpayer won or “lost” the lottery. (Note that non-winners in our control group did not “lose” the lottery in any meaningful competitive sense, since—as we show later—the vast majority are unaware of the existence of the lottery; their account number was simply not selected for the tax holiday). Thus, the horizontal axes show the time elapsed before or after the particular lottery, in connection with which a taxpayer won a tax holiday (in the treatment group) or was sampled into the control group. The right panel of the figure shows those who were classified by the municipality as good taxpayers at the time of the relevant lottery, based on being up to date on their taxes over the previous year; the left panel shows the residual category, “bad taxpayers.” Time is measured in bimonthly increments, i.e., at each new tax holiday lottery. For time periods less than

zero, the plots therefore show pre-treatment tax compliance; 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 is shown with blue triangles.

Before moving to treatment effects, note that the data in Figure 4 show some interesting descriptive patterns. At time zero, the proportion of good taxpayers in the left panel is mechanically zero while in the right panel it is 1—since we define eligible (good) and ineligible (bad) taxpayers as of the date of the relevant lottery. Taxpayers do move in and out of these “good taxpayer” and “bad taxpayer” categories over time: as we move to the left or right away from time zero, the proportion of good taxpayers in the right panel tends away from 1, and in the left panel moves away from zero. Nonetheless, over the more than 10 years of tax data that are available to us, we also see substantial structural stability: among tax payers who were ineligible to win as of the date of the relevant lottery in the left panel, the proportion of good taxpayers never reaches close to the proportion of good taxpayers in the right panel. In sum, “good” and “bad” taxpayers are not rigid categories but there is also intertemporal stability in who is a good or bad taxpayer—suggesting a plausible role for habitual forces.

More meaningful for causal assessment than the overall shape of the plots, however, is the contrast between those with winning account numbers (red circles) and those without (blue triangles). Note that Figure 4 allows two sorts of tests to validate our design assumptions. First, we have a graphical balance test for a highly prognostic pre-treatment covariate—past tax compliance. Focusing on the pre-treatment values in the left and right panels, we can see that tax payment behavior before the relevant lottery took place is statistically indistinguishable for taxpayers with winning numbers and those with non-winning numbers. To validate formally that the data are consistent with random assignment, Appendix Table 1.1 reports balance tests on pre-treatment tax compliance (for lottery periods  $t = 0$  through  $t = -4$ , where  $t$  is the date of the relevant lottery) as well as an indicator for being retired and the 2004 property value in pesos. 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. Geographically, the treatment and control groups are also intermingled evenly throughout the municipality (see Appendix Figures 1 and 2).

Second, the left panel of Figure 4 also permits a placebo outcome test: the “effect” of assignment

to a winning number for ineligible taxpayers, who were ineligible for and thus did not receive any benefit from winning the lottery.<sup>36</sup> There is no statistically discernible difference in post-treatment compliance between ineligible taxpayers with winning and losing lottery numbers. This evidence also supports an important “exclusion restriction”: assignment to a winning lottery number (rather than to the treatment of a tax holiday) did not itself influence compliance behavior.<sup>37</sup> Appendix Table 1.2 reports formal statistical tests which reach the same conclusion. In sum, the data from the natural experiment are strongly consistent with random assignment to treatment conditions and with other identifying assumptions of our design.<sup>38</sup>

Finally, Figure 4 allows estimation of treatment effects: in the right panel of Figure 4, we compare winning and losing account numbers to the right of zero—which gives the effect of a winning number on post-treatment tax compliance for good taxpayers. This is “intent-to-treat” analysis because we are not yet accounting for the fact that some eligible good taxpayers with winning account numbers did not claim their exoneration. Again, 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>39</sup> 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) beginning at the right-hand edge of the grey vertical strip—which measures the point at which all winning taxpayers had tax obligations to pay after the year-long holiday.<sup>40</sup>

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<sup>36</sup>A placebo outcome test validates the assumptions of a design by assessing whether there is an effect where one is “known” not to exist. On placebo outcomes, see Dafoe and Tunón (2014).

<sup>37</sup>On excludability, see Gerber and Green (2012): 39-43, also Dunning (2012).

<sup>38</sup>In section 4, we discuss another core assumption: non-interference. In brief, our evidence suggests that news of the lottery does not likely travel from winners to non-winners: only 8% of non-winners have even heard of the existence of the lottery.

<sup>39</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).

<sup>40</sup>In fact, our grey vertical strip ends approximately two years—12 bimonthly payment periods—after the date at which winners 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.

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



The plots are centered at zero, the date at which each taxpayer in the study group won or could have won a particular tax holiday lottery; the horizontal axis is measured in bimonthly periods before or after this date. Comparisons between winners and non-winners to the left of zero show balance on pre-treatment tax compliance. The left plot shows ineligible taxpayers who were not “good taxpayers” as of the relevant lottery; since none of these taxpayers actually won the lottery, post-treatment comparisons between taxpayers with winning and losing numbers in the left panel provide a placebo outcome test. Finally, post-treatment comparisons in the right plot estimate treatment effects for eligible good taxpayers; this is “intent-to-treat” analysis as we are not yet taking account of whether the taxpayer claimed the exoneration. We estimate treatment effects to the right of the grey vertical strip, when tax holiday winners had to resume their tax payment obligations. Vertical lines show 95% confidence intervals. The evidence suggests a negative effect of tax holidays on future tax compliance (see also Table 3.2).

As the right panel of Figure 4 shows, having a winning lottery number has a negative effect on subsequent tax compliance. Indeed, lottery winners are less likely to be good taxpayers after winning (post-treatment), compared to a comparable group of non-winning eligible taxpayers. Table 3.2 presents the control group means, estimates of the average causal effect of eligibility (i.e., the intent-to-treat analysis) and their standard errors,  $N$ s and  $p$ -values for each test. The first five rows test the effect of winning separately, for years  $t = 1$  through  $t = 5$  (where  $t = 1$  is the first year when all eligible taxpayers were required to pay taxes again).<sup>41</sup> The sixth row pools those five years of payments. The negative effect lasts for around three years and is around 3 percentage points in size. After that point, confidence intervals for the means overlap and we cannot reject the null of no effect.<sup>42</sup>

In addition, note that the analysis in Figure 4 understates the negative effect of winning the lottery for those who actually claim their exoneration. Recall that the municipality sends a letter to the address associated with eligible taxpaying accounts with winning lottery numbers, asking taxpayers to come to City Hall to claim their holiday. However, some eligible taxpayers do not do so. Moreover, a substantial proportion of accounts are associated with firms, who are not eligible for tax holidays, rather than physical persons; without asking to taxpayers to present themselves in person, the municipality does not have a technology for knowing which taxpayers are physical persons. Averaging across taxes, only 36% of eligible taxpayer accounts claim the tax holiday.<sup>43</sup> Formally, this is a situation of “single crossover” from the treatment to the control group, and this problem of non-compliance dilutes the effect of treatment assignment among good taxpayers—that is, having a winning account number. Thus, if intent-to-treat analysis suggest an effect of  $-3.5\%$ , instrumental-variables analysis with winning account number as an instrument for exoneration suggests an estimated effect of  $-0.035/.36$  or  $-9.7\%$ —an increase of nearly 180% in absolute value.

Figure 5 shows those estimated Complier Average Causal Effects (CACEs) for the property tax alone, for all three of our outcome variables: whether the taxpayer paid on time, the number of accumulated past due payments, and the “good taxpayer” measure used in Figure 4. The figure measures

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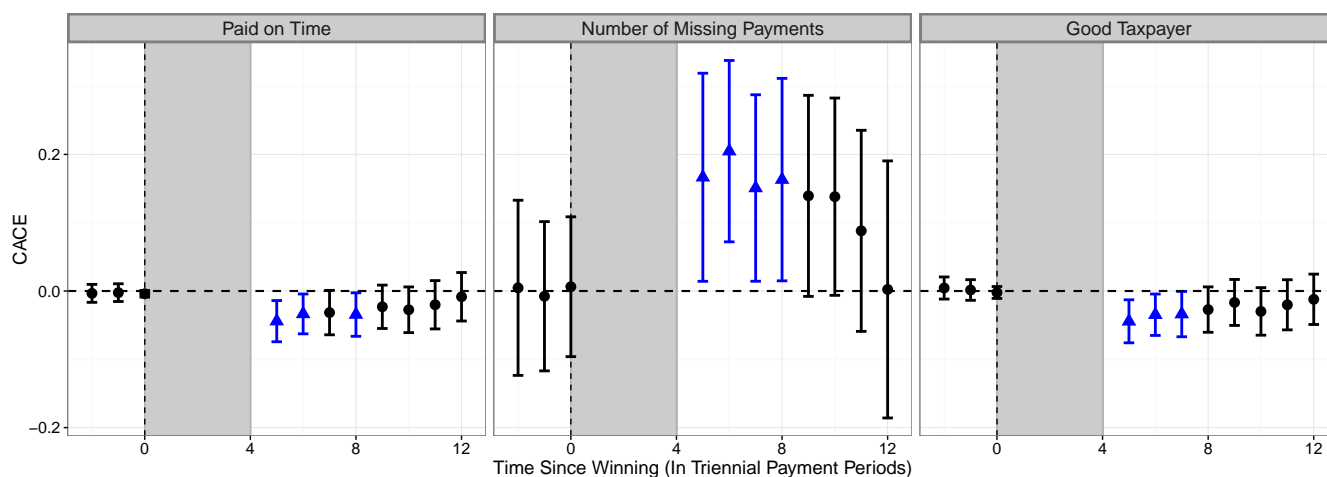
<sup>41</sup>Due to our sampling method described above, the size of the treatment and control groups are random variables; however, this will not lead to bias in treatment effect estimators due to independence of the denominator and the ratio of the numerator to the denominator, in estimators of treatment effects such as the average causal effect.

<sup>42</sup>The final four rows of Table 3.2 present estimates that are pooled across time but broken down by type of tax. We return to cross-tax variation in the next section, but we can see here that the effects are concentrated particularly in the property tax.

<sup>43</sup>The non-compliance rate is similar across types of taxes.

effects at each triennial payment period following the end of the exoneration; a payment period value of 5 is the first period (after the exoneration of one year, or three periods) at which we can be certain that all winning taxpayers had resumed payment obligations. As the figure suggests, effects are large and persistent, lasting at least four payment periods (or over a year) and in some instances plausibly longer. Table 3.2 presents numerical estimates of the Complier Average Causal Effect (CACE), using treatment assignment as an instrumental variable for treatment receipt. Here, we present comparisons for each of five years following the resumption of tax payments, as well as pooled estimates for the five year period, and we disaggregate by tax as well (the heterogeneity in effects is discussed in the next section).

Figure 5: **Property Tax: Complier Average Causal Effects**



The figure measures Complier Average Causal Effects for eligible good taxpayers. The plots show effects at each triennial payment period following the end of the exoneration for three outcomes: whether the taxpayer paid on time (left panel), the number of accumulated past due payments (middle panel), and the “good taxpayer” measure used in Figure 4 (right panel). Estimates with blue triangles are significant at the  $p < 0.05$  level, using normal approximations. The evidence suggests a large and persistent negative effect of tax holidays for Compliers—those eligible good taxpayers who would be induced to claim their exoneration by a winning account number (see also Table 3.2).

In sum, rather than fostering greater tax compliance, winning the tax holiday lottery inhibits it.

We see both a persistent negative effect on compliance and also attenuation of the effect over time.



These features of the data are consistent with knock-on effects of habit disruption discussed in Section 2 but also with their eventual decay, and are important for distinguishing habit from other potential mechanisms that might explain our findings, a point to which we return later. From a policy perspective, the lottery program is a net revenue loser—not only because of the foregone tax revenue during the tax holiday but because the holiday actually depresses future compliance. (Of course, as we discuss below, it could have other redeeming attributes in terms of shaping attitudes towards the transparency or equity of the tax system). For social science and policy alike, however, the important question to which we turn next concerns the explanation for our findings,

Table 3.2: **Effects of the Tax Holiday on the Proportion of Good Taxpayers**

	Control Mean	Average Causal Effect ( $\widehat{ACE}$ )	$\widehat{SE}$ for $\widehat{ACE}$	Complier Average Causal Effect ( $\widehat{CACE}$ )	N	<i>p</i> -value
Post-Treatment Year 1	0.704	-0.035	0.012	-0.097	5021	0.003
Post-Treatment Year 2	0.660	-0.023	0.013	-0.064	4361	0.088
Post-Treatment Year 3	0.604	-0.030	0.015	-.083	3775	0.047
Post-Treatment Year 4	0.561	-0.011	0.017	-0.031	3035	0.528
Post-Treatment Year 5	0.556	-0.028	0.019	-0.078	2347	0.144
Post-Treatment Years 1-5	0.642	-0.029	0.011	-0.081	5021	0.010
Property Tax, Years 1-5	0.790	-0.031	0.015		2024	0.034
Vehicle Tax, Years 1-5	0.260	-0.013	0.020		766	0.520
Sewage Tax, Years 1-5	0.626	-0.051	0.030		653	0.091
Head Tax, Years 1-5	0.639	-0.010	0.020		1578	0.599

The table estimates treatment effects using the proportion of good taxpayers as the outcome variable. The first five rows show the effect at years 1-5, respectively, pooling across taxes, while the sixth row shows the combined effect in years 1-5. The final four rows disaggregate the effects for years 1-5 by type of tax.

## 4 Is habit the mechanism?

Is the disruption of habit the mechanism that drives our results? Good students who are given a year off of homework may be less diligent when made to do homework again. Is a similar force to blame for the negative effects of the holiday on tax compliance?

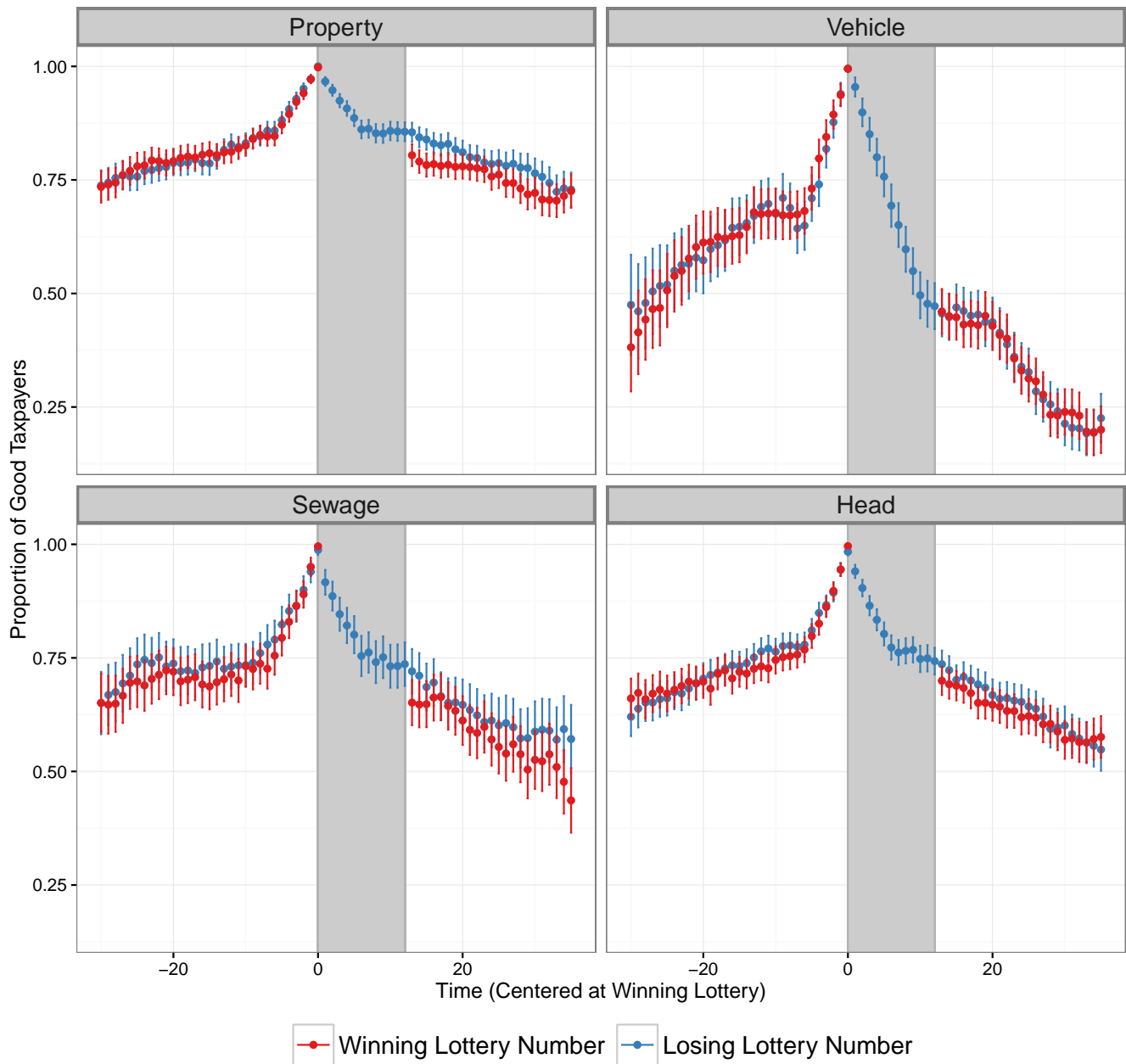
We take several approaches to answering this question. First, we assess the direct evidence for this proposition, showing that the negative impact only holds in settings where the habit of paying taxes is plausibly interrupted. Second, we show evidence from field and survey experiments that casts substantial doubt on the plausibility of alternative explanations. No single piece of evidence is dispositive in this inquiry; however, our results as a whole strongly corroborate the habit hypothesis.

One first piece of evidence comes from variation in effects across types of taxes, already shown in rows 7-10 of Table 3.2. Although we were unaware of this when we began our study, with the vehicle tax—unlike other taxes such as property and sewage taxes—winning taxpayers typically continue to pay taxes in the periods immediately following the winning of the lottery, i.e., in the payment periods included in the grey vertical strip in Figure 4. The reason is that with the vehicle tax, unlike the property tax, payment is exonerated retroactively, so that the *previous* year's payments are forgiven. Some taxpayers take the windfall as a refund, while others take it as a credit against future payments. But even those who take credits typically owe vehicle fees within the following year—because the vehicle tax is often increased annually, or because inflation of the nominal value of the payment (as opposed to the nominal value of the credit at the time the lottery was won) leads to the need to continue making small payments. In sum, winning the vehicle tax lottery does not in fact involve a disruption in the habit of paying taxes to the same degree as other municipal taxes. Thus, cross-tax variation in treatment effects is informative about whether the forces of disrupted habit are responsible for the negative effects of winning a lottery: we should expect weaker or null effects for the property tax.

Figure 6 graphically disaggregates the effect of tax holidays by type of tax. The figure presents data only for good taxpayers, to focus on treatment effects rather than placebo tests. Our results suggest that overall effects for good taxpayers are driven especially by the property tax, and to a lesser extent head and sewage taxes. However, as the figure shows and Table 3.2 confirms, the effects for the vehicle tax are null: we cannot reject the null of equivalence of winners and non-winners on post-treatment vehicle tax compliance. These null results are not dispositive, to be sure: vehicle and property taxes may differ in other ways (especially, in the types of taxpayers who pay them). We are doing some additional work, for instance, to restrict the analysis to 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, these results are suggestive of the influence of habit, since we see

weak effects for taxes where the payment habit is not interrupted.

Figure 6: **Treatment Effects by Type of Tax**



The construction of this figure is parallel to Figure 4, but here we disaggregate by type of tax. Note that this is “intent-to-treat” analysis.

A similar though stronger test comes from the fact that around 21% of the taxpayers in our natural-experimental treatment and control groups had in fact enrolled in automatic payment plans at the date of the relevant lottery.<sup>44</sup> For these automatic payers, the tax holiday does not in fact interrupt the habit of payment. Moreover, after the conclusion of the holiday, payments resume without any action on the part of the taxpayer. (Only in a few cases where there is some problem with the payment scheme would the taxpayer need to become involved). The forces of habit therefore could not conceivably operate to generate negative compliance effects for this group of automatic payers.

As a placebo outcome test, we can therefore assess whether there is any effect of winning on automatic payers—that is, a group for which there is a “known” null effect. Here, we take as our outcome whether each taxpayer paid her bill on time, at each triennial payment period after the conclusion of the lottery; this is a more “memory-less” outcome than the measure used in Figure 4, since it takes a year of compliance to become a good taxpayer. Results using the good taxpayer measure as well as a measure of accumulated debt are similar, though as one would expect the effect lasts longer.<sup>45</sup>

As Figure 7 confirms, the only detectable negative effect of the tax holiday appears among manual taxpayers; for automatic payers, for whom the habit of paying is not interrupted, the lottery does not induce non-compliance. This is strongly suggestive evidence in support of the habit hypothesis. To be sure, taxpayers who pay automatically could differ from manual payers in ways other than their form of payment, and those variables could be related to compliance. How do automatic and manual payers differ? Perhaps surprisingly, the property values of manual taxpayers are substantially greater, by a factor of almost 2.<sup>46</sup> This could be because automatic taxpayers are more likely to be physical persons than firms. While the information in the municipal data do not allow us to measure who is a physical person, 38% of good taxpayers who pay automatically claimed the exoneration while just 26% of manual payers did so. Since both groups are equally likely to win eligibility for the tax holiday, this may suggest a greater prevalence of physical persons among the automatic payers. Note that if firms are more organized about paying their taxes, this might tend to bias *against* finding a habit effect for manual payers and not automatic payers. However, it is also the case that automatic payers are

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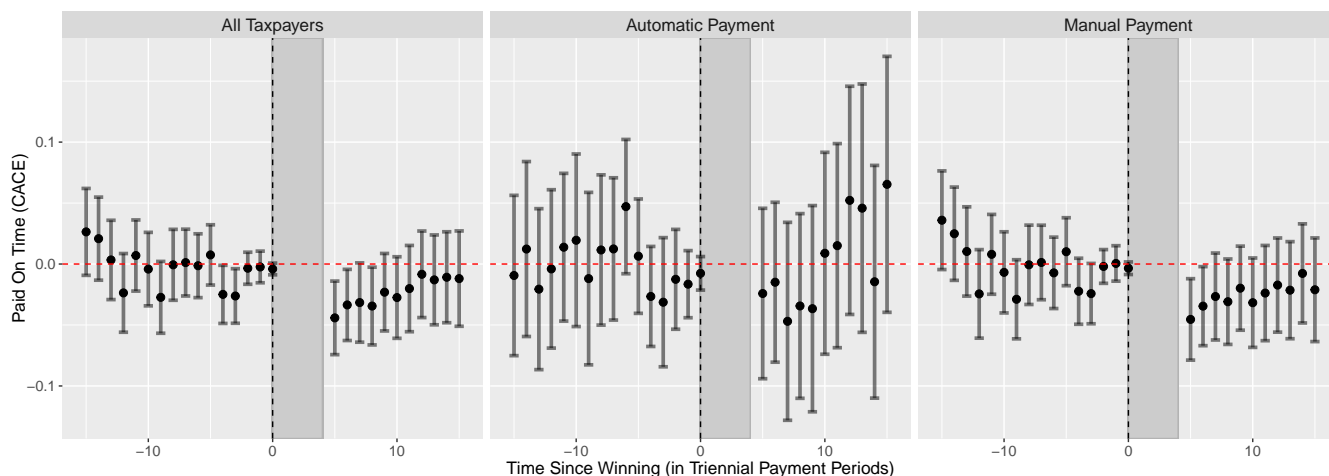
<sup>44</sup>The proportions are 20% for property tax, 15% for sewage, and 26% for head; we do not have data on automatic payment for the vehicle tax. For the property tax, we have missing data on type of payment for only 7 of the 2,693 (1,354 + 1,339) good property tax payers described in Table 3.1.

<sup>45</sup>See the Appendix.

<sup>46</sup>Manual taxpayers: 2,077,318. Automatic payers: 1,128,722 (denominated in Uruguayan pesos).

more likely to pay the entire year of taxes in advance (41% versus 28%) and thus could in general be “better compliers” than manual payers.<sup>47</sup> It is also worth noting that our tests for automatic payers also have lower statistical power than our tests for manual payers, which could conceivably account for the null effects. Notwithstanding these caveats, for the property tax, we have over 500 automatic payers from the 2,693 good property tax payers described in Table 3.1 with which to estimate effects, and point estimates are very close to zero. Our focus on habit suggests that we should only find significant treatment effects for manual taxpayers; and that is exactly what we find.

Figure 7: **Placebo test: Treatment Effects for Automatic Payers (Property Tax)**



The figure depicts the effect of winning the property tax lottery for all taxpayers (left panel), those enrolled in automatic debit at the time of the lottery (middle panel), and those who paid manually (right panel). It shows that no discernible effects exist for automatic taxpayers, whose habit is not interrupted by the tax holiday. Values after 0 estimate treatment effects, while those before 0 provide graphical balance tests.

Compliance histories provide a final useful source of heterogeneous effects to explore. If our argument about the stock of habit is correct, temporary interruptions of tax paying may have the biggest effects on those for whom the habit of tax compliance is not as firmly established. Among our group of good taxpayers—who were all “good” as of the date of the relevant lottery that draws them into

<sup>47</sup>For our entire sample of property taxpayers (both good and bad), about 10% had automatic payment in 2004 and it increased marginally over time to 13% in 2014. So good taxpayers are more likely to have automatic payment plans, as one would expect.

our sample—one might therefore distinguish between those who were always good taxpayers (as measured in our panel data set) and those who were not eligible for the tax holiday at some pre-treatment date. Figure 8 compares treatment effects for these two groups of taxpayers, pooling the analysis across all taxes, and using all three outcomes measures; here, we present estimates of Complier Average Causal Effects. (Conclusions with the ITT analysis, or for the property tax alone, are similar). As the figure suggests, effects are indeed stronger for marginal taxpayers (those with some history of non-compliance) than for always-compliant taxpayers. This provides a final piece of direct evidence in favor of the habit hypothesis.

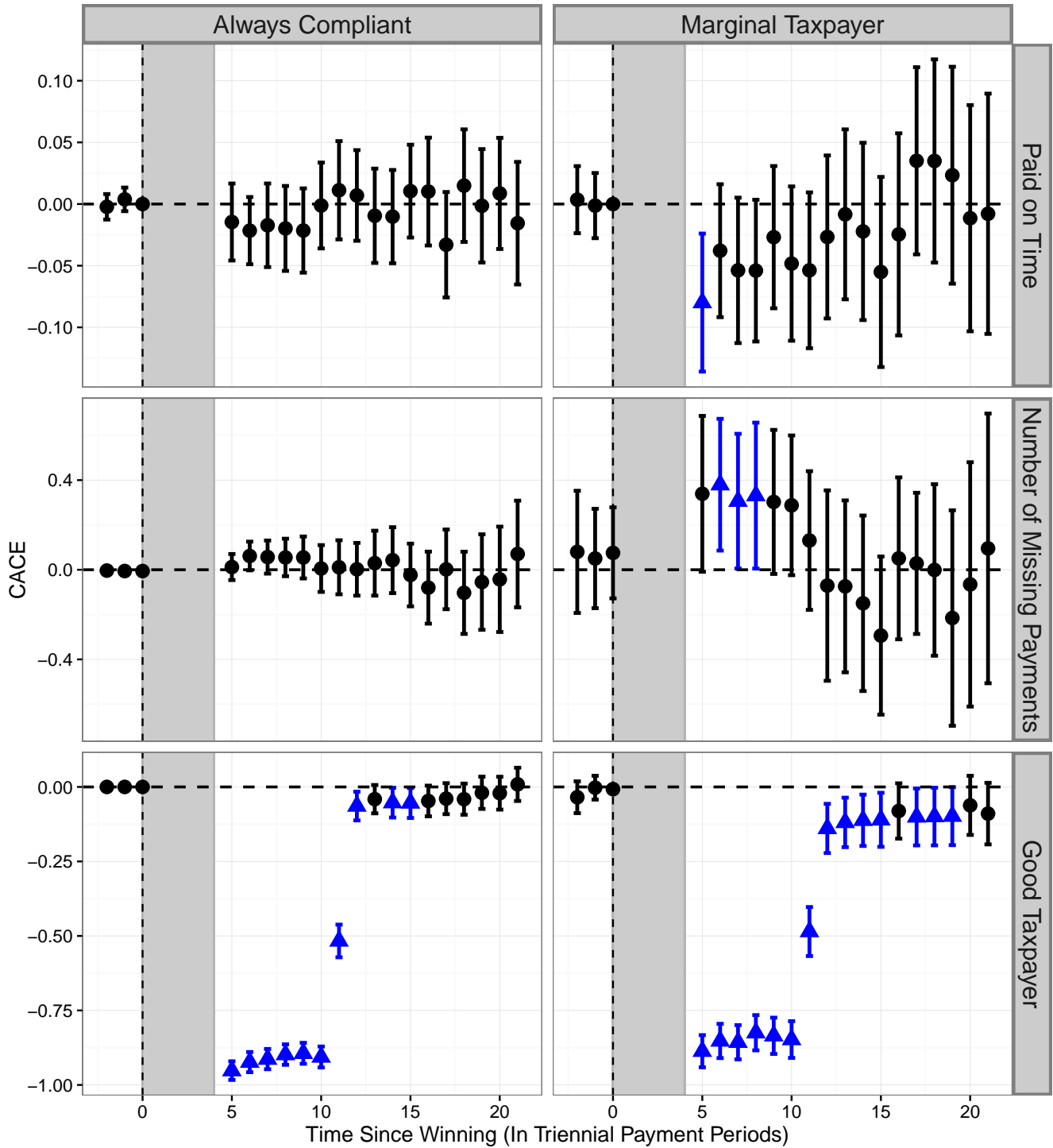
## 4.1 Alternative explanations

There are a number of alternative explanations for the negative effects of tax holiday lotteries which we must assess. Winning the lottery is a bundled treatment. It provides a year off of paying taxes and therefore disrupts the habit of paying taxes. But it also gives winners additional income for a year, which could generate substitution effects that last beyond the holiday period (for example, if taxpayers find it difficult to revert back to a lower consumption level at the end of the holiday and therefore do not fulfill their tax obligations). The experience of forgoing compliance could “break the taboo” of non-compliance among good taxpayers, leading to lower future compliance for reasons other than the disruption of habit. Perhaps most importantly, winning also informs many taxpayers of the existence of the lottery. Baseline knowledge of the program is very low. For example, in our household survey described later, only 8% of respondents identified the lottery as a municipal policy that rewards good taxpayers, while only 5% of survey respondents know someone who has won the lottery.<sup>48</sup> The award letter from the municipality is thus the first time that most winners have heard of the program.

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<sup>48</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).

Figure 8: **Heterogeneous Effects by Pre-Treatment Compliance History (Pooled Analysis, All Taxes)**



The figure depicts the effect of winning the property tax lottery for all taxpayers (left panel), those enrolled in automatic debit at the time of the lottery (middle panel), and those who paid manually (right panel). It shows that no discernible effects exist for automatic taxpayers, whose habit is not interrupted by the tax holiday. Values after 0 estimate treatment effects, while those before 0 provide graphical balance tests.

To explain the negative effects on future compliance, taxpayers would have to interpret this new information in one of several ways. They might infer (incorrectly) that having won the lottery once, their probability of winning the lottery a second time is lowered, which could lead to lower compliance among winners if the lottery indeed exerts a positive incentive effect. Perhaps taxpayers interpret the fact that the government holds a lottery to reward good taxpayers as a signal that it has a hard time eliciting compliance—thus inferring that by complying, they are “suckers” on whom non-compliers are free-riding. A priori, of course, many informational effects might suggest a positive effect on compliance; for example, the lottery could boost perceptions of the transparency or equity of the tax system.<sup>49</sup> Winning the lottery provides credible information that tax holidays will be honored for good taxpayers, and it could even generate a kind of loss aversion among winners who would want to keep paying in future periods in order to retain their status as good taxpayers and their eligibility for future rewards. Yet, winning the lottery could also lead taxpayers to update negatively about the capacity of the tax authorities or the attractiveness of paying taxes.

Each of these alternative explanations has observable implications. To assess their plausibility as sources of our negative effects, we draw on field and survey experiments in which we provided varied information to a random sample of taxpayers. These experiments allow us to unbundle the informational effects of winning the lottery from the effect of disrupting the habit of payment, because we furnish taxpayers with information on the tax holiday absent the awarding of an actual interruption of payments. Our experiments also accomplish two other important objectives. First, while our natural experiment allows us to estimate treatment effects among “good” taxpayers—those who were eligible to win the lottery on the basis of past tax compliance—the field experiment allows us to study impacts for “bad” taxpayers, i.e., those who were not eligible for the tax holiday lottery as of the time of our field experimental interventions. This may allow a more accurate assessment of the overall impact of the tax holiday policy: part of the rationale for such rewards programs, per Section 2, is that they ostensibly encourage bad taxpayers to become compliant in order to qualify for benefits. Second, our experiments also allow us to compare the impact of the positive incentives provided by the lottery with the more typical negative incentives due to punishment for non-payment.

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<sup>49</sup>We discussed both positive and negative informational mechanisms in our pre-analysis plan.



### 4.1.1 Assessing informational explanations: field and survey experiments

We collaborated with the municipal government to design and mail flyers printed with different messages for our field experiment.<sup>50</sup> The messages were designed to inform recipients in different ways about the existence of the lottery (a positive incentive) or about sanctions for non-payment (a negative incentive).<sup>51</sup> Because of the importance of the property tax, our intervention focused on this type of tax. Our baseline reminder serves as a placebo control condition:

**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](http://www.montevideo.gub.uy)).

(Bold text is as in the original Spanish; see Appendix Figures 3-9). Every other condition also includes this baseline reminder.

Our second condition adds to the baseline reminder information about 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.**

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 ad-

<sup>50</sup>Appendix Figures 3-8 depict the flyers and show their Spanish-language messages; Figure 9 shows the back of the flyers, which displays the municipal logo.

<sup>51</sup>Several studies have found that messages increasing the salience of penalties can increase compliance. See for example Slemrod, Blumenthal and Christian (2001); Kleven et al. (2011); Castro and Scartascini (2015).

ministrative 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 substantial: when folded for mailing, the municipal logo is visible, and in fact the flyers appear identical to municipal tax bills before being opened. Indeed, 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 same medium.<sup>52</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 a potential policy intervention, the impact of which we would like to assess.<sup>53</sup>

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. We set a seed for the random number generator, sequentially generated random numbers of the same length as property tax account ID numbers, and then drew the tax records corresponding to those account numbers. The population from which our household survey and administrative data samples are drawn should be conceptualized as “all households subject to property tax with bills due in July 2014.”<sup>54</sup> We aimed to generate a stratified

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<sup>52</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.

<sup>53</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.

<sup>54</sup>For example, we 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

random sample with equal numbers of good and bad taxpayers.<sup>55</sup> Here, a good taxpayer is one who was eligible for the tax holiday lottery as of March 2014. One might imagine that bad taxpayers are concentrated in poorer, outlying areas of Montevideo. In fact, per Appendix Figure 10, good and bad taxpayers are evenly spread throughout the city. We also verified that none of our sampled taxpayers had actually won a lottery in the past, since our goal was assess the effect of informing taxpayers about the possibility of exoneration. Our flyers were distributed in phases in June, such that they would arrive approximately 8 days before tax due dates, which differ for different households.<sup>56</sup> We randomized these taxpayers in our field experiment to one of six treatment groups with equal probability. Table 4.3 shows the sample sizes for each treatment condition. Figures 10 and 11 show visual depictions of balance across our treatment groups on the pre-treatment value of two outcome variables (which are highly prognostic covariates): whether the tax bill was paid on time, at each due date between March 2009 and November 2013, and accumulated missed payments over a similar period. The trend in these pre-treatment variables is very similar across all six treatment conditions and the control group and formal tests suggest that randomization worked to create equivalence between the treatment groups.

Finally, as a way to assess possible impacts of the lottery program on attitudes towards the equity and transparency of the tax system, we used several experiments embedded in our household survey instrument to evaluate how lotteries shape political attitudes, e.g. towards the equity and fairness of the tax system. As reflected in our initial pre-analysis plan, we had intended also to use our field experiment assess the impact of our mailed flyers on attitudes, using household survey data. However, serious logistical failings on the part of our survey partner, the firm CIFRA, made this impossible; for details, see amendments to our pre-analysis plan, which were posted before we had access to any outcome data. However, we can take advantage of embedded experimental variation embedded in our survey instrument to assess impact of information on attitudes, which helps us to assess several alternative explanations.

In particular, we manipulate the description of both rewards and punishments to emphasize their individual or social rationale. These prompts are intended to capture the language and meaning of the

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are not in fact issued bills.

<sup>55</sup>Note that the average value of the “good taxpayer” variable is around 50%, per Figure 1; in point of fact, we ended up with a somewhat larger group of good taxpayers, because 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>56</sup>See our pre-analysis plan and amendments thereto for implementation details.

Table 4.3: **Field Experiment: Treatment Conditions and Sample Sizes**

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

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

flyers to which households were randomly assigned in the field experiment:

**Individual benefit of lottery:** “In connection with the National Lottery, Montevideo’s City Hall raffles the exoneration of municipal taxes for one year. This policy rewards individually those who are up-to-date on their taxes.”

**Social benefit of lottery:** “In connection with the National Lottery, Montevideo’s City Hall raffles 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.”

**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.”

These prompts were randomly assigned to respondents, with two prompts per instrument. (Prompt order was randomized). We registered the following outcome variables, where respondents are asked for their degree of agreement on a 0-10 scale except as noted: 1. “In general, the municipal government does a good job.” 2. “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?” 3. “Policies that reward good taxpayers are a waste of money.” 4. “In Montevideo, benefits for good taxpayers go to the same people as always.” 5. “In Montevideo, it is worth it to be up to date on ones taxes.” 6. “Fines and charges for bad taxpayers are pointless.” 7. “People only pay their taxes on time when there are substantial fines and charges.” 8. “In Montevideo, punishments don’t apply to the privileged”. Note that not all of these outcome questions were asked for each experimental conditions; for example, it does not make sense to ask certain questions about punishments after exposing subjects to information about the lottery.

## 4.2 Results

How did our informational interventions shape tax compliance behavior? First, our interventions as a whole did affect behavior on at least one measure of compliance, relative to a pure control group that received no flyers. Second, however, information about the lottery or about sanctions for non-compliance had no additional effect, above and beyond our placebo control reminder.

Thus, Figure 9 compares outcomes in the six treatment conditions to the pure control group that received no flyers, on two variables: Intended Compliance (whether the account holder accessed his or her Web account, for example, to print a duplicate bill); and Paid Bill On Time, as measured in July 2014, the first payment period following our intervention.<sup>57</sup> For both good and bad taxpayers, nearly all the types of flyers elevated Intended Compliance, relative to the control group.<sup>58</sup> The right-hand panel of Figure 9, meanwhile, suggests that our flyers also boosted actual compliance, relative to the pure control group—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

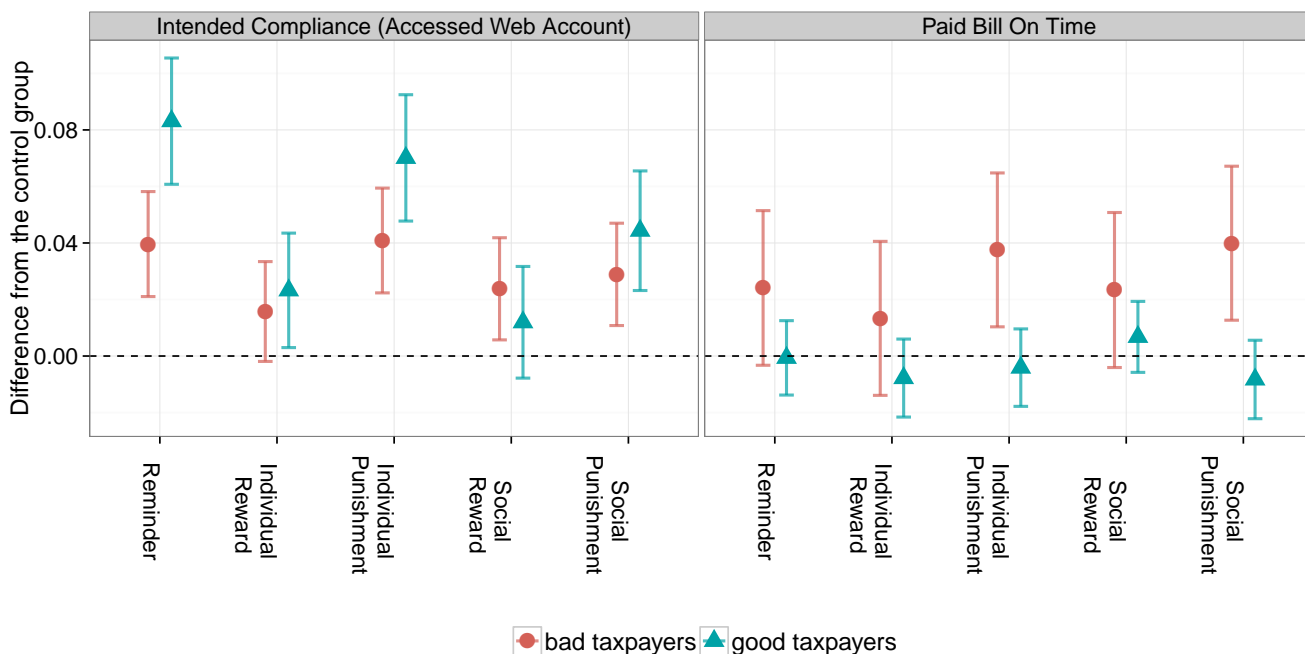
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<sup>57</sup>Note that we did not pre-register the analysis in Figure 9 because the comparison of treatments to the placebo control is our main strategy for assessing the effects of positive vs. negative incentives. This analysis is nonetheless instructive as a kind of manipulation check.

<sup>58</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.

by Castro and Scartascini (2015), in their study of the effects of punishment threats on tax compliance in Junín, Argentina.<sup>59</sup> However, as we show next, this is likely due not to the threat of sanctions—but instead to the simple reminder included with the placebo control and all the other conditions. The evidence in Figure 9 thus suggests that some taxpayers read and responded to our flyers, but it does not show that messages about positive or negative incentives did the causal work.

Figure 9: Treatment effects, good and bad taxpayers, relative to control group



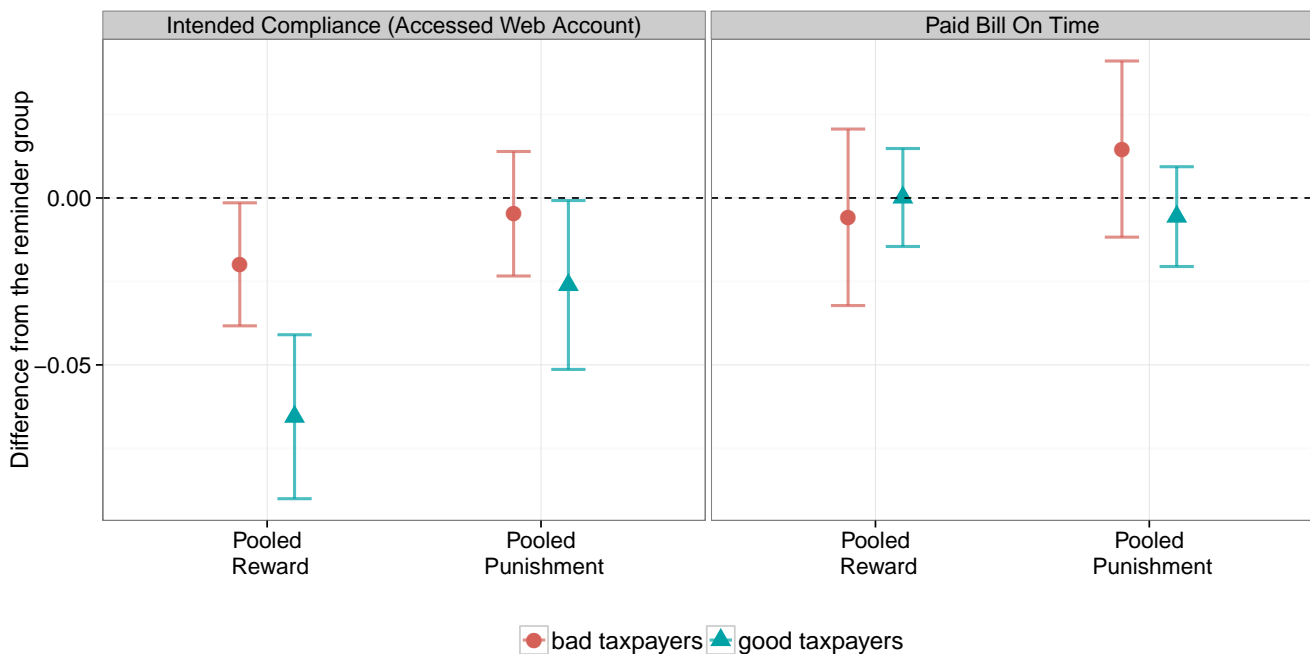
Thus, Figure 10 compares each flyer to the placebo control rather than the pure control.<sup>60</sup> Here, we also pool the “benefit” and “punishment” conditions for additional power. This comparison shows that there is no positive impact of information on either punishments or rewards on intended or actual compliance, relative to the reminder condition. And indeed, the nominal significance test suggests that the pooled reward condition actually drives *down* compliance, relative to the simple reminder (though this does not adjust for multiple comparisons). Appendix Table 2.3 shows the means, standard errors,

<sup>59</sup>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 (Appendix Figure 15).

<sup>60</sup>This is the analysis we registered in our pre-analysis plan.

and  $p$ -values for these comparisons. It is difficult to know for sure why the simple reminder itself had the largest apparent effect. It is possible that the volume of text on the other flyers could have distracted from the simple, powerful message reminding taxpayers that their account is due and that duplicate copies could be obtained on the web. We return to this question in the discussion below but note here that had we failed to include a placebo control, we would have concluded that the messages on our informational interventions had important effects. Instead, we can see that information about the lottery or information about punishments for non-payment had little discernible effect on behavior.

Figure 10: Treatment effects, good and bad taxpayers, relative to reminder group (pooling benefit and punishment conditions)



Nor does our evidence suggest any heterogeneous effects of our informational interventions. We hypothesized for example that effects might be largest for marginal taxpayers—such as 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. We reasoned that such taxpayers may be especially responsive to our experimental prompts, since the cost to them of

keeping or bringing their accounts up to date might be relatively minimal (compared to, say, seriously delinquent taxpayers). As the analysis in Table 4.4 shows, however, there are no discernible treatment effects for this group.

Table 4.4: **Heterogeneous effects for “marginal” taxpayers**

Treatment	Outcome	Taxpayer Type	Reminder Mean	Effect	SE	N	p-value
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

Finally, how do our informational interventions shape attitudes towards the equity and fairness of taxes, as well as broader political attitudes? This question is relevant both for evaluating alternative explanations for our findings—for example, in assessing the hypothesis that learning about the lottery led winners to update negatively about state fiscal capacity—and for normative interpretation and policy recommendation. For example, it might be that the tax holiday is a net revenue loser for the municipal government and yet it improves attitudes towards the tax system, in which case the welfare implications of the program may not be clear-cut. We therefore close with evidence from our survey experiment.

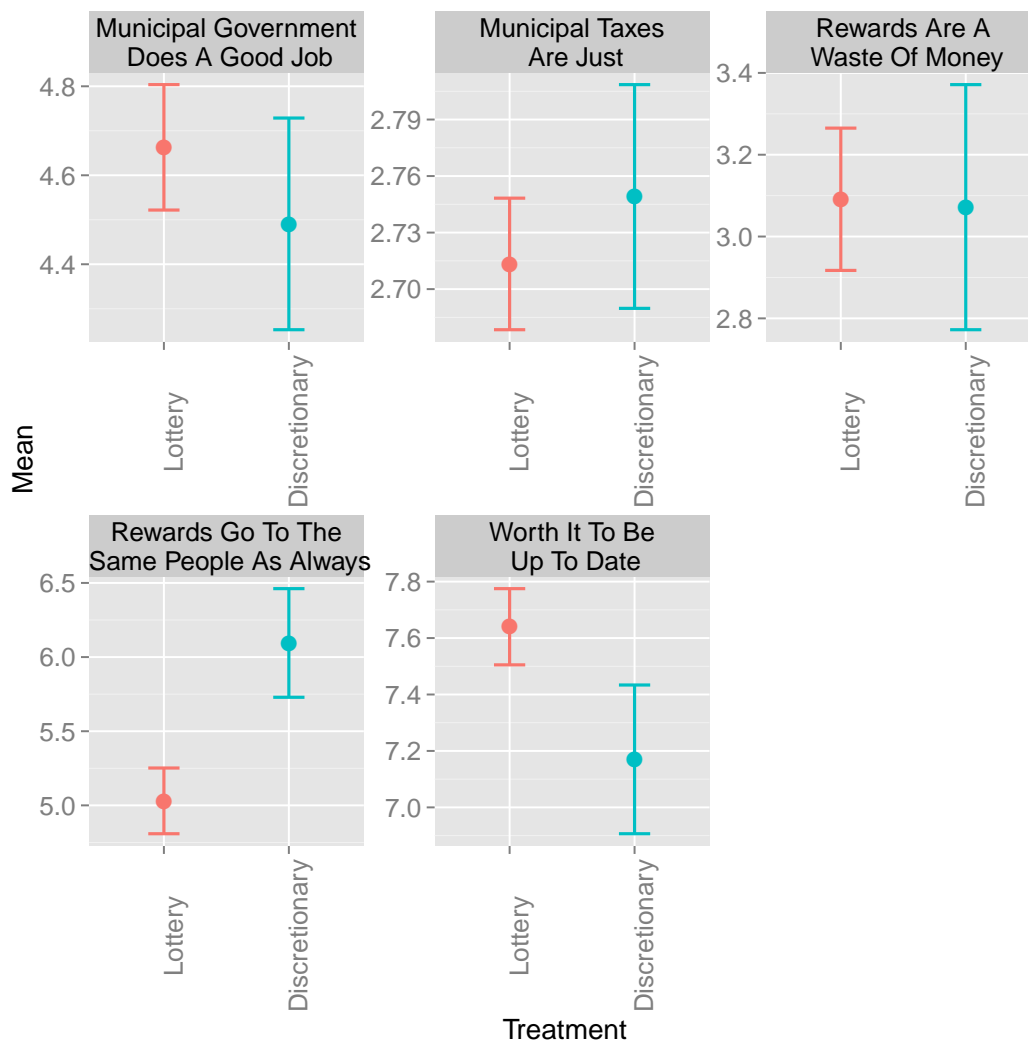
Figure 11 shows differences of means and associated confidence intervals, comparing the treatment that provides information about the lottery (pooling primes of the individual and social benefits of the lottery) to the “discretionary benefit” condition.<sup>61</sup> Recall that respondents assigned to the latter condition were told that the city of Montevideo “from time to time selects taxpayers for benefits,” without specifying that this selection is done at random through a lottery; those in the former condition were informed explicitly that the selection was done through a randomized lottery. We present effects for the first five outcome variables described above, which were the only one asked consistently for subjects randomized to one of these two treatment conditions. For the outcomes in the top panel, comparisons across the conditions show no significant differences. In particular, we do not find an

<sup>61</sup>Appendix Tables 3.7 and 3.8 show estimated effects for social vs. individual rewards separately; and pooled punishment vs. pooled rewards.



effect of exposure to information about the lottery on whether the municipality is said to do a good job, whether people perceive the tax system as just, and whether rewards are seen as a waste of money

Figure 11: **Survey experiment: Lottery vs. Discretionary Benefit**



The table presents mean outcomes after providing either (1) information about the existence of the lottery (pooling the individual and social benefit conditions) or (2) the discretionary benefit condition, using data from our survey experiment. Both differences of means are significant in the bottom panel, after using a false-discovery rate (FDR) correction to adjust  $p$ -values for multiple statistical comparisons.

However, as the bottom panel of the figure shows, we do find evidence that lotteries are viewed as less discretionary: exposure to the lottery treatment rather than the discretion treatment significantly diminishes the tendency to say that rewards “go to the same people as always,” and also significantly

boosts the propensity to say that it is worth it to be up to date on taxes.<sup>62</sup> Thus, lotteries are perhaps seen as a transparent way to allocate benefits, and they could also in principle increase incentives for being up-to-date on taxes. Of course, we found no evidence that information about the lottery shapes actual behavior for either good or bad taxpayers, even for a measure of intended compliance as costless as logging on to the tax account. These results perhaps only heighten the salience of our finding that winning a tax holiday lottery inhibits future compliance. Despite some evidence that lotteries could improve some attitudes towards the tax system, the tax holiday policy has a negative effect on tax receipts. And as we discuss next, the bulk of the evidence suggests that it is the disruption of habit rather than alternative mechanisms that explain this negative effect.

#### **4.2.1 Discussion**

What light does the evidence in the previous sub-section collectively shed on alternative explanations for the negative effect of tax holidays on tax compliance? In brief, informational or other behavioral explanations receive substantially less support than an interpretation focused on the forces of habit.

Consider first the hypothesis that lottery winners incorrectly infer that having won the lottery once, their probability of winning the lottery a second time is lowered. Our household survey described below does support the existence of misperception along these lines. For example, we described the lottery to a random sample of respondents and then asked whether the probability of winning the lottery a second time would be more, the same, or less for someone who had already won the lottery. Though the probabilities are in fact independent, 42% answered that the chances of winning again would be lower than the initial chance of winning.<sup>63</sup> However, to explain the negative effect of winning the lottery on future compliance, the lottery itself would have to exert a powerful positive incentive to pay taxes: otherwise, lottery winners wouldn't be induced to comply at lower rates by the presumption that their chances of winning the lottery a second time are less than before. We find very little evidence—and no behavioral evidence from actual tax compliance measures—that knowledge of the lottery induces

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<sup>62</sup>This finding is related to research on how individual perceptions on government spending can affect tax compliance. See for example Cowell and Gordon (1988).

<sup>63</sup>Around 2% of respondents said the chance of winning again was larger than the original probability, while 55% said it is the same. Also worth noting is that 84% of respondents underestimated the chances of winning the lottery, in closed-ended question: 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.

greater compliance, as a such a positive incentive effect would presume. A second point of evidence against this hypothesis is that the effect of this misperception should be more or less permanent: i.e., winning should permanently lower the probability of compliance for those taxpayers induced to evade at higher rates by having won the lottery. Yet, our evidence suggests that the negative effect of winning on compliance, while persistent, eventually decays and disappears.

Other informational explanations for the negative compliance effect of winning the tax holiday receive similarly weak support. For example, there is little evidence that taxpayers interpret information about the lottery as a negative signal about government capacity. It is true that we find lower “intended compliance” (access to the Web account) among taxpayers who are informed of the reward, relative to the placebo control group that only received a reminder. Yet, there is no negative effect on actual tax payment. And in our survey experiment, knowledge of the randomized reward program does not affect the extent to which citizens perceive the municipal government as doing a good job, or the degree to which taxes are perceived as just. Instead, our survey experiment suggests that the lottery is viewed as a transparent or equitable way of distributing rewards.

Two other potential alternatives are worth flagging and dismissing. First, we discussed above the possibility of Income or substitution effects, in which winners put the additional income not spent on taxes to other uses for the year and find it difficult to shift this income back to taxes when the tax holiday wins. While possible, these types of explanations might suggest heterogeneous effects of the lottery for different sorts of taxpayers—e.g., the lottery might have an especially negative effect for types of taxpayers in which winning would be most likely to induce such income or substitution effects. We test for such heterogeneous effects in subsection 4.1 but find little support either in the field or natural experimental evidence. Finally, it is possible that winning a tax holiday breaks a “taboo” against non-compliance among good taxpayers. But like the effects of misperception, one might expect this behavioral explanation to produce more or less perpetual shift in non-compliance, rather than the decay in the effect that we observe in the data.

Habit disruption therefore receives the most support in our evidence. For reasons discussed in section 2, exogenous manipulation of the habit of tax payment may have persistent, knock-on negative effects on compliance; yet these effects should also decay over time. We find this to be the case in our data. Moreover, for taxpayers whose habit is not disrupted—those who paid by automatic debit

at the time of winning and who were therefore automatically re-enrolled for payments at the end of the holiday—there is no negative effect of winning. Nor is there a negative effect for vehicle taxes, where the payment habit is not typically interrupted. Because the payment context and technology is unchanging, it appears most plausible that this effect is due to the removal of a repeated behavior; and especially given the lack of support for other alternatives, we conclude that the most plausible explanation for our negative effect is interruption of the habit of payment.

## 5 Conclusion

Scholars have often suggested that habit could have a causal effect on political participation and citizen-state interaction, yet outside of the realm of voting this conjecture has not been extensively tested. Using natural as well as field and survey experiments, and leveraging our access to individual tax payment records, we find that interrupting the habit of tax payment among good taxpayers has a persistent negative effect on future compliance. Our data suggest that while there is individual variation in compliance over time, taxpayers also tend to be sort into the categories of “good” and “bad” taxpayer. Our evidence suggests that the forces of habit may contribute to such virtuous or vicious cycles in civic participation.

Our conclusions have policy as well as social-scientific implications. Municipal governments throughout Latin America have recently developed positive incentive schemes such as randomized lotteries to boost tax compliance. 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. Yet our results suggest that policy makers ignore effects on habit at their peril. Using an example from the capital city of Uruguay, we show that by interrupting the compliance habit, a program that offers a tax holiday to good taxpayers actually induces lower future compliance.

To be sure, our results should not condemn efforts to foster tax compliance through positive incentives elsewhere in the developing world. Many other programs lack the feature of the tax holiday, instead raffling in-kind benefits such as cars or houses, or offering discounts on payments rather than

a year-long exoneration. The impact of those other programs is untested, but for Montevideo, one policy recommendation could be to enroll winners in automatic payment plans. (The impact of such an intervention can and should be experimentally tested, however). Many policies in Latin America also have other 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 to their success.<sup>64</sup> Our results also suggest that prize lotteries could have other salutary effects on attitudes towards taxation or the equity and transparency of the state. Yet, our findings counsel close attention in designing policies to the impact on habit. And they provide compelling evidence that habit disruptions have knock-on consequences in a critical realm of citizen-state interaction.

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<sup>64</sup>See Chetty, Mobarak and Singhal (2014) for evidence on the impact of social pressure on tax compliance in Bangladesh.

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