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Evidence from a Field Experiment

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Abstract

Tax evasion is a pervasive problem in many countries. In particular, some developing countries do not collect even half of what they would if taxpayers complied with the written letter of the law. The academic literature has not been oblivious to the need to explain why people pay (or do not pay) taxes. However, the empirical literature has not yet reached consensus. This paper reports the results of a large field experiment that tried to affect compliance by influencing property tax taxpayers' beliefs regarding the levels of enforcement, equity, and fairness of the tax system in a municipality in Argentina. Results indicate that the most effective message was one that stated the actual fines and potential legal consequences taxpayers may face in the case of noncompliance (tax compliance increased by more than 4 percentage points). No average effects are found for the treatments designed to affect beliefs about the equity and fairness of the system. However, the evidence also points out that not every taxpayer updates his or her beliefs in the same direction, as relevant heterogeneous effects are found across the population. The evidence in this paper advances the state of knowledge and may help to reconcile some of the different results in the literature.

JEL classifications: H26, C93, D03, H41
Keywords: Tax compliance, Evasion, Tax morale, Property tax, Randomized field experiment, Behavioral economics

1. Introduction

Tax evasion is a pervasive problem in many countries. In particular, some developing countries do not collect even half of what they would if taxpayers complied with the written letter of the law.[1] The academic literature has not been oblivious to the need to explain why people pay (or do not pay) taxes, and the striking differences in compliance across countries. Most of the literature has relied on the standard model of tax compliance developed by Allingham and Sandmo (1972), which suggests that each individual taxpayer faces a tradeoff between the monetary benefit of evading and pocketing the money, and the potential costs of being detected and having to pay the evaded tax plus a penalty. According to these models, how much each person evades is the result of the optimal decision of risk-averse individuals who maximize their expected utility. Tax evasion is supposed to decrease whenever the expected penalty (determined by the size of the penalty and the probability of getting caught) increases.

Even though this model, along with Yitzhaki (1974), has been the workhorse behind most academic research and its results have guided public policies, some of its implications do not square well with real world evidence. In particular, people tend to comply with the law more regularly than what the model would suggest given that in most countries fines and audit probabilities are rather low (Alm, McClelland, and Schulze, 1992; Andreoni, Erard, and Feinstein, 1998; Dhami and al-Nowaihi, 2007; Frey and Torgler, 2007). To explain this puzzle, two main strands of literature have developed by grounding the research in behavioral economics. One strand modifies the axioms of expected utility theory by adding risk and uncertainty to the models (Yaniv, 1999; Snow and Warren 2005; Dhami and al-Nowaihi, 2007, 2010). The other incorporates additional costs of evading to the expected utility function by assuming that the decision-making process of the individual taxpayer is also affected by social interactions (Cowell and Gordon, 1988; Gordon, 1989; Myles and Naylor, 1996; Kim, 2003; Bayer, 2006; Fortin, Lacroix, and Villeval, 2007; Eisenhauer, 2008; Dell'Anno, 2009; Traxler, 2010). In these latter models, taxpayers do weigh not only the monetary payoffs but also the moral implications of their decisions. What other taxpayers are doing and how the government uses public revenues carry much weight in tax evasion decisions (Torgler, 2003).

[1] For example, that is the case for personal income taxation and corporate income taxation in Argentina, the Dominican Republic, Ecuador and Guatemala, according to recent studies summarized in Corbacho, Fretes Cibils and Lora (2013).

Combining these two strands yields predictions more in line with the empirical evidence: most people declare more than what the standard model would suggest and not everybody engages in tax evasion (Hashimzade, Myles and Tran-Nam, 2012). In terms of their policy implications, levels of compliance seem to depend not only on actual rates but also on people's subjective beliefs about the levels of enforcement and penalties, the behavior of other taxpayers, the government's use of public monies, and on some additional intrinsic motivations. As such, it may be possible to increase tax compliance by raising the salience of enforcement and penalties, the level of compliance of neighbors, and the efficiency and/or effectiveness of government expenditures, where trust in government authorities may be a mediating factor.[2]

While the theoretical literature has advanced steadily, the empirical literature has not yet reached the same level of consensus. For example, while most studies to date find a positive result for reducing tax evasion by increasing the rates or salience of penalties and audits (e.g., Slemrod, Blumenthal, and Christian, 2001), there is still controversy about the effect of messages that appeal to moral considerations, and those reflecting the use of public monies by the government. For example, while Torgler (2003), and several laboratory and survey-based studies find confirmatory evidence (Dell'Anno 2009), Blumenthal, Christian, and Slemrod (2001), Torgler (2004), and Fellner, Sausgruber, and Traxler (2013) do not find significant effects across all types of taxpayers.

One way to evaluate the actual relevance of the different hypotheses is by using an identification strategy that reduces concerns about internal validity while keeping a relatively high level of external validity. For that purpose, we conducted an experiment designed to test the determinants of compliance with the most relevant municipal tax in a municipality of Argentina, which is equivalent to a property tax. This tax, locally known as *tasas*, is levied upon individuals according to the linear front size of the property and the services they receive from the local government, such as street lighting, trash collection, and street cleaning. This tax serves perfectly for this purpose because the tax base and compliance are perfectly observable by the authorities. Consequently, there are no reporting (or misreporting) issues, people are billed by the government at regular intervals, tax evasion can be perfectly measured, and the tax is directly linked to the provision of public goods by the same level of government that collects the tax.

[2] Some studies looking at this issue include Aguirre and Rocha (2010), Alm and Martínez-Vázquez (2007), Torgler et al. (2008), Alm and Torgler (2006), Cummings et al. (2005), Dell'Anno (2009), Frey and Torgler (2007), Scholz and Lubell (1998), Scholz and Pinney (1995), and Murphy (2004).

In the experiment, approximately 23,000 individual taxpayers of this *tasa* in the Municipality of Junín, a midsized and largely urbanized district located in the upper north of Argentina's main province, Buenos Aires, who are billed bimonthly, were randomly divided into 4 groups. Because changing tax codes and randomizing the provision of public goods tends to be difficult (if not impossible) the strategy was to affect taxpayers' beliefs with information (Lora, 2008; Coleman, 1996, 2007; Thaler and Sunstein, 2008). One of the groups received no treatment (the control group); the other three were treated by including messages in their tax bill. The treatments were designed to test the main determinants of tax compliance according to the literature: deterrence (or beliefs about enforcement and fines), equity (or beliefs about other taxpayers' behavior), and fairness (or beliefs about the use of resources by the government) (Congdon, Kling and Mullainathan, 2011; Hashimzade, Myles and Tran-Nam, 2012). This way, the experiment would provide evidence about the different hypotheses in the literature at the same time, and the effect and effectiveness of nudging taxpayers.

The results indicate that introducing messages in the tax bill is a good instrument for affecting taxpayers' behavior. Still, not all the messages seem to have the same effect. The most effective message was the one on deterrence that listed the actual fines and potential administrative and judicial steps that the municipality might follow in the case of noncompliance. More precisely, tax compliance among the taxpayers that received this deterrence message increased by almost 5 percentage points with respect to the control group (which is equivalent to reducing tax evasion by more than 10 percent).[3] We do not find any average treatment effects for the other two messages. This result, which is in line with Torgler (2004) and Fellner, Sausgruber, and Traxler (2013), contradicts the evidence in the survey-based literature.[4] Consequently, while people tend to answer that they would be willing to pay more taxes, this behavior is not necessarily corroborated in practice. Interestingly, we do find some heterogeneous effects across the population, which indicates that not everybody reacts to the messages in the same way. Own compliance behavior, the level of provision of public goods, and wealth seem to affect the incidence of the messages.

[3] Our results may be underestimating the full effect. First, even though every taxpayer should have received the message, not all of them may have read it. Second, any contamination of the control group would also bias our results downward.

[4] Finding different results between opinion-based surveys and behavior-based experiments is not unique to this case. Lacetera, Macis, and Slonim (2013) report similar results in the area of blood donations. There, while people report in surveys that they would be less likely to donate if offered a monetary reward, the field experiment evidence does not support those findings.

In addition to its policy relevance (a low-cost, high-return policy innovation), this paper contributes in several ways to the existing literature on the importance of information flows in influencing the individual taxpayer's compliance decision.[5] First, having grounded the research design on both the behavioral economics and tax evasion theoretical literature, this paper advances toward performing a more comprehensive test of the different hypotheses in existence by evaluating the three main hypotheses (equity, fairness, and deterrence) within the same experimental framework in order to ensure that differences in their effectiveness come from the policies themselves and not the context in which they are applied.

Second, it provides further evidence regarding which (if any) of the mechanisms usually identified by the theoretical literature is relevant for explaining taxpayer behaviors. As such, this paper contributes not only because of its findings but also because of what it does not find. As mentioned above, it reinforces the notion that enforcement matters while reducing the confidence in across-the-board mechanisms that stress equity and fairness considerations. If the evidence keeps mounting, these results should encourage researchers to evaluate how much to stress the use of tax morale mechanisms in the theoretical models as a way to square off the theory with the stylized facts, and to weigh appropriately the findings of the survey-based literature. For example, some theoretical papers require a high value of the parameter of tax morale to reverse Allingham and Sandmo's result on the effect of changes on tax rates on tax evasion. The existence of heterogeneous effects may also encourage researchers to develop more complex models of social interaction based on heterogeneous agents over a wider range of parameters.

Third, the experiment was carried out on a property-based tax, whereas the previous literature focused on income taxes (e.g., Slemrod, Blumenthal, and Christian, 2001; Blumenthal, Christian, and Slemrod, 2001; Kleven et al., 2011), VAT (Pomeranz, 2013), or very specific fees (Fellner, Sausgruber, and Traxler, 2013). Because the revenue of this tax is directly related to the provision of highly visible, locally provided public services, it should work better for measuring the impact of messages that target the fairness of the tax system on actual payments (Torgler, 2004, 2005). It has the added policy value of working on a tax that is the main source of revenue at the subnational level at a time when local jurisdictions are facing pressure to raise their own

[5] Some of the most salient papers in this literature are Slemrod, Blumenthal, and Christian (2001); Blumenthal, Christian, and Slemrod (2001); Pomeranz (2013); Kleven et al. (2011); and Fellner, Sausgruber and Traxler (2013).

levels of revenues following several waves of decentralization and fiscal tightening at the federal level.

Fourth, concentrating the research design on studying the decision to pay or not to pay an amount that is "exogenously" determined, as we do here, helps estimate more precisely the impact of the policy (because it reduces the possibility of changes in behavior in other markets—e.g., labor market—and increases in tax evasion that may affect some of the estimations based on declared income) and provides a direct measure of reductions in tax evasion, which changes in reported income may not capture. Moreover, this paper uses actual payment behavior, instead of willingness to pay, as an outcome variable. While survey responses could be very valuable for understanding changes in perceptions (a relevant policy experiment by itself), researchers can only reliably test the hypotheses in the theoretical models by measuring changes in actual payment behavior.

Fifth, while previous studies typically rely on mailing letters to potential evaders, we include the messages and images directly on the tax bill, eliminating some (but not all) of the concerns regarding intent-to-treat (ITT) bias (there will still be some taxpayers, particularly among the group of tax evaders, who do not even read the bill).[6]

Sixth, instead of sending the messages to a selected sample of known evaders, which may lead to problems of sample selection and external validity, we randomized the treatments over the entire population of compliant and noncompliant taxpayers. Including every type of taxpayer may have the added bonus in terms of policy implications of not only helping generate positive incentives for those who failed to pay in the past but also to maintain compliance of the formerly "good" taxpayers. Additionally, uncovering heterogeneous effects across the type of taxpayers helps to guide policy design, particularly in the case of policies that would be aimed at the universe of taxpayers.

Finally, focusing on a developing country helps validate the results in the literature in a region, Latin America, where tax evasion is rampant. Therefore, understanding the relative effects of different policies is more relevant. Moreover, this is a region where trust in government is lower than in the developed world, which again, could further our understanding of the effect of messages in such an environment. As a side product, we are also providing an

[6] In a survey conducted by the authors on a sample of representative taxpayers from another municipality, 82 percent answered that they had read at least some parts of the bill. This percentage drops to 56 percent when taxpayers were asked about whether they had read any other accompanying material sent by the municipality.

exact measure of tax evasion on a property-like tax for an average size municipality in the region.

The rest of the paper is organized as follows. Section 2 reviews the literature on the determinants of tax compliance and discusses current approaches on how to increase compliance based on the standard tax evasion model and recent developments in behavioral economics related to the effects of tax morale. Section 3 describes the field experiment and our main results as well as some additional empirical analyses. Section 4 presents our main findings. Section 5 concludes and discusses some potential avenues for future research.

2. Why Do People Pay Taxes?

2.1 Literature Review

As already mentioned, the standard model of tax compliance (Allingham and Sandmo, 1972, henceforth AS; Yitzhaki, 1974, henceforth Y] suggests that taxpayers face a tradeoff between the monetary benefit of evading and pocketing the money, and the potential costs of being detected and having to pay the evaded tax plus a penalty. Tax evasion levels are the optimal decision of risk-averse individuals that maximize their expected utility. Given the assumptions of this model, tax evasion decreases whenever the expected penalty (determined by the amount of the penalty and the probability of getting caught) increases.[7]

One way the literature has found to explain why people comply more than what the actual rates of enforcement and penalties would suggest has been to add a term to the standard formulation that captures the additional individual (psychic, moral, social) costs that the taxpayer faces when evading (Gordon, 1989; Myles and Naylor, 1996; Kim, 2003; Fortin, Lacroix and Villeval, 2007; Traxler, 2010). The parameters included in this additional term usually depend on the taxpayer's beliefs regarding the fairness and equity of the system. Then, following Traxler (2010), the optimization problem can be formulated as follows

$$U(e_i, \theta_i) = E[u(x(e_i))] - \theta_i e_i c(n)$$

where *e*, the share of tax being evaded, is the variable of choice of the individual, the first term is the traditional expected utility term coming from the AS and Y models, and the second is a term

[7] For space limitations, and because these are very well known models in the economics profession, we don't reiterate their formulation here. For comprehensive overviews, see Traxler (2010) and Hashimzade, Myles, and Tran-Nam (2012), among others.

that captures the moral cost of tax evasion.[8] In this formulation, the "moral" cost depends on how much each individual internalizes the social cost [θ_i], which may depend on how much each individual evaluates the degree of fairness of the tax system, and a function *c(n)* that captures the strength of the norm for a given share of evaders *n* (or the equity of the system).[9]

The policy implications of this setting are obvious. Tax evasion drops as penalties and enforcement increase (as in the traditional model), and it also decreases as θ_i (fairness) increases and *n* decreases—or equity increases (Traxler, 2010: 92–3). Overall, the empirical evidence (in particular survey and cross-section studies) tends to support these results. People tend to comply more if they believe that others comply as well, and if they believe that the government makes good use of the money it collects. According to survey evidence, a significant correlation exists between tax morale and the size of the shadow economy (Alm and Torgler, 2006) and individuals who have heard about tax avoidance have significantly lower tax morale than others (Torgler, 2005). It has also been found that a higher sense of obedience leads to higher compliance, so that if an individual believes that others comply with the tax code then their tax evasion will diminish (Scholz and Pinney, 1995; Scholz and Lubell, 1998; Torgler et al., 2008).[10] Laboratory experiments and surveys have also revealed a positive correlation between taxpayers' priors about the tax code fairness and tax compliance (Rawlings, 2004; Cummings et al., 2005). In connection to the behavior of public officials, a positive correlation exists between tax morale and the belief that the government will spend the money wisely. For example, Barone and Mocetti (2009) find that that the attitude towards paying taxes is more positive when resources are spent more efficiently.[11]

Factors affecting tax morale can explain differences in tax compliance across countries or regions with similar tax systems. For example, Bergman and Nevarez (2006) rely on differences in tax morale to explain the success of Chile and the failure of Argentina in collecting taxes over

[8] We use *E(u)* because it is the most popular representation of the decision problem, but this first term could be modeled instead using any non-expected utility formulations. For example, a prospect theory model in which probabilities are weighted, and the value functions are convex in losses and concave in gains, would also work.

[9] As mentioned, this is one of the many ways to model the moral costs. See Hashimzade, Myles, and Tran-Nam (2012) for a survey of alternative models. Dell'Anno (2009) presents a very detailed and schematic table summarizing the literature on tax morale and compliance.

[10] Relatedly, Traxler and Winter (2012) find that the more commonly a norm violation is believed to occur, the lower the individual's inclination to punish it.

[11] It is important to note that this does not capture trust in government in general, but the individual's opinion about government's spending behavior (Aguirre and Rocha, 2010; Alm and Martínez-Vázquez, 2007; Murphy, 2004).

the 1990s. Recent empirical evidence also shows that there is significantly lower tax morale in South America and Mexico than in Central America and the Caribbean (Torgler, 2005).

Evidence compiled at the Inter-American Development Bank (IDB), using data from the Americas Barometer for a selected group of countries for which data are available, show similar results (Scartascini, 2011).[12] Basically, people who are more satisfied with the provision of public services at the local level, who trust the municipality more, who have not been exposed to corruption, and who consider the level of interpersonal trust to be higher tend to answer that they would be more willing to pay higher taxes to the municipality. Similar evidence has been found by Ortega, Ronconi, and Sanguinetti (2013) based on household survey data for 17 Latin American cities. In their study, the net share of people responding that they would be willing to pay more taxes if the government improved their performance is positive and significant in most cities and for most measures of better performance. Moreover, they find that reading a brief piece of factual information generated significant changes in perception in some, but not all, of the cities in the study. In one of them they find evidence of reciprocity of a fairly sizable magnitude.

2.2 Existing Experimental Evidence

As Ortega, Ronconi and Sanguinetti (2013) show, in order to change people's perceptions it may not be necessary to change actual policies. The way in which people form their beliefs depends on the information they receive and the way it is presented, and their underlying system of beliefs (Lora, 2008; Thaler and Sunstein, 2008). Coleman (1996, 2007) conducted a randomized, controlled trial in Minnesota using messages to affect tax compliance. The State Revenue Service sent a letter to 20,000 taxpayers with a message designed to correct the erroneous perception of many taxpayers that cheating on taxes was common. The impact of the message was significant: the mean increase in federal declared taxable income for the group receiving the letter was $2,390. Wenzel (2002, 2007) found similar results in Australia.

Slemrod, Blumenthal, and Christian (2001) studied the impact of messages sent to 1,724 randomly selected taxpayers in Minnesota as well. The message informed taxpayers that the returns they were about to file would be closely examined. The result was that those in the treatment group (individuals who received the letter) increased their average tax payments

[12] Survey waves 2008 and 2010 for Colombia, Dominican Republic, Guatemala, Honduras, Peru and Uruguay.

compared to previous years. In particular, the effect was stronger for the self-employed, who may have more opportunity to evade. Blumenthal, Christian, and Slemrod (2001) do not find significant average effects when the messages present normative appeals instead.

Torgler (2003) conducted a lab experiment in a small village of Costa Rica where only 37 volunteers—actual taxpayers—participated. The experiment consisted of three types of interventions in order to study tax compliance. The first was related to public goods provision financed from the taxes paid, the second was a moral suasion, and the third was a positive reward for tax compliance. Results show that the three interventions had a positive effect on tax compliance. The moral suasion message, even though large and positive for explaining timelines of payments, was not statistically significant when utilized in a randomized controlled trial with 580 taxpayers in a small town in Switzerland (Torgler, 2004).

In the same vein, Kleven et al. (2011) conducted a randomized field experiment over a representative sample of 40,000 taxpayers in Denmark. In the first stage, half of the population was randomly selected to be audited, while the rest was deliberately not audited. In the second stage, they sent threat-of-audit letters to a random sample in the two groups. Both the audits and the letters had a significant positive effect on self-reported income. Similar results were found by Pomeranz (2013), who finds that random audit announcements are transmitted up the VAT chain and increase compliance by firms' suppliers.

Finally, Fellner, Sausgruber and Traxler (2013) carried out a large field experiment with potential evaders of TV license fees in Austria, sending different messages to taxpayers. They find a strong effect on compliance, particularly for a treatment aimed at raising the salience of detection risks. Neither appealing to morals nor providing information about other taxpayers' compliance behavior had any significant effect on compliance.

In spite of the growing number of articles on the topic, this is still a nascent literature and more work is warranted. As was already mentioned in the introduction, first, while some results seem to produce robust findings, such as the positive effect of the threat to audit on compliance, there is still no definitive answer about some of the other potential determinants.[13] Second, while most studies have focused on income taxes, few have dealt with taxes in which the tax base is

[13] For example, while some papers find positive effects for moral suasion treatments (Torgler, 2003), others do not (Fellner, Sausgruber and Traxler, 2013). Similar results are noted with the fairness treatments: Torgler (2003) finds positive effects, as do Ortega, Ronconi and Sanguinetti (2013), but only under some restrictive conditions (which makes the results significant for the group in only 1 out of 17 cities in which they performed the survey experiment).

perfectly known and tax evasion can be perfectly monitored, and where taxes are directly linked to the provision of a public good. Third, most studies have worked with selected samples of taxpayers instead of the overall population, which raises issues of sample selection and external validity (particularly the value of implementing the policy recommendations over the entire universe of taxpayers.) Finally, most of the studies have been written for developed countries, where tax evasion is low, enforcement is a regular activity of the tax administration, and trust in government is high. Pursuing these studies in developing countries would test the significance of the hypotheses in contexts where tax evasion is rampant, governments have fewer resources for enforcing laws, and trust in institutions is lower.

3. The Experiment

With the objective of raising compliance with the tax and evaluating the effectiveness of sending different messages to taxpayers, the Municipality of Junín agreed to redesign the property tax bill include different messages in it in a randomized experimental setting.

3.1 Background on the CVP in Junín

The "Public Space Conservation" tax (*Tasa de Conservación de la Via Pública*, or CVP henceforth) is a tax levied on real estate.[14] Taxed property includes homes, farms, business premises, and most other real estate. The tax is computed by taking into account the linear frontage, in meters, of the taxed real estate, the number of streetlights around the property, and the type of trash collection and street cleaning services it receives.[15] Property owners are billed every two months. Taxpayers have approximately 10 days to pay from the moment they receive the bill to the first due date. In case of payment delays, a cumulative compound monthly interest rate of 2 percent is applied to the outstanding liabilities.

By August 2011, there were around 26,000 individual taxpayers registered to pay the CVP, equivalent to a third of the population of Junín, according to the last 2010 Argentine

[14] A relevant aspect about having performed this experiment in Argentina instead of other developing countries is that land and property titling is highly developed so there are no controversies regarding ownership. Additionally, the universe of individuals is billed directly, as there are no bank-held escrow accounts as is the case in the United States.

[15] While there is available information on the fiscal valuation of the majority of taxed real estate for 2009, the data are not utilized for the computation of CVP liabilities, as local authorities consider them unreliable and outdated. Hence, we do not use this information in our baseline estimations.

census.[16] While the tax is levied every two months, the Municipality also allows taxpayers to pay on a yearly or monthly basis. However, only around 12 percent of taxpayers select either of these options. The large majority of the taxpayers (around 80 percent) pay their monies personally at the Municipality or other offices of the local government instead of using direct deposit or other automatic payment modalities.[17] Most taxpayers own one relatively small taxed property, and only 20 percent of taxpayers own more than one registered taxed property. Also, 2.7 percent of registered taxpayers have postal addresses outside the Municipality of Junín.[18]

Over the last four years, tax compliance, defined as total tax payments over tax liabilities, hovered around 40 percent on average in each bimonthly billing cycle. Compliance declined from 2010 onwards, only to show a gradual recovery in 2011. Moreover, payments display a marked seasonal behavior with larger payments made in January and February (bimester 1), mainly as a result of taxpayers paying in advance some of their ensuing liabilities for the remainder of the year (see Figure 1).

Junín is divided into 15 districts and 10 sections (see maps in Appendix 1). Districts are larger and generally encompass more than one section. Tax compliance, public services' provision as well as the number and size of taxed real estate vary widely across districts as a reflection of heterogeneous characteristics of the underlying taxpayer population. As the capital city of the municipality, Junín is the largest district in terms of the number of taxpayers and taxed properties as well as the amount of taxes collected. Within the city of Junín itself, there is also much heterogeneity in taxpayers' observable characteristics across sections. While the remaining districts are largely rural, they also include some small cities. As can be observed in Appendix 1, Table 1, Junín is quite similar to (although slightly richer than) the average municipality in the Province of Buenos Aires (the most populated province in Argentina).[19]

The units of analysis are individual taxpayers. We excluded private companies and social organizations from the sample because for those taxpayers somebody other than the person receiving the treatment may make the payment decision, which could bias the results.[20] Among the individual taxpayers of the CVP in Junín, the entire universe was included in the randomized

[16] http://www.censo2010.indec.gov.ar/definitivos_bajarArchivo.asp?idc=93&arch=x
[17] Unfortunately, the municipality does not collect this data at the individual level.
[18] It includes, mostly, people with investment properties or second homes in the city.
[19] This experiment was part of a series of experiments performed in different cities in the Province. As such, there is nothing particularly unique to Junín that would make us believe that external validity might be affected.
[20] For example, the bill may be received by a clerk who had been instructed in advance by the firm's owner whether to pay the bill or not. The decision then would have been unaffected by the treatment.

field experiment with the exception of 3,000 individuals who had previously paid their tax liabilities in advance and therefore were not billed during the period the experiment took place. For each taxpayer, we have collected the administrative information on tax liabilities, tax arrears, the number and size of taxed real estate (e.g., average linear front meters) and some public goods provided by the Municipality (e.g., streetlights, garbage collection, and street cleaning services). The definition of each one of these variables is provided in Appendix 2, Table 1.

For selecting the taxpayers into each treatment, we followed a stratified or bloc randomization strategy due to the presence of highly correlated taxpayers' observable characteristics at the section level.[21] Therefore, we use the geographical location of each taxpayer to define the strata or blocs. In the case of taxpayers listed with a mailing address from outside the Municipality, we created an additional artificial bloc as they may show highly idiosyncratic unobservable characteristics. Further, taxpayers registering multiple properties but lacking a unique mailing address were assigned to the section in which the property with the highest tax liability prior to the experiment was located. We also found that 4 percent of the taxpayers had multiple fiscal identification (ID) numbers but the same exact name, surname and mailing address. Despite consulting with the Municipality, it was not possible to establish the cause of this situation; therefore, we decided to treat these "duplicated" taxpayers as single taxable units, to reduce potential assignment errors. Hence, a fictitious unique identification number was created and utilized in the randomization procedures for those taxpayers

Within each bloc, for each taxpayer randomly assigned to a treatment a taxpayer was assigned to the control group. Thus, because we have three treatments, 60 percent of taxpayers were randomly assigned to the control group (about 15,000), and the remainder was equally distributed to each of the treatment groups within each strata (about 2,500 each). Even though this decision may have lowered statistical power for each treatment, and it may work against finding statistically significant results, it would also reduce the possibility of cross-treatment contamination (which could have affected the ability of finding different results across treatments). Contamination of the control group instead would render the estimates as a lower bound. This decision of "undertreating" was also in line with the municipal government's preferences.

[21] For instance, the calculation of a simple measure of intra-section correlation yields a coefficient of 0.5 for trash collection and other locally provided public services, 0.25 for the number of streetlights and 0.2 for the average tax liabilities and the number of unrecoverable debtors.

In order to ensure the right balance across groups, we ran the randomization 1,000 times and selected the random draw that showed the best balance for all the pre-experimental covariates (including the pre-experimental outcome variable) controlling for the strata dummies. The final draw was selected according to two criteria: i) the minimum statistical difference between treatment and control for draws statistically significant at 5 percent or lower and ii) the minimum maximum t-stat (Bruhn and McKenzie, 2009).

After the randomization and random assignment of taxpayers, we also carried out a comprehensive set of actions aimed at minimizing potential administrative errors during implementation that could affect the randomized design of the experiment. First, we sent to the local authorities, three weeks before they started to print the tax bills, a file with a code assigning each taxpayer to either the treatment groups or the control. Based on that codification, the Municipality generated an administrative record that we compared against our files in order to detect and correct any mistakes with the randomization. Second, we also conducted, one week prior to distribution of the tax bills, a physical check with a random sample of 1,000 printed bills at the Municipality in Junín. No assignment errors were found in this final control.

The tax bills containing the treatment messages were sent out in August 2011, corresponding to payments due in the September-October (Bim 5) billing period of the same year. That period might be considered "normal" as there are no pronounced seasonal variations in tax payments such as occurs in the January-February (Bim 1) billing period, when a large number of tax payments is made in advance for the rest of the year.

3.2 The Treatments

The wording of the messages and the selection of the images was prepared by a communications team in coordination with the municipal authorities and tested in a small focus group. In every case, and following standard recommendations in the literature, the objective was to provide the combination of words and pictures that could have the maximum impact while conveying the message that was intended.

3.2.1 Deterrence

The first treatment included a message in the tax bill that provided taxpayers with a simplified example of the cost of noncompliance by computing how much the cost of a hypothetical debt of AR$ 1,000 would be after a year (given a cumulative monthly interest rate of 2 percent).

Specifically, the message stated that, for a liability of such amount, the taxpayer would have to pay AR$ 269 in arrears after that period of time. The message also warned taxpayers that the Municipality would take administrative and legal steps in case of noncompliance. An image of a gavel was also included in the tax bill with the intention of reinforcing the deterrence message. Table 1 provides the translated text of the messages, and an example of a tax bill is included (in Spanish) in Appendix 3.

The aim of the message was twofold: i) to raise the taxpayers' subjective perception of the probability of receiving a fine and other possible administrative or legal penalties in the case of noncompliance[22] and ii) to reduce the likely computational costs derived from the calculation of arrears on unpaid tax liabilities using a compounded interest rate. The literature on tax compliance that incorporates the insights of behavioral economics points to the importance of limited computational capabilities, particularly in the case of relatively complex intertemporal tax calculations for explaining taxpayers' responses to fines and other penalties (Congdon, Kling and Mullainathan, 2011). This literature also suggests that, in the presence of taxpayers with limited attention, raising the salience of fines and legal actions in the case of noncompliance might affect individual behavior with respect to tax payments (Bernheim and Rangel, 2007, 2009).

3.2.2 Fairness

The second treatment introduced a message in the bill with information about the actual use of revenues by the Municipality by showcasing investment works. It highlights the number of streetlights, and the number of water and sewerage connections installed by the local government of Junín in the previous six months. The message was also accompanied by an image of an easily recognizable "men at work" traffic signal (Table 1 and sample tax bill in Appendix 3). This message was intended to modify taxpayers' perceptions about the fairness of the tax by influencing their priors about the quantity and quality of public services provided by the Municipality (Cowell and Gordon, 1988).

[22] It is important to notice that the information on the interests and penalties was not displayed in the bill before.

3.2.3 Equity

The third treatment introduced a message in the tax bill about the levels of CVP compliance. The message asked whether the taxpayer was aware that only three out of 10 taxpayers did not pay their tax liabilities.[23] It also added a sentence questioning whether the taxpayer was currently paying her liabilities ("What about you?"), which attempted to capture the essence of the moral suasion arguments. In order to reinforce the message, the bill also contained an image with seven larger figures, personifying the taxpayers who have paid over time, and three smaller ones, representing the people who have never paid (Table 1 and sample tax bill in Appendix 3). The goal of the message was to influence taxpayers' perceptions of the extent of compliance in the local community (as suggested by Dell'Anno, 2009).

We are aware that even though extreme care has been taken in selecting the factual information and wording so the messages would tend to encourage people to update their beliefs upward (and reduce evasion), it is still possible that some people may have updated them in the opposite direction. For this reason, in the empirical work, we have taken special care to look into potential heterogeneous effects. Of course, those who usually do not pay may also be skeptical of government messages, and they may not update their beliefs at all regardless of the message they receive. We try to measure the existence of this "trust effect" as well.

3.3 The Dependent Variables

We have defined the outcome variable in several alternative ways (but always dichotomously) in order to ensure we capture changes in behavior as precisely as possible. The main outcome variable, *paid*, takes the value 1 only if the taxpayer has paid in full the total tax liabilities for the period of the experiment. Two variables record the timeliness of the payment: *paid_by1D* takes the value of 1 if the payment took place before the first due date; *paid_by2D* takes the value of 1 if the payment took place before the second due date. Because the taxpayer may decide to pay in addition some arrears and reduce his/her debt or to pay future liabilities in advance, we created the variable *overpaid*, which takes the value 1 if the taxpayer has paid not only her liabilities for the period in full but she has also made some additional payments. For taxpayers with multiple

[23] The message corresponds with the fact that 30 percent of the taxpayers are considered "unrecoverable debtors" (have basically never paid their tax bill). The other 70 percent have paid with some recurrence even if they had not done so in the last bimester before the experiment.

properties, all these variables take value 1 if the taxpayer paid all of the bills in full.[24] Finally, the variable *addpayments* takes a value 1 when the taxpayer has made advanced payments or paid down part of her debt, regardless of whether they have paid their current liabilities in full.

Table 2 presents the summary statistics for the three treatment groups, compared to the control group in the pre-experimental Bim 3 billing period. As expected, given the random assignment, average observable characteristics for each one of the groups is very similar. Most importantly, there are no statistically significant differences among the groups in terms of the dependent variables. Table 2 indicates that prior to the experiment, around 21 percent of taxpayers paid before the first due date. This number increased to 33 percent before the second due date. By the end of the bimester, 40 percent of taxpayers had paid the liabilities for the period in full. Moreover, 3 percent of those taxpayers had made additional payments during the period, such as paying down their debt. In total, approximately 4.5 percent had made some kind of additional payments.

How do these percentages translate into money? Basically, while the average liability for the CVP amounts to AR$ 122, the mean tax payment is equivalent to AR$ 56. On average, the typical individual taxpayer owns more than one property of 16 linear front meters, has regular trash collection, and has almost 3 street lights surrounding her property.[25]

3.4 Empirical Strategy

To estimate the causal effects of the treatment messages on tax compliance, we employ a probit model. Formally, we estimate the following specification:

$$Prob(Y_i = 1|X) = \Phi\left(\alpha + \beta_1 T_{1i} + \beta_2 T_{2i} + \beta_3 T_{3i} + \gamma Z_i + \delta_{is}\right) \quad (1)$$

where Y is the binary outcome variable equal to one if the individual taxpayer i meets her tax obligations in the bimonthly billing period 5 (September-October) according to any of the definitions provided before; T are binary variables representing the three treatment messages (T_1=Deterrence; T_2=Fairness; and T_3=Equity), Z is a vector of control variables comprising

[24] As additional robustness checks we have also used different cutoffs, such as having paid 50, 60 or 75 percent of the taxed liabilities without finding any significant changes in the results. Even though taxpayers cannot make partial payments, taxpayers with multiple properties could still decide how many bills to pay. This way, we could check the effect of the messages on the extensive (paying or not) and intensive margins (how much to pay). We later discuss some of the results for the 50 percent cutoff as an example.

[25] We also include in Table 2 the balance of randomization for the public services variables in the period in which we conducted the experiment to show that no public works affected the groups differentially.

taxpayers' observable characteristics (e.g., income proxies, payment frequency and compliance status) and the amount and type of public services provided by the municipality (e.g., number of streetlights and trash collection and street cleaning services), and δ is a set of strata fixed effects. Following Duflo, Glennerster and Kremer (2008) and Angrist and Pischke (2009), we include the lagged outcome variable as an additional control to avoid potential serial correlation concerns, mainly because there is high persistence in payment behavior.

4. Results

Table 3 presents the average treatment effects (ATE) of the probit estimation described in equation (1), showing marginal effects computed at the mean.[26] Baseline estimations include the three treatment messages, the lagged outcome variable (because compliance shows great persistence), and bloc fixed effects. The estimations with controls also include the variables for public service provision (trash collection and street lighting services during the period), the number of properties owned by each taxpayer, the average linear front size of the properties, and a dummy that controls for those taxpayers who elected to pay monthly.[27]

The result that clearly emerges from the estimations is that the deterrence message had a positive, statistically and economically significant effect on compliance while the other two messages had no significant average effect. The size of the effect differs according to the dependent variable used. As expected, the size of the effect is larger for our main variable of interest, *paid*, that considers compliance with the tax regardless of the date on which payments were made. In this case, the message increased compliance by almost 5 percentage points, which represents an increase in compliance rates of approximately 12 percent.[28]

Results are smaller in magnitude for the other dependent variables but no less interesting in terms of the policy implications. The message increased compliance before the first due date by almost 2 percentage points, and compliance before the second due date by 3 percentage points. Moreover, it not only had an effect on the compliance for current liabilities, but taxpayers

[26] Full regression tables are provided in Appendix 4

[27] Public services are included contemporaneously instead of lagged to control for any effect on payments that could have been caused by an increase or decrease of public services during the period of the experiment. Results are identical if we include these variables lagged instead. Also, results are basically identical if we run the regressions without including fixed effects as shown in the tables in Appendix 4.

[28] As shown in Table 6 of Appendix 4, coefficients are slightly larger for the variable that computes compliance according to whether the taxpayers has paid at least 50 percent of her tax liabilities (*paid_50p*). This result indicates that the message may have an impact on both the intensive and extensive margins.

who received this message were also more likely to pay arrears and future liabilities in advance. These results confirm one of the hypotheses coming from the theoretical literature. Increasing the salience of penalties affects compliance behavior.

Results remain statistically identical when we include the additional controls, and these variables display the expected signs (as shown in Appendix 3). In addition to persistence in payment behavior, those who paid in the past are more likely to pay now, as are people who receive more public services from the municipality. As suggested by recent theoretical work, the perception of an adequate provision of public goods with respect to tax payments or "fairness" might provide an incentive to comply with the tax code (Hashimzade, Myles and Tran-Nam, 2010). Still, this is evidence of a correlation and not necessarily of causation.

Taxpayers who pay monthly are also more likely to comply. This may be explained by selection bias because people have opted for this type of billing. Hence, they may be more aware of their responsibilities with the municipality. On the other hand, taxpayers with multiple properties and/or larger properties tend to display lower compliance. One explanation may be that wealthier individuals are more likely to engage in riskier activities (Sandmo, 2005). Of course, it may also be that people with multiple properties may be more likely to misplace some of the bills, or have rented properties (and rely on tenants for the payment).

So far, we have described the results for the marginal effects at the mean level. We have also checked for whether the size of the effect varies at different points in the distribution of the covariates; differences tend be very small. By way of example, and always using *paid* as the dependent variable, the effect of the deterrence message tends to increase as the provision of trash collection improves (from 0.042 for those with no collection to 0.049 for those with the best service).

While the results for the deterrence message are large, we find no average behavioral changes for people who received either of the other two messages. There are several potential explanations for this non-result. First, invoking moral reasons, despite evidence in the literature that it may switch perceptions at the margin (as Ortega, Ronconi and Sanguinetti, 2013, have found), may not affect payment behavior because the perceptions elasticity for changing tax

compliance is low (Blumenthal, Christian and Slemrod, 2001). This is not uncommon. For example, Fryer (2013) finds the same in an experiment in the education sector.[29]

Second, people who do not comply may have lower trust in the government, which may translate into lower effectiveness of the messages (i.e., those who do not trust the government may not trust the messages they receive about the levels of tax evasion and public works).

Finally, the average effects may be masking differences among individuals. For example, while we expected that people would tend to evaluate information about actual use of money by the local government in positive terms, people who had priors about government works that were "too high" may have revised them downward instead of upward. A similar adjustment may have occurred with the rate of compliance of neighbors.

We have checked for heterogeneous effects across the observable characteristics that are deemed reasonably likely to affect compliance behavior variably across taxpayers.[30] First, we may suspect that people evaluate the fairness message (provision of public goods) differently according to their own experience with the municipality's provision of public services (because heuristics). On the one hand, those who receive poor (good) services may update their beliefs upward (downward) when they are informed about public works the municipality has engaged in recently (for simplicity, this can be called the *information effect*). On the other hand, those who usually receive better services may provide a higher weight to the information provided by the municipality than those who have not had good experiences dealing with the municipality so far; the latter may disregard the messages altogether (*trust effect*).

If we look at each public service (street lights, trash and garbage collection) separately, we find no differential effect for the treatments either across different levels of public service provision by the municipality at the individual level or by including a variable that measures the difference between the services they receive compared to the median public services provided in their strata. However, we do find different effects when we use a variable that summarizes overall public services provision (*public services*). As can be observed in Figure 2b, the *information effect* seems to play a relevant role in people's decisions: the fairness message seems to have a positive effect on those who receive a lower quantity and/or quality of public services

[29] A moral suasion message changed reported student's beliefs about the relationship between education and outcomes. They also reported being more focused and working harder. However, there were no measurable changes in attendance, behavioral incidents, or test scores.

[30] Appendix 6 presents regression results for the models with interactions. Figures 2 to 5 summarize the marginal effects of the interactions.

(upward revision of beliefs following the message) and a negative effect on those who receive better services. Complementing the previous results, Figure 2a may indicate that there is not much of a *trust effect* at work, as taxpayers who receive lower public services reacted more to the deterrence message than people at the other end of the distribution.[31]

A similar picture emerges if we use property size instead of public good provision (Figure 3). Taxpayers with smaller properties (and presumably lower levels of wealth) respond more positively to the messages than people with larger properties. The reasons behind this result are multiple. First, people with higher income may be less risk averse. As such, those at higher income levels may be less affected by the deterrence messages. Second, people with higher income—which is highly correlated with education in Latin America—may also be more savvy in several dimensions; for instance, they may estimate actual enforcement levels and play the system in their favor (e.g., use the tax system as a source of financing when penalties are lower than inflation rates). Third, because taxpayers at different levels of wealth have different reference points, the amounts mentioned in the deterrence message and the public works stated in the fairness message may resonate differently at different levels of wealth and tax liabilities. Results do not change according to the distribution of properties (having one or more properties).

Regarding the equity message, we may suspect that people's beliefs are highly correlated to past payment behavior. Those who pay regularly may estimate tax evasion in the community to be lower than those who usually do not do so (again, because of heuristics). Past payment behavior would naturally also affect the deterrence message directly. Only those who do not pay would change their behavior after learning or updating their beliefs regarding enforcement. As demonstrated in Figure 4, which summarizes the marginal effects for the interactions between the treatments and the lagged outcome variable, the deterrence message (T1) has a positive impact on those taxpayers who did not pay in the previous period (lagged dependent variable equal to zero) but it has no statistically significant effect for those who did. The opposite occurs regarding the equity message (T3). While the effect of the message is not statistically significant for those who had not complied before, it is negative for those who had. That is, the message seems to have been a disincentive for those who had complied in the past (and may have been overstating other people's rates of compliance.)

[31] We find no statistically significant results for the equity message.

The same results are observed if we use, instead of the lagged outcome variable, a variable that classifies taxpayers according to whether they have debts with the municipality or not. Again, as shown in Figure 5, the deterrence message (T1) seems to affect only those with debts while it serves as a disincentive to those with no debt. The equity message (T3) works in the opposite direction. Learning about the actual levels of evasion does not greatly change the behavior of those with debt, but it produce a disincentive effect on those who have paid regularly in the past.[32]

A final check on heterogeneous effects and formation of beliefs has been to run the regressions separately for the group of taxpayers who live outside the city, given that they may hold different priors than the rest of the population. Interestingly, as a first approximation to the potential set of priors of this group of taxpayers, compliance is 25 percent lower than average compliance in the city. The results, presented in Table 4, show that both the deterrence and the fairness messages tend to be positive (and larger than the results for the overall population). This would tend to indicate that people outside the city react more to the information about public services than people in the city, who are direct witnesses of the effort by the Municipality.

So far, the analysis has assumed that all taxpayers were treated. However, a reasonable potential concern with the results is that some of the taxpayers may not have read the messages, and therefore were not treated, introducing a downward bias in our estimates. In particular, there is a group, the "unrecoverable debtors," defined as taxpayers who did not register any payments on their CVP liabilities between 2007 and 2011, who are more likely to ignore the messages included in the bill.[33] To investigate this potential concern, we estimate the same empirical model as before but exclude the taxpayers who were unrecoverable debtors by Bim 3, 2011 from the sample (results in Table 1 in Appendix 5). The estimations show a slightly larger effect of the deterrence message on tax compliance for some of the dependent variables, suggesting that we could be in fact underestimating its impact due to the presence of a potential intended-to-treat (ITT) downward bias (Duflo, Glennerster and Kremer, 2008; Angrist and Pischke, 2009). As shown in the same appendix (Table 2), the change in the magnitude of the deterrence message

[32] In both cases, the fact that we find no statistically significant results for the fairness message works as a placebo test.

[33] We have tried with other proxies too, such as identifying potential "untreated" subjects according to different number of unpaid bills. The results show a nonlinear relationship. As the definition of "unrecoverable debtor" is broadened, more potentially untreated taxpayers are excluded—which should increase point estimates—and fewer noncompliant taxpayers who may be affected by the treatment are included in the sample, which reduces the size of the coefficients.

coefficient is not driven by a potential bias in the underlining distribution of taxpayers amongst treatments and control groups. The balance between the treatment messages and the control group remains virtually unchanged when unrecoverable debtors are excluded from the sample.

To provide some intuition for the findings in terms of revenues collected instead of individual-level decisions, Figure 6 plots the tax compliance ratio—computed as tax payments over tax liabilities—for the three treatment groups and the control group in Bim 5. It also displays the tax compliance ratio for the period when randomization was conducted (Bim 3, 2011) as a benchmark as well as for the same billing period but one year earlier (Bim 5, 2010). This figure suggests that tax compliance for taxpayers that received the deterrence message was 3 percentage points higher than for taxpayers in the control group (no message). This difference is statistically significant according to a nonparametric test of proportions. Notice that tax compliance increases for both the treatment and the control groups with respect to the previous billing period and the same period a year earlier. A potential explanation is that the Municipal authorities' introduction of a simplified tax bill with an improved design in the same bimonthly billing period in which the experiment was conducted provided an incentive for this generalized increase in tax compliance across the treatment groups and the control.[34] It is noteworthy that this effect does not affect the validity of our results, as the same redesigned bill was distributed to every taxpayer.

One relevant and final research question would be whether the results are persistent over time or not. Unfortunately, we could not evaluate the persistence because the Municipality launched a moratorium (or tax amnesty) during the following bimester. These types of plans facilitate the payment of arrears and reduce outstanding liabilities (in particular, they forgive some of the interests and penalties). As such, payments in the bimester could have been affected by the plan announced by the local government and rendered our treatments ineffective.[35] Additionally, as mentioned previously, many people who received the messages decided to pay

[34] It may also be the case (even though we have no evidence) that some of the people in the control group may have learned about the existence of the deterrence (or the other) messages from some of their neighbors; which may have ended up affecting our estimates downward. Therefore, we cannot rule out that the increase in compliance in the control group was affected by enforcement spillovers. Rincke and Traxler (2011) find sizable spillover effects of actual enforcement—one additional household compliance for every three additional units of enforcement.

[35] We could not check either whether the treatments affected the acceptance of the moratorium because of the way the Municipality collected the data.

some of their future liabilities in advance, which could bias any analysis because the people who remained in the sample were self-selected.[36]

Summarizing, the evidence indicates that increasing the salience of enforcement has a positive effect on compliance. Messages that appeal to equity and fairness do not necessarily show the same average effects. Still, information about public goods provision and the compliance behavior of other taxpayers seems to matter for some people. Unfortunately, under certain conditions information may discourage compliance.

5. Conclusions and Further Research Directions

This paper explores empirically whether providing information to taxpayers influences or not the individual taxpayer's compliance decision. Specifically, we conducted a large randomized field experiment to test whether including messages in the tax bill about the levels of fairness, equity and enforcement of the tax would modify taxpayers' behavior by affecting their beliefs about the use of public monies by the municipality, the prevailing level of compliance, and the severity of enforcement. This way, we attempted to provide evidence on the relative importance of these complementary policy instruments.

For conducting the experiment, we randomized (within 25 geographical blocs) the entire universe of individual taxpayers of a property-based tax of a municipality in Argentina into four groups. Extending the experiment to the universe reduces problems of sample selection and increases the external validity of the experiment. Conducting it in an average city of a developing country complements existing studies by testing the hypotheses in a context in which tax enforcement and trust in governments is lower, and tax evasion is rampant. Using a local property-based tax allows for testing hypotheses about fairness more directly than using direct taxes collected by the federal government. It also provides a more direct measure of the impact of tax evasion than previous studies.

The results indicate that the marginal probability or impact effect on tax payments for the group of taxpayers who received a deterrence message was on average almost 5 percentage points higher than for the control group of taxpayers who did not receive any message. To put these findings in context, extending the use of this message to the entire population of taxpayers

[36] More than 700 people made advanced payments in the period. This is more than 30 percent higher than in the pre-experimental bimester.

may have resulted in a sizeable increase in compliance of more than 10 percent. Moreover, the empirical results may understate the actual impact of such a policy, as some people who have not been paying the tax may not have read the message. Additionally, people who have not paid for a long time have little incentive to change their behavior in a particular billing period as it does not erase their record. Combining these policies with some payment plans for the debt stock could have an important effect on the size of the effect.

We find no average treatment results for the messages that were designed to affect beliefs on the equity and fairness of the taxes. The reasons behind this are multiple. First, even if messages affect perceptions, these may not translate necessarily into changes in behavior. This result is relevant because it may help to square the evidence that comes from the survey-based literature with the field-experiments-based literature. Second, lower trust in government may translate into lower effectiveness of the messages (those who do not pay taxes because they do not trust the government may also not trust the messages about levels of tax evasion and public works). The results seem to indicate that this channel is not very strong, which lends support to the use of messages even in countries where people may be a little skeptical about government efficiency.

Third, the average effects may mask differences across individuals. Some of these differences can be uncovered with observable data. In fact, we find that past payment behavior (measured as either having paid in the previous period or having debts) tend to affect how people react to the messages. In particular, those who complied in the past tended to react negatively to information about other people's compliance levels. Levels of income or wealth may also affect how people react to messages, whether because of differences in risk aversion, knowledge about how the system actually works, or different reference points from which to judge the new information. Finally, people with different experiences about the workings of the government may respond differently to messages about the fairness of the system.

While our results provide novel empirical evidence about whether influencing taxpayers' perceptions affects tax compliance, more in depth research is needed to understand the causal channels through which these effects operate. In particular, it would be relevant to disentangle the relative effect of the information about how to calculate fines on unpaid liabilities that was included in the deterrence message from the cautionary messages about the consequences of noncompliance and the images that were also included in the treatment. This would allow for

assessing which is more important: addressing the potential limited computational capabilities of taxpayers or raising the salience of the monetary and legal consequences of noncompliance. Additionally, it may be important to disentangle whether the fairness message is having an effect (when it does) because of social norms or because it provides indirect information about the stringency of the government's enforcement.

Another research avenue would be to concentrate further on using this technique for evaluating the determinants of the individual taxpayer's compliance decision in the standard model of tax evasion by concentrating on the penalty versus detection relationship (Are they substitutes? Complementary?) For any of these experiments, evaluating the persistence of the effect and how to combine it with policies that maintain or strengthen the effect of the messages would prove valuable.

It may also be worthwhile to conduct experiments like this in contexts where additional information is available at the individual level, such as political preferences, to study whether the moral suasion messages affect different people differently, particularly for those messages that could be read through a particular political lens. Moreover, combining this type of experiment with large-scale pre and post-experiment surveys could provide a measure of the perceptions-elasticity of compliance.

Beyond the academic interest, these results have ample policy implications. First, this paper presents additional evidence on the important role of adequately managing the many opportunities that policymakers have to influence citizens' beliefs and how this cheap policy alternative could yield substantive benefits for the government. In the case of taxation, and the fight against tax evasion, this policy alternative minimizes administrative costs, a feature (as indicated by Sandmo, 2005) usually ignored by the literature. Second, it highlights once more the higher prominence of deterrence messages and policies compared with other alternatives. Third, it shows how important it is to design the messages well by selecting only those that can have a positive effect on taxpayers. Finally, it draws attention to the fact that "universal" policies may backfire as positive and negative behavioral responses may cancel each other out. Therefore, policies (particularly nudges) should be tailored to taxpayers' types.

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Tables and Figures

Table 1. Messages Included in the Tax Bill

#	Message	Text of the message	Image
1	Deterrence	Did you know that if you do not pay the CVP on time for a debt of AR$ 1,000 you will have to disburse AR$ 268 in arrears at the end of the year and the Municipality can take administrative and legal action?	
2	Fairness	In the first 6 months of this year, CVP's collection contributed to placing 28 new streetlights, water connections in 29 streets and sewerage networks in 21 blocks.	
3	Equity	Did you know that only 30 percent of taxpayers do not pay the CVP? What about you?	
4		Control group	No message/Image

Table 2. Baseline Summary Statistics and Balance of Randomization (Bim 3)

	(1) Control Group	(2) Difference: Deterrence	(3) Difference: Fairness	(4) Difference: Equity
Trash collection	1.585***	-0.013	0.008	0.009
	(0.004)	(0.011)	(0.011)	(0.011)
Streetlights	2.755***	-0.025	-0.012	-0.041
	(0.010)	(0.025)	(0.025)	(0.025)
Lineal front meters	15.629***	-0.393	-0.200	-0.018
	(0.197)	(0.519)	(0.519)	(0.518)
Mean valuation of properties	15,485.426***	7.038	329.102	506.534
	(182.102)	(480.677)	(481.076)	(480.439)
Properties	1.395***	0.015	-0.056	-0.040
	(0.013)	(0.035)	(0.035)	(0.035)
Monthly payments	0.010***	0.002	0.006**	-0.002
	(0.001)	(0.002)	(0.002)	(0.002)
Tax liability	121.612***	-0.320	-2.898	-3.070
	(1.804)	(4.762)	(4.766)	(4.760)
Tax payments	56.129***	0.661	-1.404	-0.891
	(1.081)	(2.862)	(2.864)	(2.861)
Paid_by1D	0.213***	-0.011	0.014	-0.003
	(0.003)	(0.009)	(0.009)	(0.009)
Paid_by2D	0.332***	-0.014	-0.003	-0.008
	(0.004)	(0.010)	(0.010)	(0.010)
Paid	0.404***	-0.016	0.002	-0.004
	(0.004)	(0.010)	(0.010)	(0.010)
Overpaid	0.031***	-0.002	-0.000	0.004
	(0.001)	(0.004)	(0.004)	(0.004)
Addpay	0.045***	0.002	-0.002	0.000
	(0.002)	(0.004)	(0.004)	(0.004)
Paid at least 50% of the total tax liability	0.427***	-0.014	-0.004	-0.005
	(0.004)	(0.011)	(0.011)	(0.010)
Trash collection (Bim 5)	1.586***	-0.012	0.008	0.007
	(0.004)	(0.011)	(0.011)	(0.011)
Streetlights (Bim 5)	2.752***	-0.026	-0.013	-0.037
	(0.010)	(0.025)	(0.026)	(0.025)

Notes: Each row shows a regression of the pre-treatment variable in question on treatment dummies and a constant term. Observations are presented for the bimonthly period prior to treatment (May-June). The constant captures the value for the control group (no message). Columns (2)-(4) show the difference between the treatment groups and the control group. Monetary amounts are in Argentine Pesos (AR$). Robust standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3. Average Treatment Effects (Bim 5)

VARIABLES	(1) Paid_by1D	(2) Paid_by2D	(3) Paid	(4) OverPaid	(5) Additional Payments	(6) Paid_1D	(7) Paid_2D	(8) Paid	(9) Overpaid	(10) Additional Payments
T1: Deterrence	0.017*	0.030**	0.047***	0.007**	0.011***	0.017*	0.031**	0.048***	0.007*	0.010**
	(0.010)	(0.012)	(0.015)	(0.004)	(0.004)	(0.010)	(0.012)	(0.015)	(0.004)	(0.004)
T2: Fairness	-0.005	-0.002	-0.001	-0.004	-0.004	-0.006	-0.004	-0.003	-0.004	-0.003
	(0.010)	(0.012)	(0.015)	(0.004)	(0.004)	(0.010)	(0.013)	(0.015)	(0.004)	(0.004)
T3: Equity	0.005	0.004	-0.009	0.002	0.006	0.004	0.004	-0.008	0.002	0.007
	(0.010)	(0.013)	(0.015)	(0.004)	(0.004)	(0.010)	(0.013)	(0.015)	(0.004)	(0.004)
Observations	23,195	23,195	23,176	23,073	23,211	23,186	23,186	23,168	23,065	23,081
Model	Baseline	Baseline	Baseline	Baseline	Baseline	Controls	Controls	Controls	Controls	Controls
Sample	Whole	Whole	Whole	Whole	Whole	Whole	Whole	Whole	Whole	Whole
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

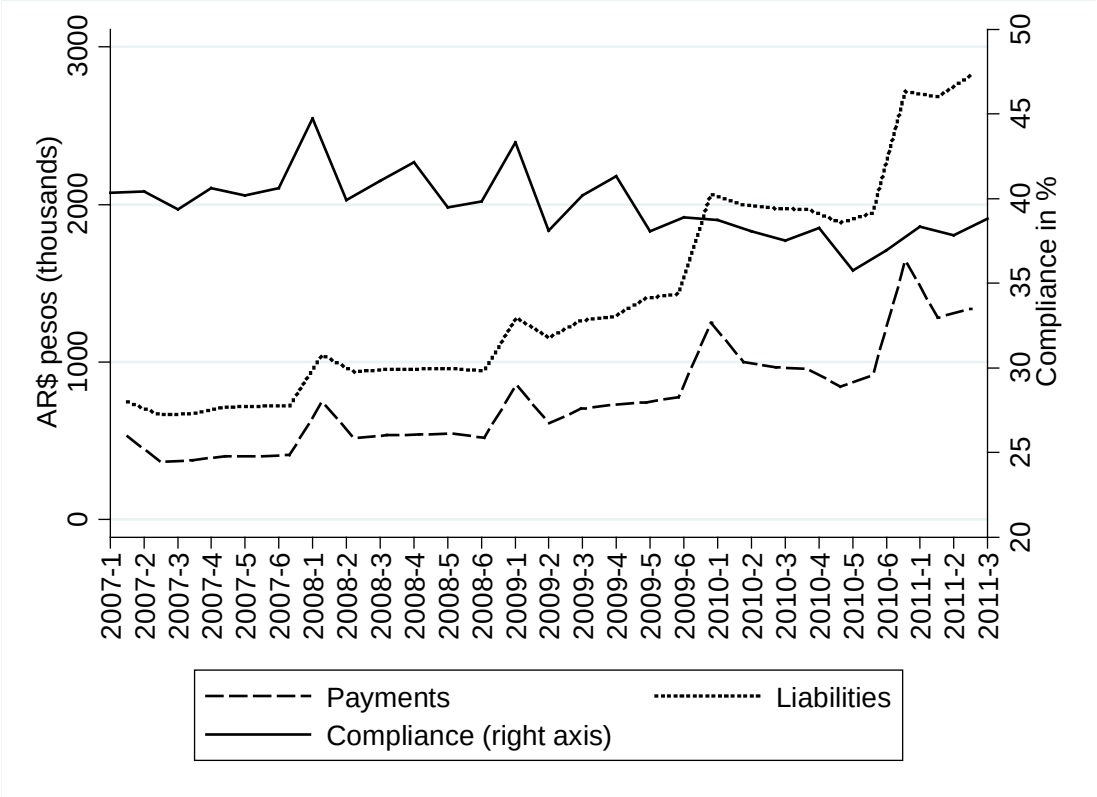
Notes: Dependent variable used in each regression identified in the header. The first set of regressions (baseline) includes the three treatment messages, the lagged outcome variable, and block-level fixed effects. The second set of regressions (controls) also includes the variables for public service provision (trash collection and street lightning services during the period), the (log of the) number of properties that each taxpayer has, the (log of the) average linear front size of the properties, and a dummy that controls for those taxpayers who elected to pay monthly. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Average Treatment Effects (Bim 5): Taxpayers Living Outside the City

VARIABLES	(1) Paid_1D	(2) Paid_2D	(3) Paid	(4) OverPaid	(5) Additional Payments	(6) Paid_1D	(7) Paid_2D	(8) Paid	(9) Overpaid	(10) Additional Payments
T1: Deterrence	0.034	0.115*	0.133*	0.008	0.033	0.042	0.127*	0.140*	0.005	0.031
	(0.053)	(0.063)	(0.073)	(0.037)	(0.040)	(0.054)	(0.068)	(0.074)	(0.033)	(0.040)
T2: Fairness	0.006	0.117*	0.149*	0.000	0.009	0.004	0.119*	0.136*	0.002	0.013
	(0.055)	(0.067)	(0.078)	(0.036)	(0.041)	(0.055)	(0.068)	(0.077)	(0.033)	(0.042)
T3: Equity	0.031	0.053	-0.031	0.030	0.051	0.025	0.050	-0.035	0.025	0.049
	(0.052)	(0.068)	(0.087)	(0.032)	(0.036)	(0.052)	(0.065)	(0.086)	(0.029)	(0.036)
Observations	471	471	470	469	476	468	468	467	466	467
Model	Baseline	Baseline	Baseline	Baseline	Baseline	Controls	Controls	Controls	Controls	Controls
Sample	Whole	Whole	Whole	Whole	Whole	Whole	Whole	Whole	Whole	Whole
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable used in each regression identified in the header. The first set of regressions (baseline) includes the three treatment messages, the lagged outcome variable, and block-level fixed effects. The second set of regressions (controls) also includes the variables for public service provision (trash collection and street lightning services during the period), the (log of the) number of properties that each taxpayer has, the (log of the) average linear front size of the properties, and a dummy that controls for those taxpayers who selected to pay monthly. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1. Tax Liabilities, Payments and Compliance Ratio (AR$ millions and percent), 2007-2011



Source: Authors' compilation based on administrative information provided by the Municipality of Junín.

Figure 2. Interaction Effects of the Treatments with Public Service Provision[37]

Figure 2a. Marginal effect of deterrence message according to public goods received.

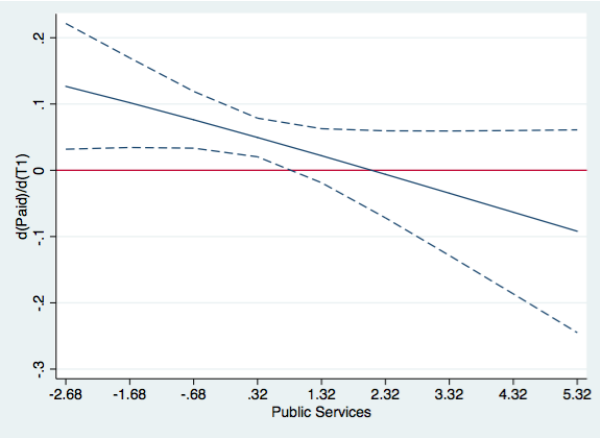


Figure 2b. Marginal effect of fairness message according to public goods received

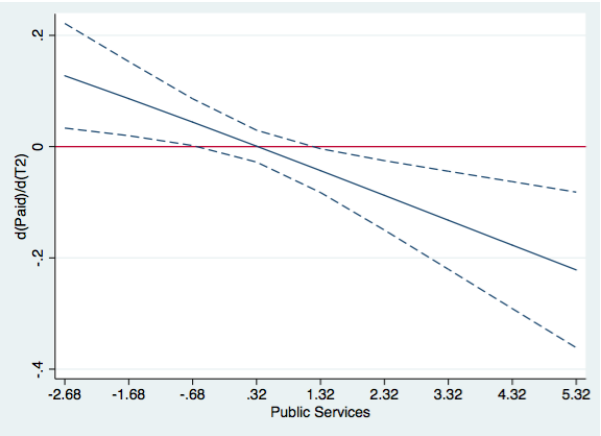


Figure 3. Interaction Effects of the Message with Size of the Property[38]

Figure 3a. Marginal effect of deterrence message according to property size

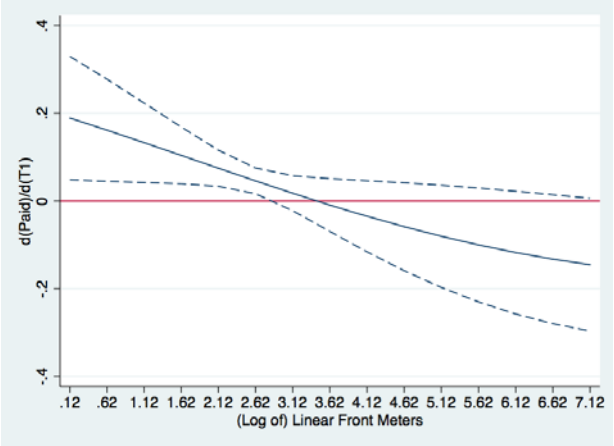
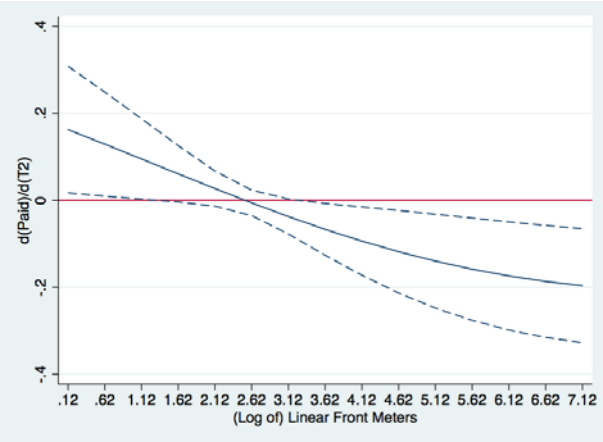


Figure 3b. Marginal effect of fairness message according to property size



[37] The conditional effect of the equity message is not statistically significant at any value of the distribution.
[38] The conditional effect of the equity message is not statistically significant at any value of the distribution.

Figure 4. Marginal Effect of the Treatments According to Past Payment Behavior

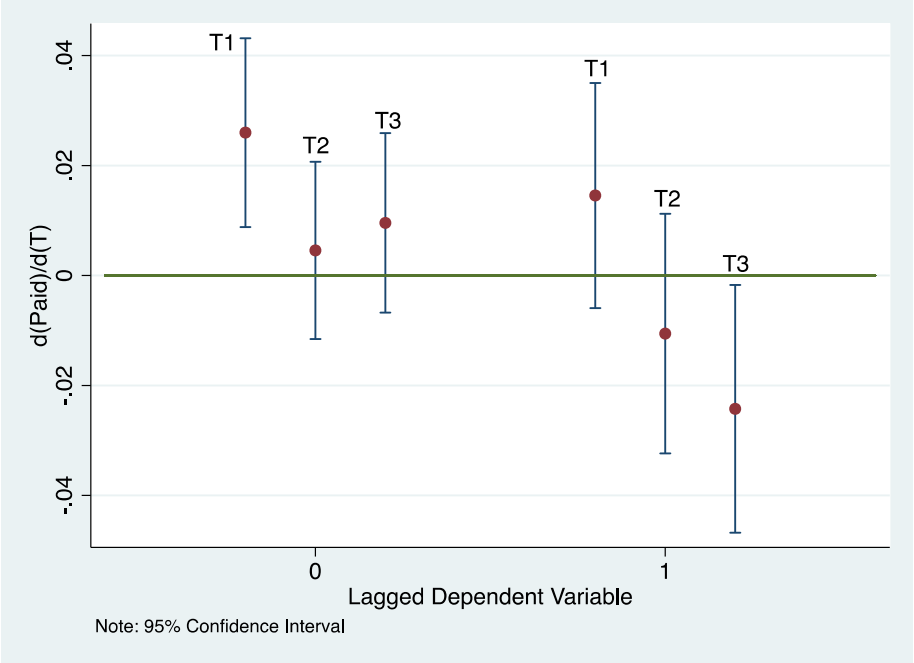


Figure 5. Marginal Effect of the Treatments According to Having Debts

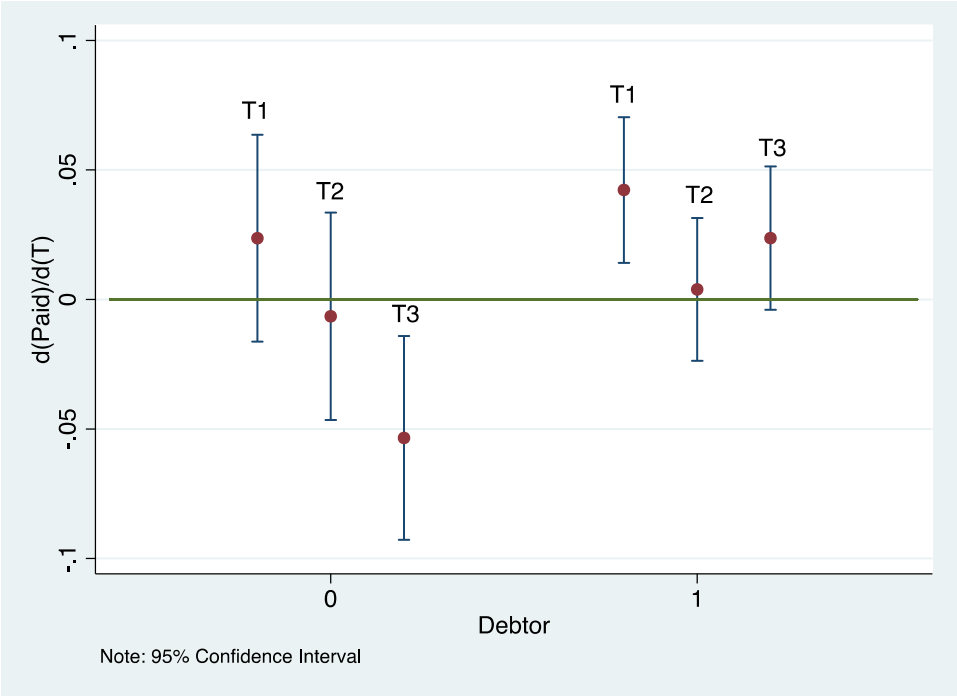
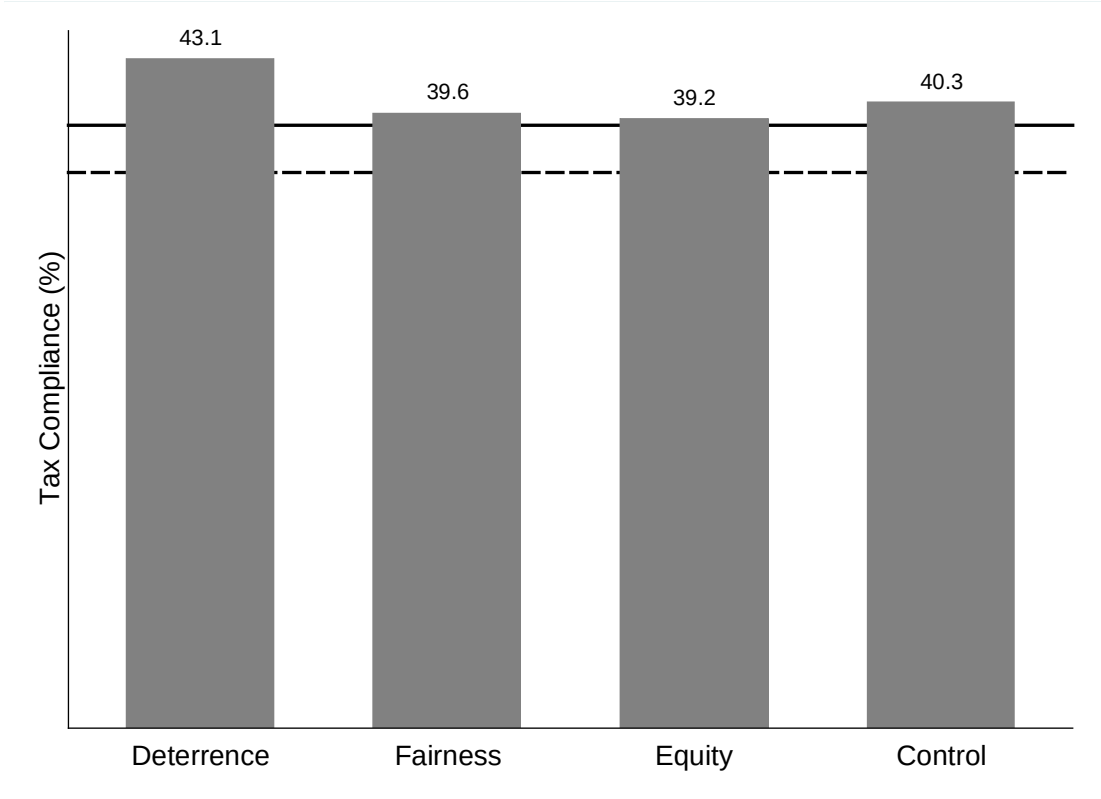


Figure 6. Tax Compliance by Treatment Groups and Control Group (in percent) (Bim 5)



Notes: This figure plots the tax compliance ratio (tax payments over tax liabilities) for the treatment groups and the control. It also displays—in a solid line—the average tax compliance ratio prior to the experiment (Period 3 – May-June) as well as—in a dotted line—the average tax compliance ratio in the same period 5 (September-October) but one year before (2010). The difference in compliance between the group that received the deterrence message and the control is statistically significant at the 1 percent level according to a proportions test. Differences are not statistically significant for the other two groups.

Appendices

Appendix 1. A Closer Look at Junín, Province of Buenos Aires

Table 1. Socio-economic Characteristics of Junín and the Province of Buenos Aires

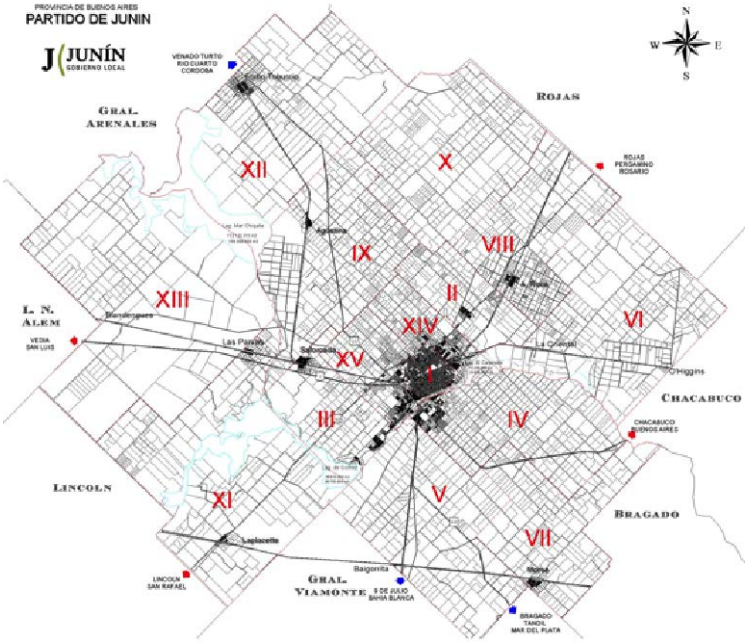
Jurisdiction	Surface (km2)	Population (2010)	Urbanization rate (2010)	School attendance (2001) [1]	GDP in thousands of PPP US$ (2003)	GDP p.c. in PPP US$ (2003)	GDP as percent of Buenos Aires (2003)
Junin	2,260	94,926	90.70%	92.60%	974,615	10,539	0.88%
Average Municipality (Buenos Aires)	2,278	115,514	94.90%	91.90%	817,025	7,654	0.74%

Notes: [1] Percentage of the population between 5 and 18 years old attending any type of school.
Source: Authors' compilation based on Ministry of Interior of Argentina, National Censuses of 2001 and 2010, Provincial Bureau of Statistics and *Tribunal de Cuentas de la Provincia* de Buenos Aires.

Locational Map of Junín within the Province of Buenos Aires

Source: http://www.junin.gov.ar/archivos/mapas.html

Map of Junín by District



Note: This figure presents the map of the 15 administrative districts in which Junín is divided. District I represents the largest district and capital of the municipality, the city of Junín. Names of adjoining municipalities are also display in caps.
Source: http://www.junin.gov.ar/archivos/mapas.html

Appendix 2. Description of Variables

Table 1. Description of Variables

Variable	Description
Trash collection	Categorical variable equal to 0 if neither trash collection nor street cleaning services are provided; 1 if only trash collection services are provided; and 2 if trash collection and street cleaning services are both provided.
Streetlights	Number of streetlights around the properties associated to the taxpayer
Public services	Principal component of the previous two variables
Linear front meters	Average linear front meters of all the properties associated to the taxpayer
Mean valuation of properties	Fiscal assesment of property value. Average for all the properties belonging to an individual taxpayers
Properties	Number of registered properties associated to the taxpayer
Monthly payments	Dummy = 1 if the taxpayer has at least one property for which the bill is paid monthly.
Tax liability	Total amount billed to the taxpayer
Tax payment	Total amount paid by the taxpayer in the period
Tax compliance ratio	Tax payments over tax liabilities ratio (in percent)
Paid_by1D	Dummy = 1 if the payment took place before the 1st due date
Paid_by2D	Dummy = 1 if the payment took place before the 2nd due date
Paid	Dummy = 1 only if the taxpayer has paid in full the total tax liabilities for the period
Overpaid	Dummy = 1 if the taxpayer has paid not only her liabilities for the period in full but she has also made some additional payments
Additionalpayments	Dummy = 1 when the taxpayer has made advanced payments or paid down part of her debt, regardless of whether they have paid their current liabilities in full.
Paid_50p	Dummy = 1 if the taxpayer paid at least 50 percent of the tax liabilities
Debtor	If taxpayer has at least one remaining unpaid bill
Unrecoverable Debtor	Dummy=1 if the taxpayer did not pay any tax bill between 2007 and 2011

Source: All data collected from the administrative records of the Municipality of Junín.

Appendix 3. Sample Tax Bills with Treatment Messages (in Spanish)

Sample Tax Bill: Deterrence Treatment

JUNÍN GOBIERNO LOCAL

C.V.P. (Bimestral)

[redacted]

PERIODO 05/2011

PAGO ELECTRÓNICO 010000005729500000

[redacted]

Detalle: [redacted]

¿Sabía Usted que si no paga a tiempo el CVP, para una deuda de, por ejemplo, 1.000 pesos deberá pagar 268 pesos adicionales a fin de año, y el municipio puede llegar a intimarlo administrativa y hasta judicialmente?

Mantener la ciudad limpia, iluminada y en condiciones es un deber y un derecho de todos. Solo con el pago de sus tasas es posible.

Donar sangre y órganos es dar vida.

DETALLE

Pago Cuota 5/2011

Mts.9.5-Recol/2 Barrido Mecán.-2 Lum.	50,80
Repavimentación Urbana	14,60

Pago Resto año 2011 desde cuota 5

Mts.9.5-Recol/2 Barrido Mecán.-2 Lum.	101,60
Repavimentación Urbana	29,20
Descuento pago resto año (3,36%)	-3,41

Contribuyente con deuda

Próximo Vencimiento 10/11/2011

	CUOTA ACTUAL		RESTO CUOTAS	
1°	12/09/2011	65.40	12/09/2011	127.39
2°	19/09/2011	65.71	19/09/2011	127.99

JUNÍN GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO 29151 PERIODO 05/2011

[redacted]

CUOTA 05/2011 [redacted]

RESTO CUOTAS [redacted]

	CUOTA ACTUAL	
1°	12/09/2011	65.40
2°	19/09/2011	65.71

	ANUAL / RESTO DEL AÑO	
1°	12/09/2011	127.39
2°	19/09/2011	127.99

JUNÍN GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO 29151 PERIODO 05/2011

[redacted]

	ANUAL / RESTO DEL AÑO		CUOTA ACTUAL	
1°	12/09/2011	127.39	12/09/2011	65.40
2°	19/09/2011	127.99	19/09/2011	65.71

[redacted]

Sample Tax Bill: Fairness Treatment

JUNÍN GOBIERNO LOCAL

C.V.P. (Bimestral)

CONTRIBUYENTE

[redacted]

Detalle: [redacted]

La recaudación del CVP en los primeros 6 meses de este año contribuyó a colocar 28 luminarias y 248 luces colgantes, a instalar conexiones de agua en 29 calles, y cloacas en 21 cuadras de Junín.

Mantener la ciudad limpia, iluminada y en condiciones es un deber y un derecho de todos. Solo con el pago de sus tasas es posible.

Donar sangre y órganos es dar vida.

Contribuyente con deuda

Próximo Vencimiento 10/11/2011

PARTIDA [redacted]

PERÍODO 05/2011

PAGO ELECTRÓNICO 010000005783100000

RECIBO 29199

DETALLE

Pago Cuota 5/2011

Mts.9,5-Recol/2 Barrido Mecán.-2 Lum. 50,80

Pago Resto año 2011 desde cuota 5

Mts.9,5-Recol/2 Barrido Mecán.-2 Lum. 101,60

Descuento pago resto año (3,36%) -3,41

	CUOTA ACTUAL		RESTO CUOTAS	
1º	12/09/2011	50.80	12/09/2011	98.19
2º	19/09/2011	51.04	19/09/2011	98.65

JUNÍN GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO 29199

PERÍODO 05/2011

[redacted]

PARTIDA [redacted]

CUOTA 05/2011 [redacted]

RESTO CUOTAS [redacted]

	CUOTA ACTUAL	
1º	12/09/2011	50.80
2º	19/09/2011	51.04

	ANUAL / RESTO DEL AÑO	
1º	12/09/2011	98.19
2º	19/09/2011	98.65

JUNÍN GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO 29199

PERÍODO 05/2011

CONTRIBUYENTE / PARTIDA

[redacted]

	ANUAL / RESTO DEL AÑO		CUOTA ACTUAL	
1º	12/09/2011	98.19	12/09/2011	50.80
2º	19/09/2011	98.65	19/09/2011	51.04

[redacted]

Sample Tax Bill: Equity Treatment

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

www.junin.gob.ar

CONTRIBUYENTE
[redacted]

PARTIDA
[redacted]

PERIODO
05/2011

PAGO ELECTRÓNICO
01000000720000000

RECIBO
29153

Detalle: [redacted]

¿Sabia Usted que sólo el 30% de los contribuyentes de Junín no paga el CVP? ¿Y Usted?

Mantener la ciudad limpia, iluminada y en condiciones es un deber y un derecho de todos. Solo con el pago de sus tasas es posible.

Donar sangre y órganos es dar vida.

DETALLE

Pago Cuota 5/2011

Mts.9,5-Recol/2 Barrido Mecán.-2 Lum.	50,90
Repavimentación Urbana	14,60

Pago Resto año 2011 desde cuota 5

Mts.9,5-Recol/2 Barrido Mecán.-2 Lum.	101,60
Repavimentación Urbana	29,20
Descuento pago resto año (3,36%)	-3,41

Contribuyente con deuda

Próximo Vencimiento 10/11/2011

	CUOTA ACTUAL		RESTO CUOTAS	
1°	12/09/2011	65.40	12/09/2011	127.39
2°	19/09/2011	65.71	19/09/2011	127.99

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO 29153 PERIODO 05/2011

[redacted]

PARTIDA
[redacted]

CUOTA
05/2011

[redacted]

RESTO CUOTAS

[redacted]

	CUOTA ACTUAL	
1°	12/09/2011	65.40
2°	19/09/2011	65.71

	ANUAL / RESTO DEL AÑO	
1°	12/09/2011	127.39
2°	19/09/2011	127.99

JUNÍN
GOBIERNO LOCAL

C.V.P. (Bimestral)

RECIBO 29153 PERIODO 05/2011

CONTRIBUYENTE / PARTIDA
[redacted]

	ANUAL / RESTO DEL AÑO		CUOTA ACTUAL	
1°	12/09/2011	127.39	12/09/2011	65.40
2°	19/09/2011	127.99	19/09/2011	65.71

[redacted]

Appendix 4. Full Regression Tables

Table 1. Dependent Variable *Paid_by1D*

VARIABLES	(1) Paid_by1D	(2) Paid_by1D	(3) Paid_by1D	(4) Paid_by1D	(5) Paid_by1D	(6) Paid_by1D	(7) Paid_by1D	(8) Paid_by1D
T1: Deterrence	0.017*	0.017*	0.017*	0.017*	0.017*	0.022	0.023	0.023
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.014)	(0.014)	(0.014)
T2: Fairness	-0.004	-0.005	-0.006	-0.006	-0.005	-0.004	-0.005	-0.004
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.014)	(0.014)	(0.014)
T3: Equity	0.006	0.005	0.005	0.004	0.004	0.003	0.002	0.002
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.014)	(0.014)	(0.014)
Paid_by1D (t-1)	0.600***	0.563***	0.562***	0.550***	0.549***	0.614***	0.601***	0.599***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)	(0.010)
Streetlights			0.015***	0.003	0.003		0.001	0.000
			(0.003)	(0.004)	(0.004)		(0.005)	(0.005)
Trash Collection			0.120***	0.096***	0.100***		0.101***	0.116***
			(0.007)	(0.009)	(0.009)		(0.013)	(0.013)
Monthly payments			0.038	0.028	0.029		-0.051	-0.048
			(0.030)	(0.030)	(0.030)		(0.081)	(0.082)
Log(Number of Properties)			-0.036***	-0.062***	-0.047***		-0.143***	-0.075***
			(0.012)	(0.012)	(0.015)		(0.016)	(0.024)
Log(Lineal Front Meters)			-0.036***	-0.018**	-0.011		-0.022*	0.010
			(0.008)	(0.008)	(0.009)		(0.012)	(0.015)
Log(Tax Liability)					-0.010			-0.042***
					(0.006)			(0.011)
Observations	23,195	23,195	23,186	23,186	23,186	15,845	15,837	15,837
Model	Baseline	Baseline	Controls	Controls	Controls	Baseline	Controls	Controls
Sample	Whole	Whole	Whole	Whole	Whole	Non-defaulters	Non-defaulters	Whole
Fix effects	No	Yes	No	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2. Dependent Variable *Paid_by2D*

VARIABLES	(1) Paid_by2D	(2) Paid_by2D	(3) Paid_by2D	(4) Paid_by2D	(5) Paid_by2D	(6) Paid_by2D	(7) Paid_by2D	(8) Paid_by2D
T1: Deterrence	0.031**	0.030**	0.031**	0.031**	0.031**	0.038**	0.039**	0.039**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.016)	(0.016)	(0.016)
T2: Fairness	-0.002	-0.002	-0.004	-0.004	-0.004	-0.002	-0.004	-0.003
	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)	(0.016)
T3: Equity	0.005	0.004	0.004	0.004	0.004	0.003	0.002	0.002
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)	(0.016)
Paid_by2D (t-1)	0.840***	0.809***	0.809***	0.797***	0.797***	0.806***	0.794***	0.792***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Streetlights			0.014***	0.008*	0.008		0.008	0.007
			(0.004)	(0.005)	(0.005)		(0.006)	(0.006)
Trash Collection			0.092***	0.081***	0.088***		0.079***	0.101***
			(0.009)	(0.011)	(0.012)		(0.015)	(0.016)
Monthly payments			0.108***	0.104***	0.105***		0.213***	0.218***
			(0.040)	(0.040)	(0.040)		(0.082)	(0.083)
Log(Number of Properties)			-0.050***	-0.070***	-0.034*		-0.157***	-0.053**
			(0.015)	(0.015)	(0.019)		(0.017)	(0.027)
Log(Lineal Front Meters)			-0.030***	-0.021**	-0.005		-0.029**	0.020
			(0.009)	(0.010)	(0.011)		(0.013)	(0.016)
Log(Tax Liability)					-0.023***			-0.064***
					(0.008)			(0.013)
Observations	23,195	23,195	23,186	23,186	23,186	15,845	15,837	15,837
Model	Baseline	Baseline	Controls	Controls	Controls	Baseline	Controls	Controls
Sample	Whole	Whole	Whole	Whole	Whole	Non-defaulters	Non-defaulters	Whole
Fix effects	No	Yes	No	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Dependent Variable *Paid*

VARIABLES	(1) Paid	(2) Paid	(3) Paid	(4) Paid	(5) Paid	(6) Paid	(7) Paid	(8) Paid
T1: Deterrence	0.046***	0.047***	0.047***	0.048***	0.047***	0.043***	0.044***	0.044***
	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
T2: Fairness	-0.000	-0.001	-0.002	-0.003	-0.003	0.001	-0.000	0.001
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
T3: Equity	-0.009	-0.009	-0.008	-0.008	-0.009	-0.017	-0.017	-0.016
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
Paid (t-1)	1.041***	1.011***	1.014***	1.001***	1.007***	0.876***	0.864***	0.861***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Streetlights			0.010**	0.003	0.003		0.000	-0.000
			(0.005)	(0.005)	(0.005)		(0.006)	(0.006)
Trash Collection			0.092***	0.071***	0.058***		0.064***	0.080***
			(0.011)	(0.013)	(0.014)		(0.014)	(0.016)
Monthly payments			0.159***	0.153***	0.150***		0.339***	0.341***
			(0.053)	(0.053)	(0.053)		(0.087)	(0.086)
Log(Number of Properties)			-0.033*	-0.061***	-0.121***		-0.136***	-0.066**
			(0.017)	(0.018)	(0.024)		(0.017)	(0.030)
Log(Lineal Front Meters)			-0.039***	-0.028**	-0.053***		-0.022*	0.013
			(0.010)	(0.012)	(0.013)		(0.013)	(0.018)
Log(Tax Liability)					0.039***			-0.045***
					(0.010)			(0.016)
Observations	23,176	23,176	23,168	23,168	23,168	15,831	15,823	15,823
Model	Baseline	Baseline	Controls	Controls	Controls	Baseline	Controls	Controls
Sample	Whole	Whole	Whole	Whole	Whole	Non-defaulters	Non-defaulters	Whole
Fix effects	No	Yes	No	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Dependent Variable *Overpaid*

VARIABLES	(1) Overpaid	(2) Overpaid	(3) Overpaid	(4) Overpaid	(5) Overpaid	(6) Overpaid	(7) Overpaid	(8) Overpaid
T1: Deterrence	0.007*	0.007**	0.007*	0.007*	0.007*	0.010*	0.009*	0.009*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
T2: Fairness	-0.004	-0.004	-0.004	-0.004	-0.003	-0.005	-0.005	-0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
T3: Equity	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Overpaid (t-1)	0.035***	0.032***	0.032***	0.031***	0.031***	0.031***	0.031***	0.031***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)
Streetlights			0.001	0.001	0.001		0.003	0.002
			(0.001)	(0.001)	(0.001)		(0.002)	(0.002)
Trash Collection			0.008***	0.007**	0.009***		-0.000	0.005
			(0.003)	(0.003)	(0.003)		(0.005)	(0.005)
Monthly payments			0.011	0.010	0.011		0.030	0.032
			(0.010)	(0.010)	(0.010)		(0.026)	(0.026)
Log(Number of Properties)			-0.003	-0.005	0.005		-0.015**	0.009
			(0.004)	(0.004)	(0.005)		(0.006)	(0.009)
Log(Lineal Front Meters)			-0.014***	-0.015***	-0.010***		-0.025***	-0.013**
			(0.003)	(0.003)	(0.004)		(0.005)	(0.006)
Log(Tax Liability)					-0.006***			-0.015***
					(0.002)			(0.004)
Observations	23,174	23,073	23,166	23,065	23,065	15,794	15,786	15,786
Model	Baseline	Baseline	Controls	Controls	Controls	Baseline	Controls	Controls
Sample	Whole	Whole	Whole	Whole	Whole	Non-defaulters	Non-defaulters	Whole
Fix effects	No	Yes	No	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Dependent Variable *Additional payments*

VARIABLES	(1) Additional Payments	(2) Additional Payments	(3) Additional Payments	(4) Additional Payments	(5) Additional Payments	(6) Additional Payments	(7) Additional Payments	(8) Additional Payments
T1: Deterrence	0.011**	0.011***	0.010**	0.010**	0.010**	0.015**	0.014**	0.014**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
T2: Fairness	-0.004	-0.004	-0.003	-0.003	-0.003	-0.006	-0.004	-0.004
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
T3: Equity	0.007	0.006	0.007	0.007	0.007	0.009	0.009	0.009
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
Additional Payments (t-1)	0.061***	0.056***	0.054***	0.053***	0.053***	0.060***	0.058***	0.058***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
Streetlights			0.001	0.001	0.001		0.001	0.001
			(0.001)	(0.002)	(0.002)		(0.002)	(0.002)
Trash Collection			0.008**	0.004	0.006		-0.006	-0.001
			(0.003)	(0.004)	(0.004)		(0.005)	(0.006)
Monthly payments			0.019*	0.019*	0.019*		0.037	0.038
			(0.010)	(0.010)	(0.010)		(0.026)	(0.026)
Log(Number of Properties)			0.029***	0.026***	0.033***		0.029***	0.050***
			(0.004)	(0.004)	(0.005)		(0.006)	(0.010)
Log(Lineal Front Meters)			-0.013***	-0.012***	-0.009**		-0.019***	-0.009
			(0.003)	(0.004)	(0.004)		(0.006)	(0.007)
Log(Tax Liability)					-0.004*			-0.013***
					(0.003)			(0.005)
Observations	23,313	23,211	23,182	23,081	23,081	15,915	15,800	15,800
Model	Baseline	Baseline	Controls	Controls	Controls	Baseline	Controls	Controls
Sample	Whole	Whole	Whole	Whole	Whole	Non-defaulters	Non-defaulters	Whole
Fix effects	No	Yes	No	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Dependent Variable *Paid at least 50 percent of tax liabilities (paid_50p)*

VARIABLES	(1) Paid_50p	(2) Paid_50p	(3) Paid_50p	(4) Paid_50p	(5) Paid_50p	(6) Paid_50p	(7) Paid_50p	(8) Paid_50p
T1: Deterrence	0.048***	0.050***	0.050***	0.050***	0.050***	0.044***	0.045***	0.045***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
T2: Fairness	0.013	0.012	0.012	0.011	0.011	0.012	0.012	0.012
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
T3: Equity	-0.011	-0.011	-0.010	-0.011	-0.011	-0.016	-0.016	-0.016
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Paid at least 50% of all the liabilities (i	1.061***	1.028***	1.035***	1.022***	1.027***	0.825***	0.821***	0.818***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Streetlights			0.012**	0.003	0.003		-0.000	-0.000
			(0.005)	(0.006)	(0.006)		(0.006)	(0.006)
Trash Collection			0.089***	0.060***	0.046***		0.045***	0.061***
			(0.011)	(0.013)	(0.014)		(0.014)	(0.015)
Monthly payments			0.250***	0.243***	0.240***		0.363***	0.364***
			(0.059)	(0.059)	(0.059)		(0.126)	(0.127)
Log(Number of Properties)			0.016	-0.008	-0.072***		-0.059***	0.011
			(0.017)	(0.017)	(0.024)		(0.016)	(0.028)
Log (Lineal Front Meters)			-0.038***	-0.022*	-0.049***		-0.013	0.021
			(0.011)	(0.012)	(0.013)		(0.012)	(0.017)
Log(Tax Liability)					0.041***			-0.044***
					(0.010)			(0.015)
Observations	23,176	23,176	23,168	23,168	23,168	15,831	15,823	15,823
Model	Baseline	Baseline	Controls	Controls	Controls	Baseline	Controls	Controls
Sample	Whole	Whole	Whole	Whole	Whole	Non-defaulters	Non-defaulters	Whole
Fix effects	No	Yes	No	Yes	Yes	Yes	Yes	Yes

Appendix 5. Excluding Unrecoverable Debtors

Table 1. Excluding Unrecoverable Debtors (Bim 5)

VARIABLES	(1) Paid_1D	(2) Paid_2D	(3) Paid	(4) OverPaid	(5) Additional Payments	(6) Paid_1D	(7) Paid_2D	(8) Paid	(9) Overpaid	(10) Additional Payments
T1: Deterrence	0.022	0.038**	0.043***	0.010*	0.015**	0.023	0.039**	0.044***	0.009*	0.014**
	(0.014)	(0.016)	(0.016)	(0.005)	(0.006)	(0.014)	(0.016)	(0.016)	(0.005)	(0.006)
T2: Fairness	-0.004	-0.002	0.001	-0.005	-0.006	-0.005	-0.004	-0.000	-0.005	-0.004
	(0.014)	(0.016)	(0.016)	(0.006)	(0.006)	(0.014)	(0.016)	(0.016)	(0.006)	(0.006)
T3: Equity	0.003	0.003	-0.017	0.002	0.009	0.002	0.002	-0.017	0.002	0.009
	(0.014)	(0.016)	(0.016)	(0.005)	(0.006)	(0.014)	(0.016)	(0.016)	(0.005)	(0.006)
Observations	15,845	15,845	15,831	15,794	15,915	15,837	15,837	15,823	15,786	15,800
Model	Baseline	Baseline	Baseline	Baseline	Baseline	Controls	Controls	Controls	Controls	Controls
Sample	Non-defaulters	Non-defaulters	Non-defaulters	Non-defaulters	Non-defaulters	Non-defaulters	Non-defaulters	Non-defaulters	Non-defaulters	Non-defaulters
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable used in each regression identified in the header. The first set of regressions (baseline) includes the three treatment messages, the lagged outcome variable, and block-level fixed effects. The second set of regressions (controls) also includes also the variables for public service provision (trash collection and street lightning services during the period), the (log of the) number of properties that each taxpayer has, the (log of the) average linear front size of the properties, and a dummy that controls for those taxpayers who selected to pay monthly. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2. Balancing of the Sample Excluding Unrecoverable Debtors

	(1) Control Group	(2) Difference: Deterrence	(3) Difference: Fairness	(4) Difference: Equity
Trash collection	1.667***	-0.006	0.011	0.000
	(0.005)	(0.012)	(0.012)	(0.012)
Streetlights	2.866***	-0.012	-0.009	-0.050*
	(0.011)	(0.030)	(0.030)	(0.030)
Lineal front meters	14.447***	0.178	-0.182	0.508
	(0.174)	(0.459)	(0.458)	(0.456)
Mean valuation of properties	17,409.872***	-31.546	-192.915	489.637
	(199.751)	(528.298)	(526.250)	(524.859)
Properties	1.445***	0.013	-0.048	-0.045
	(0.016)	(0.043)	(0.043)	(0.043)
Monthly payments=1	0.003***	0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Tax liability	128.839***	0.314	-0.869	-1.142
	(1.766)	(4.670)	(4.652)	(4.639)
Payment for current bim	77.044***	-0.488	-2.326	-2.133
	(1.336)	(3.546)	(3.533)	(3.524)
Paid 100% of the total tax liability before first expiration	0.303***	-0.018	0.013	-0.007
	(0.004)	(0.012)	(0.012)	(0.012)
Paid 100% of the total tax liability before second expiration	0.471***	-0.021*	-0.010	-0.015
	(0.005)	(0.013)	(0.013)	(0.013)
Paid	0.571***	-0.022*	-0.005	-0.013
	(0.005)	(0.013)	(0.013)	(0.013)
Overpaid	0.045***	-0.004	-0.001	0.003
	(0.002)	(0.005)	(0.005)	(0.005)
Addpay	0.063***	0.003	-0.004	-0.002
	(0.002)	(0.006)	(0.006)	(0.006)
Paid at least 50% of the total tax liability	0.604***	-0.019	-0.015	-0.015
	(0.005)	(0.013)	(0.013)	(0.013)
Trash collection (Bim 5)	1.669***	-0.004	0.012	-0.002
	(0.005)	(0.012)	(0.012)	(0.012)
Streetlights (Bim 5)	2.862***	-0.011	-0.008	-0.044
	(0.012)	(0.031)	(0.030)	(0.030)

Notes: Each row shows a regression of the pre-treatment variable in question on treatment dummies and a constant term. Observations are presented for the bimonthly period prior to treatment (May-June). The constant captures the value for the control group (no message). Columns (2)-(4) show the difference between the treatment groups and the control group. Monetary amounts are in Argentine Pesos (AR$). Robust standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Appendix 6. Interaction Models

Table 1. Probit Regressions: Interaction Models

VARIABLES	(1) Paid	(2) Paid	(3) Paid	(4) Paid
T1: Deterrence	0.152***	0.069	0.497**	0.151***
	(0.047)	(0.060)	(0.197)	(0.041)
T2: Fairness	0.028	-0.019	0.431**	0.039
	(0.050)	(0.058)	(0.201)	(0.041)
T3: Equity	0.059	-0.149***	-0.079	0.009
	(0.049)	(0.055)	(0.205)	(0.042)
Paid (t-1)	2.667***	1.355***	2.629***	2.639***
	(0.030)	(0.047)	(0.025)	(0.025)
Streetlights	0.008	-0.001	0.009	
	(0.014)	(0.015)	(0.014)	
Trash Collection	0.187***	0.165***	0.185***	
	(0.034)	(0.036)	(0.034)	
Monthly payments	0.404***	1.241***	0.402***	0.410***
	(0.139)	(0.092)	(0.139)	(0.139)
Log(Number of Properties)	-0.159***	-0.135***	-0.159***	-0.168***
	(0.046)	(0.048)	(0.046)	(0.046)
Log (Lineal Front Meters)	-0.074**	-0.018	-0.045	-0.115***
	(0.030)	(0.031)	(0.034)	(0.028)
Debtor		-1.543***		
		(0.051)		
Public Goods Provision (PCA)				
T1: Deterrence x paid(t-1)	-0.066			
	(0.080)			
T2: Fairness x paid(t-1)	-0.086			
	(0.078)			
T3: Equity x paid(t-1)	-0.187**			
	(0.076)			
T1: Deterrence x debtor		0.093		
		(0.079)		
T2: Fairness x debtor		0.034		
		(0.081)		
T3: Equity x debtor		0.242***		
		(0.077)		
T1: Deterrence x meters			-0.144*	
			(0.074)	
T2: Fairness x meters			-0.170**	
			(0.076)	
T3: Equity x meters			0.022	
			(0.078)	
T1: Deterrence x Public Serv				-0.065*
				(0.034)
T2: Fairness x Public Serv				-0.040
				(0.036)
T3: Equity x Public Serv				-0.154***
				(0.035)
Constant	-1.220***	0.055	-1.265***	-1.747***
	(0.199)	(0.217)	(0.202)	(0.230)
Observations	23,168	23,168	23,168	23,161
Interaction	Lagged Dep. Var.	Debtor	Linear Front Meters	Public Goods
Model	Controls	Controls	Controls	Controls
Sample	Whole	Whole	Whole	Whole
Fix effects	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.