From Flat to Fair?

The Effects of a Progressive Tax Reform

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Abstract

This paper investigates the relationship between tax progressivity and compliance. We leverage a major progressive tax reform in a large Argentine municipality. First, we use a quasi-experimental design to estimate the causal effect of changes in a household's own tax rates on its tax compliance. Second, we utilize a large-scale natural field experiment to examine whether, holding a household's own tax rates constant, tax compliance is influenced by the tax rates of poor or rich households. We find that reducing taxes for poor households increases their compliance, while increasing taxes for rich households decreases theirs. When poor households learn about the tax hike on the rich, this increases their stated perceived fairness of the tax system and their actual tax compliance. When rich households learn about the tax cuts for the poor, their stated perceived fairness also increases significantly, but their compliance, if anything, goes down. Leveraging a further reform and an additional field experiment that took place a year later, we show that both the quasi-experimental and experimental findings replicate. Our evidence highlights that tax compliance depends not only on a household's own tax rate but also on how they *truly* feel about the broader tax schedule. Our findings also highlight the gap between stated and revealed preferences for redistribution. Lastly, we conduct a counterfactual analysis to illustrate the implications of our findings for the design of tax policies.

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1 Introduction

Progressive tax schedules are widespread throughout the world and play a crucial role in the redistribution of income (Piketty and Saez, 2007; Saez and Zucman, 2019a).[1] There are, however, significant differences in the degree of tax progressivity between countries and even across different taxes within a given country (Fisher-Post and Gethin, 2023). A natural hypothesis for the adoption of progressive tax schedules is that individuals care not only about how policies will affect them, but also about how they will impact others, for example, through other-regarding preferences or social comparisons (e.g., see Stantcheva, 2021). For instance, wealthy households may tolerate a reform that increases their tax burden if it advances fairness objectives, such as supporting disadvantaged households. Although it is such a pillar of modern tax systems, empirical evidence on the effects of tax progressivity is scarce. This study addresses this gap by analyzing how taxpayers respond to a progressive tax reform.

We combine experimental and quasi-experimental methods to show that increasing tax progressivity has substantial effects on tax compliance, and that those effects vary depending on how the reform affects the individual taxpayer and their peers. Our findings offer valuable insights into individuals' true attitudes toward progressive taxation. Individuals may state that they prefer more progressive taxes (Tarroux, 2019), but talk is cheap: do they put their money where their mouths are? The behavioral responses to a progressive reform give us insights on the attitudinal and practical consequences of such reforms and on the determinants of the support for progressive taxation in general. Our results have important implications for designing revenue-neutral tax reforms and for their support, particularly in contexts where tax enforcement is limited.

We hypothesize that progressive taxation could affect tax compliance through two distinct channels. The *own-rate* effect posits that a taxpayer's tax compliance may respond to a change in their own tax rate, irrespective of what happens to the tax rates of other households. For instance, lowering the taxes on a poor household may increase its tax compliance, while increasing the taxes on a rich household may lower its tax compliance. The *cross-rate* effect refers to the fact that, while keeping its own tax rate constant, the compliance of a taxpayer may also depend on their perception about the tax rates of other households. In the context of a progressive reform, a poor household may change its tax compliance after finding out that, in addition to lowering its own tax rate, the government increased that on the rich. Likewise, a rich household may change its tax compliance upon learning that the government reduced tax rates for the poor, over and above the effect of an increase in its own tax rate. Ex-ante, cross-rate effects could be positive or negative,

[1] For example, according to Fisher-Post and Gethin (2023), progressive tax systems reduce inequality by 5% to 15% in Western Europe and the United States. For more evidence on developing countries, see Lustig (2023) and Bachas et al. (2024).

depending on the prevailing social preferences and how different groups of taxpayers are affected. For example, some taxpayers may react negatively to increased progressivity if they believe that it would be fairest for everyone to pay the same rate. On the other hand, if taxpayers want more progressive taxes, finding out about a progressive reform may boost their tax morale.

We study a progressive tax reform on property taxes implemented by Tres de Febrero, a major municipality in the province of Buenos Aires, Argentina (and a suburb of the nation's capital). The tax is levied monthly on the assessed values of properties, with an average tax rate of about 2.6% in 2022. The municipality uses tax revenues to fund basic services such as street lighting and urban sanitation. In January 2023, the municipality implemented a progressive tax reform intended to be revenue-neutral. The properties at the bottom 37% of the value distribution received a tax cut of about 30%. Hereinafter, we refer to these as *poor households* for short.[2] Properties in the top 27% (*rich households*) faced a significant tax increase of about 15%.[3] Tax rates for properties with valuations in the middle of the distribution were not affected by the reform – hereinafter, we refer to these as *middle households* for short.

The setting in which this reform occurred has two advantages in addressing the research question at hand. First, there is substantial inequality, and thus there is ample scope for redistribution through taxation.[4] This inequality is also reflected in home values. For example, a property in the 90th percentile of assessed value is worth 6.9 times as much as a property in the 10th percentile. The second advantage is that, due to limitations in tax enforcement, compliance choices provide insight into an individuals tax morale. For example, in the year prior to the reform, the average probability of making timely payments (i.e., within three months of the due date) was 47.9%. Thus, there is scope for tax compliance to increase or decrease in response to the reform.

We identify reform's own-rate effects by leveraging two sources of identification. First, the reform involved two sharp discontinuities: households with property values below a predetermined threshold (poor households) received a tax cut, while households with property values above a different threshold (rich households) were hit with a tax hike. Properties in the middle group did not experience any tax change. We use a two-cutoff cumulative regression discontinuity design (RDD) (Cattaneo et al., 2016). Moreover, we leverage the timing of the reform as a second source of causal identification: we compare the evolution of the outcomes right before versus right after the reform took effect.

[2]We use "households" for brevity, but the sample also includes some commercial properties, which represent approximately 16% of the total.

[3]The 30% tax reduction for the poor and 15% surcharge for the rich are simplified summaries of the reform. Section 2.3 details its full implementation.

[4]For instance, the Gini coefficient for the Greater Buenos Aires Metropolitan Area (which includes Tres de Febrero) was 0.404 (CEDLAS, 2024), which is comparable to that in cities such as Miami, Lisbon, and Brussels (OECD, 2018).

The RDD estimates reveal significant own-rate effects of the reform on tax compliance, which we can summarize as behavioral elasticities. A 1% reduction in the tax rate for the poor increases their compliance by 0.257%, implying an elasticity of ε^{own}_{poor}=0.257. In contrast, a 1% increase in the tax rate for the rich reduces their compliance by 0.484%, corresponding to an elasticity of ε^{own}_{rich}=0.484.[5] While we do not have data to disentangle the two, our preferred interpretation is that the effects of the own rate are likely a mix of price and tax morale effects. For example, some poor households may have wanted to comply all along, but were not able to afford their tax liabilities. The price effect represents the impact of lowering their tax obligations – more affordable taxes are more likely to be paid (e.g., see Brockmeyer et al., 2023; Bergeron et al., 2024). Likewise, higher taxes on the rich make them less affordable, making them less likely to pay. However, tax morale may also contribute to the own-rate effects. For instance, when facing a tax cut the poor may increase their compliance to reciprocate the government's gesture, or because of a perception of increased fairness in the tax system. Likewise, the rich may reduce their compliance after a tax hike to reciprocate the government's measure that hurt them, or because it may lower their perceived fairness of the system.

These RDD estimates are robust to a host of sensitivity checks. Most notably, we provide two key falsification tests: we find null effects when using placebo thresholds and when using a placebo event date for the policy change. Moreover, we leveraged a second progressive tax reform that took effect in January 2024, a year after the first reform. The new reform used the same thresholds to determine which households would experience a tax cut and which would experience a tax hike. We can reproduce the RDD analysis exactly as for the first reform. Reassuringly, the results replicate: we find estimates that are both qualitatively consistent and in the same order of magnitude for both reforms.

To identify the cross-rate effects of the reform, we conducted a pre-registered, large-scale, natural field experiment, in which we randomized information about the progressive nature of the tax reform. As part of regular communication with taxpayers, the municipality sent letters to a large sample of taxpayers. We embedded an information provision experiment in the January 2023 mailers. We randomly assigned each subject to receive one of two types of letters. The first type included information solely on how the reform changed the household's own tax rate. The second type included additional information about how the reform affected the tax rates of other households. For instance, a poor household could receive one of two letters: a control letter detailing the tax cut for poor households, or a treatment letter that also highlighted the broader reform (that is, mentioning also the tax hike for rich households). Both groups received information about the amounts deducted from or added to their bills. This approach ensured that informa-

[5]Because the reform did not affect the tax rate of middle households, we cannot estimate a corresponding own-rate effect for this group.

tion about changes to a household's own tax rate was held constant while experimentally varying awareness about how the reform affected other households.

Using administrative records, we can estimate the impact of treatment on subsequent tax compliance. In addition, to provide complementary evidence on the causal mechanisms at play, we collected survey data. We invited a subsample of taxpayers with email addresses in file to complete a brief online survey, achieving a 13.2% response rate with 1,851 households participating. One potential concern with information-provision experiments is that subjects may not pay attention to the information provided to them or that the control group may find out about the information through other means. To address these concerns, the survey included a question designed to assess awareness of the progressive tax reform: subjects received a list of recent policies and were asked to indicate which ones they had heard of. With this outcome, we can measure whether treatment actually had a significant effect on awareness of the progressive reform. And to investigate the tax morale mechanism, one survey question elicited perceived fairness of the tax system, using a subjective scale from 0 (very unfair) to 10 (very fair).

We begin by presenting evidence that a significant proportion of subjects opened the letters sent to them. To establish this, we selected a small random sample of households *not* to receive a letter. We find that households that received the control letter, compared to those that did not receive any letter, exhibited a significant increase in tax compliance. Furthermore, the survey data shows that households paid attention to the information provided in the letters. Only a small minority of households in the control group were aware of the progressive tax reform. However, those assigned to the treatment letter were significantly more likely to be aware of the reform than those assigned to the control letter.

Next, we use administrative records to compare tax compliance between households with treatment and control letters. We start with the results for poor households. We show that the treatment had a positive and significant effect on their tax compliance. In other words, poor households increase their tax compliance when they learn about the progressive nature of the reform. Relative to poor households who received the control letter, the treatment caused a statistically significant (p-value = 0.006) increase in tax compliance of 0.808 percentage points (pp). We estimate a cross-rate elasticity of $\varepsilon_{poor}^{cross}$=0.098 for poor households: a 1% increase in the (perceived) taxes of the rich increases their own tax compliance by 0.098%. To put this magnitude in context, we can compare it with the effect of the own rate: a 1% increase in the tax rate of the rich has the same effect on the compliance of a poor household as lowering the poor household's own tax rate by 0.38%. Although the magnitude of this cross-rate effect is already substantial, this is just an intention-to-treat estimate, so the treatment effects on the treated could be

substantially higher.[6] By construction, since the household's own taxes are held constant, the cross-rate effects cannot be attributed to price effects. Instead, our preferred interpretation is that the cross-rate effects are driven by the tax morale mechanism. Indeed, we use the survey data to provide some direct evidence in support of this hypothesis: when informed about the tax hike on the rich, poor households increase their rating of the tax system's fairness.

Next, we discuss the cross-rate effects for rich households. As with poor households, we find that the treatment increased rich households' awareness of the progressive tax reform. Moreover, when the rich find out that the poor are getting a tax cut, they are more likely to state that the tax system is fair. But although their stated perceptions of the tax system's fairness increased by about the same degree as for the poor, we do not find a positive effect on tax compliance for the rich. If anything, the rich become *less* likely to pay their taxes after finding out that the poor experienced a tax cut, although the effect is close to zero in magnitude ($\varepsilon_{rich}^{cross}$=0.019) and not statistically significant. We also estimate the cross-rate effects for middle households. Although they do not experience a change in their own tax rates, finding out about the tax hike on the rich and the tax cut on the poor may still affect their compliance. The results for this group mimic those of rich households. The treatment had a significant positive effect on the awareness of the progressive tax reform and on the stated perception of fairness of the tax system; however, the effect on tax compliance is close to zero and statistically insignificant.

The results of the field experiment are robust to a host of sensitivity checks. Most importantly, we estimated an event-study analysis showing that the timing of the effects of the treatment coincides exactly with the timing of the mailing intervention. Moreover, to see if the results replicate, we conducted another pre-registered field experiment in January 2024, leveraging the second progressive tax reform that took place at that time. The results for the 2024 experiment are largely similar to the results from the 2023 experiment. Lastly, to assess whether these results were unexpected, we conducted a prediction survey with 39 academics with relevant research experience in public finance and related fields. After receiving a description of the experiment, these experts were asked to forecast the effects of the treatment. The survey revealed a lack of expert consensus, with significant variation in predictions and most experts expressing uncertainty about their forecasts. Only a minority predicted effects similar to our experimental estimates.[7]

Our findings have potential implications for the design of progressive tax reforms. In summary, the government must factor in behavioral responses to accurately forecast the effects of a tax policy (Saez and Zucman, 2023). Once behavioral responses are factored in,

[6]For example, a significant fraction of the households may not have received the letters, or they have not read them carefully.

[7]For more details about the design and the results of the forecast survey, see Appendix H.

tax progressivity may not increase as much as intended, and a reform that was supposed to be revenue-neutral may turn out not to be. Our counterfactual analysis evaluates a hypothetical tax reform that reduces the tax liability for poor households by 45.5% and increases it for rich households by 17.32%. In the absence of behavioral responses, the reform is revenue-neutral and increases the gap in tax rates between rich and poor households, a simple measure of tax progressivity, by 0.5 pp. To compare the outcomes with and without behavioral responses, we adopt a sufficient statistics approach (Chetty, 2009): we show that the counterfactual analysis depends solely on the four behavioral elasticities estimated with the quasi-experimental and experimental approaches. Relative to the counterfactual without behavioral responses, the real effects of the reform are significantly different, with an effective tax progressivity that is 48% lower and tax revenues that are 4.4% lower. Lastly, the role of cross-rate effects shows that behavioral responses may also depend on how taxpayers perceive the broader tax system.

Our paper relates and contributes to multiple strands of literature. First, we contribute to the literature on tax progressivity, which has recently received growing attention. This increased interest is particularly notable in the context of property taxes, which tend to have flat schedules, strongly contrasting with modern progressive income and wealth taxes (Chancel et al., 2022; Dray et al., 2023). Recent studies have documented trends in progressivity and its connection with income inequality (Piketty and Saez, 2007; Bachas et al., 2024), while theoretical work has examined optimal progressivity levels that balance efficiency and equity (Heathcote et al., 2017). These contributions, while invaluable, have been primarily descriptive or theoretical. Our work builds on these previous findings and contributes with the first causal estimate of the effects of a real-world progressive tax reform on tax compliance.

Second, our study contributes to the broad literature on social preferences, tax preferences, and redistribution preferences. There have been substantial advances in understanding perceptions and support of progressivity and their implications (Kuziemko et al., 2015; Ballard-Rosa et al., 2017; Stantcheva, 2021; Hoy, 2025), largely based on survey data. For example, Stantcheva (2021) showed that educational videos on redistribution increase support for progressive taxes, while efficiency-focused videos have no effect. Similarly, Hoy (2025) found that self-reported tax morale increases when individuals learn about the progressive nature of their tax systems. Although valuable, these studies rely on stated preferences, which may be subject to social desirability and other biases, for example, if rich households express support for higher taxes but behave differently when stakes are real.[8] We complement this literature by examining support for progressive taxation through revealed preferences in a natural, high-stakes context. Our results highlight important discrepancies between stated and revealed preferences: while middle and

[8]For more discussion of the generalizability of data on social preferences, see Epper et al. (2024).

rich households report higher perceived fairness after learning about the progressive reform, this does not translate into increased compliance. In contrast, poor households show consistency between their survey responses and behavior—both their stated fairness perceptions and tax compliance increase upon learning about the reform.

Lastly, our findings on the cross-rate effects of progressive reform contribute to the growing body of evidence on tax morale (Luttmer and Singhal, 2014; Slemrod, 2019). For example, Besley et al. (2023) provides evidence that the United Kingdom's regressive poll tax increased tax evasion. Similarly, Cullen et al. (2021) finds that tax compliance is higher when taxpayers trust the government in power, while Giaccobasso et al. (2022) shows that tax compliance increases when taxpayers believe they benefit from government spending.[9] We contribute to this literature by identifying a new tax morale channel—preferences for progressivity—and by estimating both own-rate and cross-rate effects within a unified framework.

The rest of the paper proceeds as follows. Section 2 describes the institutional context and data. Section 3 discusses the own-rate effects, while Section 4 discusses the cross-rate effects. Section 5 presents the counterfactual analysis. The last section concludes.

2 Institutional Context and Data

2.1 The Property Tax

Our study focuses on Tres de Febrero, a major urban municipality in the Greater Buenos Aires metropolitan area, with approximately 365,000 residents as of 2022. Tres de Febrero levies a local property tax known as *Tasa por Servicios Generales* (TSG), applied to around 96,000 residential properties and 18,000 commercial properties. Although our sample includes some commercial properties, the vast majority (84%) are residential, so we refer to taxpayers as "households" for brevity.[10]

Tax revenues are used to provide local public services such as street lighting, urban sanitation, and maintenance. This tax represents the standard revenue mechanism in Argentine municipalities, acting as their main funding source.[11] For our analysis, we have access to monthly administrative data from the municipality's tax records.[12]

[9] Among other examples, some studies provide suggestive evidence on reciprocal fairness– that is, taxpayers are more willing to pay taxes when they believe others are contributing their fair share (Nathan et al., 2024; Hallsworth et al., 2017; Del Carpio, 2014).

[10] Although property owners are legally responsible for the tax, it is customary for tenants to pay the bill. Therefore, we expect that most letter recipients are the ones who actually bear the tax burden.

[11] In Tres de Febrero, the TSG comprised 20% of total resources and 45% of locally generated revenue in 2021. These proportions roughly align with the contribution of real estate property taxes to state and local revenues in the United States (Saez and Zucman, 2019b).

[12] We exclude from the analysis 22,935 properties without property value assessments and 142 households

We illustrate the tax schedule by means of statistics for the pre-reform year, 2022 (see Appendix A.1 for further details). The average property tax bill was AR$ 32,561.1 per year in current Argentine pesos, equivalent to 529 U.S. dollars.[13] Throughout the remainder of the document, unless explicitly stated otherwise, all monetary amounts are expressed in current Argentine pesos. On average, in 2022 households paid an annual tax rate of 2.6% of the assessed value of the property. The provincial tax authority's cadastre determines property assessments, but these values systematically underestimate the real market values, so the municipality uses relatively high statutory rates to compensate. Calculating the tax rates relative to the market values (instead of assessed values) results in an average tax rate of about 0.9%, which is similar to the average property tax rate in the United States, and close to the average rates in Missouri (0.82%) and Maryland (0.96%) (Tax Foundation, 2024).[14]

This property tax consists of two components: a variable part, calculated as a percentage of the property's assessed value (with tax rates ranging from 0.42% to 3.22% across eight property categories), and a fixed component, earmarked for specific services such as security and health.[15] For reference, for a household with a median-value home, about 62% of the total tax burden comes from the variable component, while the remaining 38% is attributed to the fixed component. Although the variable part is progressive, the flat charge makes the overall tax schedule somewhat regressive. For example, a property in the 25th percentile of the assessed value distribution faced an effective tax rate of 2.47%, compared to 1.99% for a property in the 75th percentile.

2.2 Tax Compliance

Local tax agencies in developing countries face significant challenges in enforcing property taxes due to constraints in human, technical, and legal resources. As a result, enforcement capacity is weaker, leading to lower compliance rates and higher tax delinquency compared to developed economies. For example, in Dallas County, Texas, a municipality in a developed country, only 0.42% of households had delinquent property taxes; in contrast, studies from Argentina, Brazil, Haiti, and Malawi report that more than 50% of households did not pay property taxes on time (Nathan et al., 2024). Even in developed countries, tax enforcement can be imperfect in some contexts, leading to low rates of tax compliance. For example, in the United States, while the 40 largest U.S. cities collect

with incomplete tax compliance records.

[13] This conversion is based on the USD PPP exchange rate of AR$ 61.5 for 2022 (World Bank, 2025).

[14] Using proprietary market data, we estimate that assessed values in Tres de Febrero represent approximately 29.54% of their market values. The under-assessment, in percent terms, is quite similar in magnitude across the distribution of assessed values, so it does not make the schedule any more or less progressive.

[15] Although the tax reform applied uniformly across property types, the tax structure differs between residential and commercial properties – see Appendix A.1 for more details.

nearly 95% of property taxes on time (Chirico et al., 2016), compliance with income taxes can be substantially lower: the IRS estimates that individuals under-report 63% of income without third-party reporting, such as self-employment earnings (Cullen et al., 2021).

In Tres de Febrero, property taxes are billed monthly, with each installment due by the 15th of the respective month.[16] Our primary outcome of interest is a binary variable that indicates whether a household made a tax payment within three months of the due date. This definition of compliance is consistent with related studies (Del Carpio, 2014; Castro and Scartascini, 2015; Carrillo et al., 2021; Cruces et al., 2025; Flores et al., 2025), although the results are robust to alternative definitions. In the pre-reform year (2022), the average household made timely payments in 47.9% of the months. This average compliance rate masks substantial heterogeneity between households. Because tax enforcement is imperfect, payment behavior offers a window into individual tax morale and payment capacity. At one extreme, 42.3% of taxpayers did not make any on-time payments throughout the year – some households, in fact, go years without paying any property tax bills at all. At the other extreme, 26.7% of households paid on time every month. The remaining households made timely payments in some months but not in others. In sum, there is plenty of room for both upward and downward adjustments in compliance.

2.3 The Progressive Tax Reform

Prompted by political and equity considerations, the municipality made the property tax more progressive. The main reform to the tax schedule took effect in January 2023, modifying the variable component of the tax while keeping the fixed component unchanged, with the goal of maintaining revenue neutrality. The government implemented this reform swiftly, limiting advertising beyond the experiment. For instance, the municipal council discussed and passed the 2023 tax reform on December 5, 2022, published it in the annual tax bill on December 13, 2022, and the new rates became effective on January 1, 2023.[17] Despite the lack of public discussion on the reform, results from a taxpayer survey we conducted in 2023 indicate that most respondents, even among the rich, supported a tax reform that would lower taxes for the poor and raise them for the rich (see Appendix D.3 for more details). This suggests a relatively favorable context for a progressive reform, assuming respondents answer these types of survey questions honestly.

[16] A portion of the property tax is collected in advance through the electricity bill, as a fixed charge. This mechanism increases compliance by ensuring that even non-compliers contribute partially to the tax, given that nearly all households pay their electricity bills. Although it is technically possible to request the removal of this charge from the electricity bill, very few households do so. In 2023, 98% of the households paid at least part of their property tax through this mechanism, highlighting its high compliance rate.

[17] Source: link. Likewise, the 2024 reform was also approved shortly before it became effective.

The reform established four taxpayer groups based on sharp, binding thresholds of assessed property values: properties valued at or below AR$ 750K (in the bottom 37% of the value distribution) received a tax cut equal to a 30% reduction in the variable component. Properties valued between AR$ 750K and AR$ 1.5M (the middle 36% of the distribution) were not affected by the reform. Properties valued above AR$ 1.5M (the top 27% of the distribution) experienced a tax hike. More precisely, the properties assessed between AR$ 1.5M and AR$ 3M faced a 10% surcharge in the variable component, while the properties assessed above AR$ 3M experienced a 20% increase in the variable component. These cutoffs were exclusive to this reform and not used in other policies. We should note that when estimating own-rate effects, given the small share of properties above the AR$ 3M mark (around 6%), we lack sufficient statistical power to analyze the discontinuity at that threshold, so we focus on the AR$ 1.5M threshold instead.

In January 2024, the municipality further increased the progressivity of the property tax, offering a unique opportunity to validate our findings.[18] The 2024 reform maintained the same eligibility thresholds as in 2023, but it strengthened the reform further: in addition to the existing changes to the variable components, they made the fixed component more progressive as well. More precisely, the 2024 reform transformed the previously uniform fixed component into a group-specific charge, making it progressive. Properties valued at or below AR$ 750K received a reduction in the fixed component of approximately 16%. Properties assessed between AR$ 1.5M and AR$ 3M faced a surcharge in the fixed component of approximately 39%, while properties valued above AR$ 3M experienced a surcharge of approximately 64%. And, like in the 2023 reform, properties valued between AR$ 750K and AR$ 1.5M (the middle 36% of the distribution) were not affected by the 2024 reform.

3 Own-Rate Effects

3.1 Research Design

In this section, we estimate how the changes to the taxpayers' own tax rates affect tax compliance in the context of a progressive tax reform. We estimate these own-rate effects by combining two sources of identification. First, we take advantage of the sharp discontinuities in tax rates induced by the reform as a function of the property valuation brackets. Second, we exploit the timing of the reform by comparing the outcomes before and after its implementation. Since we leverage threshold discontinuities and temporal changes around the implementation date, our research design resembles a difference-in-

[18] An earlier reform occurred in 2022, but we do not study it due to its smaller scope, lack of public awareness, and reliance on outdated valuations—for more details see Appendix A.2.

discontinuity approach.

A distinctive feature of our approach is that we apply a regression discontinuity framework to an outcome in changes rather than levels. In what we labeled the first stage, we examine how tax rates shifted from one year to the next around these thresholds, establishing that the changes induced by the reform were indeed binding. More specifically, we calculate this tax rate as the annual tax liability divided by the property valuation. In the subsequent reduced-form analysis, we study how compliance rates changed around those thresholds. More precisely, we define the outcome of interest as the change in the proportion of bills paid timely (i.e., within three months of the due date) between the pre-reform and post-reform semesters.[19,20] We estimate the RDD parameters using nonparametric local-linear regression with an optimal mean squared error bandwidth (Calonico et al., 2014).

Our approach of using changes in outcomes rather than levels is key for the replication exercise of the 2024 reform. Since this reform used the same eligibility thresholds as the 2023 reform, by focusing on changes rather than levels, we isolate the incremental effect of each reform rather than the cumulative effects of previous reforms. Focusing on changes rather than levels is also crucial for the falsification test based on a placebo implementation date, which we discuss below.

3.2 Main Results

In this section, we present our estimates of own-rate effects for the two relevant comparisons around the thresholds—between the poor and the middle, and between the middle and the rich. We first establish the presence of a first-stage effect—that is, whether the reform led to a sharp change in tax rates around the thresholds. We then estimate whether tax compliance also changed around these thresholds, which would constitute evidence of an own-rate effect on tax compliance.

Figure 1 presents our main own-rate results. Panels (a) and (c) focus on poor households. Panel (a) shows the first-stage results, showing how the reform altered tax rates. The x-axis corresponds to the assessed value of the properties, which serves as the running variable for the RDD. The y-axis in panel (a) corresponds to the change in tax rates after the reform. As expected, there is a sharp discontinuity at the AR$ 750K threshold: poor

[19] For instance, if a property paid four bills in the last semester of 2022, and then paid five bills in the first semester of 2023, her outcome would be 0.167 ($= \frac{5}{6} - \frac{4}{6}$).

[20] For October, November, and December 2022, we restricted our binary variable that indicates whether a household made a tax payment by not allowing payments of a pre-treatment installment to be made in the post-treatment period. In cases where such payments were made after the post-treatment period, their values were set to zero. This correction is made to prevent pre-treatment outcomes from being contaminated by post-treatment choices. In any case, this is a minor detail, as the results remain almost identical without this correction.

households (left of the cutoff) experienced a 0.392 pp reduction in their tax rate (p-value < 0.001). Panel (c) presents the corresponding reduced-form effect, where the y-axis corresponds to the change in tax compliance. Following the tax cut for the poor, compliance increased by 2.718 pp (p-value = 0.002) for this group compared to the middle (around the threshold). This indicates the presence of a strongly significant own-rate effect for the poor, as they increased their tax compliance following a reduction in their tax rate.

Panels (b) and (d) present the corresponding results for rich households. Panel (b) shows that, as expected, the changes in the reform were binding and rich households (those to the right of the AR$ 1.5M threshold) faced a 0.146 pp increase in their tax rate (p-value < 0.001). Panel (d) then presents the behavioral response–the change in tax compliance measured in the y-axis. Rich households responded to the tax increase by reducing compliance by 2.248 pp (p-value = 0.050). The estimates for poor and rich households provide compelling evidence that own-rate effects are substantial and symmetric: lower taxes increase compliance, while higher taxes reduce it.

These RDD results are robust to a series of placebo tests that support the validity of our identification strategy. Figure 2 presents the results of a placebo date test in which we replicate the analysis above assuming a hypothetical, nonexistent reform in mid-2023. Since no tax reform occurred in June 2023, we should not observe any effects on tax rates or compliance. Panels (a) and (b) show the reduced-form estimates for poor and rich households, respectively. As expected, the estimated discontinuities at the placebo threshold are close to zero and statistically insignificant ($\beta^{own}_{poor} = -0.074$, p-value = 0.907; $\beta^{own}_{rich} = 0.046$, p-value = 0.827), confirming that our results are not driven by unrelated temporal trends. This placebo test also confirms that the assessed property value thresholds were not applied to other policies, ensuring that any observed discontinuities in tax compliance result from the reform itself rather than other factors.

On the other hand, Figure 3 presents the results of a further placebo test where we re-estimate the RDD using placebo thresholds instead: we arbitrarily set the thresholds at AR$ 500K and AR$ 2M (instead of the true AR$ 750K and AR$ 1.5M values). If our main findings were spurious rather than a true causal effect, we would expect to see significant discontinuities at or around other thresholds. However, panels (a) and (b) show that the estimated jumps in tax rates at these points are very small and statistically indistinguishable from zero ($\beta^{own}_{poor} = 0.003$, p-value = 0.592; $\beta^{own}_{rich} = -0.002$, p-value = 0.652). Similarly, panels (c) and (d) show no significant effects on tax compliance ($\beta^{own}_{poor} = -1.612$, p-value = 0.274; $\beta^{own}_{rich} = -1.596$, p-value = 0.371), reinforcing the validity of our main estimates.

Taken together, these placebo tests provide strong evidence that our findings are not driven by pre-existing trends or confounding factors. The absence of significant effects on the placebo date and placebo threshold tests confirms that the observed changes in

tax compliance can be attributed to the progressive tax reform. Furthermore, we conducted RDD manipulation tests around the cutoffs and found no evidence of relevant discontinuities in the density of the running variable (see Appendix B.1 for more details).

Finally, we performed several robustness checks to validate our preferred specification. We observe consistent patterns when including additional pre-reform controls, when using a uniform kernel, when including only residential properties in the analysis (i.e., excluding commercial properties), when redefining the outcome variable (for instance, including the possibility of paying 6 months after the due date instead of only 3), or when including in the sample only those households which received a control or a treatment letter, respectively. Appendix C presents and discusses these additional results in detail.

3.3 Own-Rate Elasticities

The previous section presented and discussed estimates of the tax reform's own-rate effects on tax compliance. The changes in taxes and in subsequent behavior around the thresholds allow us to assess the magnitude of the own-rate effects by expressing them in terms of behavioral elasticities—that is, by quantifying the change in behavior as a function of the change in one's tax rate.

Let β_j^{own} represent the reduced-form effect on compliance for group j, where $j \in \{poor, rich\}$, obtained from the RDD. Let α_j denote the first-stage effect of the reform on tax rates for group j. The compliance levels and tax rates for middle households (used as the baseline) are denoted by C_j^{own} and τ_j, respectively. Both elasticities are expressed in terms of changes from six months to six months, comparing compliance and tax rates before and after the reform. Specifically, the percentage change in compliance is derived from the reduced-form estimates (β_j^{own}), while the percentage change in tax rates comes from the first stage (α_j). We establish baseline compliance and tax rates using 2023 levels for middle households, focusing on middle properties situated near the thresholds – above for poor households and below for rich households. The results are presented below, with 90% confidence intervals shown in brackets:[21]

$$\varepsilon_{poor}^{own} = \frac{\frac{\beta_{poor}^{own}}{C_{poor}^{own}}}{\frac{\alpha_{poor}}{\tau_{poor}}} = \frac{\frac{-2.718}{60.1}}{\frac{0.392}{2.221}} = \underset{[-0.388;-0.103]}{\text{-0.257}} \tag{1}$$

$$\varepsilon_{rich}^{own} = \frac{\frac{\beta_{rich}^{own}}{C_{rich}^{own}}}{\frac{\alpha_{rich}}{\tau_{rich}}} = \frac{\frac{-2.248}{58.3}}{\frac{0.146}{1.837}} = \underset{[-0.906;-0.023]}{\text{-0.484}} \tag{2}$$

We estimate a significant own-rate elasticity for poor households of -0.257, indicating

[21] We calculated confidence intervals using 5,000 bootstrap iterations.

that for every 1% decrease in their tax rate, compliance increases by 0.257%. Similarly, the own-rate elasticity for rich households is -0.484, implying that for every 1% increase in their tax rate, compliance decreases by 0.484%. The elasticities are stronger for the rich (-0.484) than for the poor (-0.257), suggesting that households may be more inclined to stop paying taxes after a tax hike than to start paying them after a tax cut. However, we must take this result with a grain of salt, as the difference between these two elasticities is statistically insignificant (p-value = 0.402).[22]

In summary, our estimates indicate substantial changes in tax compliance behavior among both poor and rich households when faced with changes in their tax rates. Compliance increases as taxes decrease, and vice versa. The rich appear to respond more strongly than the poor, although these differences are not statistically significant. Although we have concentrated so far on the direct effects of each group's own rate changes on their compliance, in Section 4 we examine whether there is an indirect effect of changes in other groups' tax rates on the own group compliance.

3.4 Replication: The 2024 Reform

The local government implemented a further progressive reform in 2024, this time targeting the fixed component of the tax rather than the variable one. Thus, we can replicate our own-rate analysis of the 2023 reform using data from the 2024 reform. One limitation of the 2024 analysis is that we only have two months of post-treatment data. However, this is not a substantial limitation, as two months of post-treatment data provide more than sufficient statistical power to detect meaningful effects.

The own-rate effects from the 2024 reform are presented in Appendix F.2 and summarized below. In terms of sign and statistical significance, the results for the 2024 reform are consistent with those of the 2023 reform. Poor households respond to the tax reduction by increasing their compliance, whereas rich households respond to the tax increase by reducing their compliance. Notably, the own-rate elasticities are nearly identical between the two years: for poor households the estimates are -0.257 for 2023 and -0.288 for 2024, while for rich households the estimates are -0.484 for 2023 and -0.471 for 2024.

[22] We calculated the p-value of the difference by conducting 5,000 bootstrap iterations.

4 Cross-Rate Effects

4.1 Experimental Design

In this section, we seek to estimate how changes to the tax rates of others affect one's own perceptions of fairness and tax compliance in the context of a progressive tax reform. With this goal, we conducted a natural field experiment.

In the context of the start of the tax year and in close collaboration with the research team, the municipality sent a personalized letter to taxpayers in January 2023. Our main sample includes approximately 92,000 taxpayers who received a letter.[23,24] These letters included personalized information to each account holder, such as the assessed value of the property, a monthly breakdown of the tax amount due, their expiration dates, and payment options. This level of detail ensured that the recipients had a clear understanding of their financial obligations. Appendix I.1 shows an example of a letter sent to a household.

All participating households received their usual tax bill. Both treatment and control letters included on the front page the exact peso amount by which the recipient's own tax amount changed, under the "progressivity correction" line (see the example in Appendix I.1). The experimental messages were embedded on the second page. All letters contained a broad statement about the implementation of a tax reform aimed at enhancing equity and specified whether the recipient's *own* tax rate was reduced, increased, or remained unchanged. However, while the control letters did not provide further information about how the reform affected other groups' tax rates, the treatment letters included an infographic and accompanying text that showed how the reform affected tax rates across property valuation brackets (poor, middle, and rich households) and by how much. Panel (a) of Figure 4 shows an example of the message received by poor households in the control group, while panel (b) shows the corresponding message for the treatment group.[25]

Since it was an official government communication (as opposed to a letter from researchers), the municipality had full discretion over how to convey the reform. The municipality prioritized a simple and easy-to-understand message that was both non-deceptive and technically accurate. In the treatment message, the municipality communicated a discount of 30% for poor households and a surcharge of 15% for rich households. This

[23] We exclude 3,568 taxpayers who made up-front payments for 2023 before the letters were sent, as their compliance decisions had already been made. Additionally, we exclude 12,689 taxpayers with a registered email address from the main sample, as they constitute a separate survey sample. As discussed in the following section, the results remain consistent when they are included.

[24] This was the only bill of 2023 delivered in paper format. The municipality sent additional communications during that year, but only digitally to the subset of taxpayers with a registered email address.

[25] For sample letters sent to middle and rich households, see Appendix I.1.

message conveys the essence of the reform and some of this detail, but it is built on two simplifications. First, the figures communicated by the municipality correspond to the actual reform – changes to the variable component of the tax. However, due to the existence of a fixed component, the actual discount and surcharges on the total tax liability were somewhat smaller than those described in the message.[26] Second, the surcharge for the rich contained two levels. The 15% surcharge for rich households mentioned in the letter was computed as an unweighted average of the change for properties valued between AR$ 1.5M and AR$ 3M (facing a 10% surcharge) and properties valued above AR$ 3M (facing a 20% surcharge).

Since the reform was not widely discussed publicly or advertised in the media, and it was approved and implemented in a relatively short period of time, most taxpayers were probably not aware of it or of its details. This view is consistent with the survey data discussed in the results section below, according to which only a minority of subjects in the control group reported being aware of this reform. We expect our information treatment to have increased awareness and understanding of the reform, particularly among a largely uninformed public. At a minimum, it should have increased the salience of the reform for those already aware of it. Through greater awareness, learning, or salience, this detailed explanation of the reforms progressive nature may have influenced perceptions of fairness and tax compliance relative to the control group.

Because subjects in different groups (poor, middle, and rich) were impacted differently by the reform, we want to study the treatment effects separately for each group. For that reason, we stratified the randomization across the three property valuation groups. The results reported in Appendix C show that, consistent with random assignment, the treatment arms are balanced in observable characteristics. In addition, a small group of taxpayers (5,913) was randomly selected to receive no letters. As shown in prior research (Cruces et al., 2025), if read, these letters are expected to increase compliance by serving as a simple reminder to pay taxes and providing useful payment information. Comparing the control letter group with the no-letter group offers a straightforward way to validate whether taxpayers were opening the correspondence.[27]

4.2 Complementary Taxpayer Survey

The main outcome of interest of the experimental design is the taxpayer's tax compliance choices, sourced from administrative records. Additionally, we conducted a post-treatment

[26] For example, properties in the bottom 37% of the value distribution (poor households) saw a median tax reduction of about 13.2%, while those in the top 27% (rich households) experienced a median tax surcharge of approximately 9.9% – see Appendix A.1 for more details.

[27] For the experimental analysis, we excluded from the sample individuals who had already made payments in 2023 before the letters were sent, as they had already made their choices and could not be influenced by the letter.

survey of taxpayers to complement the results and to shed light on the potential underlying mechanisms. Of the original population of 92,000 in our experimental sample, we set aside a group of 14,060 households with email addresses registered in the municipality database—called the *email sample*. As part of the main experiment, households in this sample were assigned to treatment and control groups and received the respective informational letters in January 2023. Additionally, only for this group, we emailed an invitation to complete a survey about municipal taxes and services 45 days after the original mailing. We thus sent 14,060 emails inviting taxpayers to participate, providing a short description of the study and a direct survey link. Because taxpayers invited to the survey receive additional information (both in the body of the invitation email and in the survey) and questions related to the reform (as described below), as well as further reminders by email, we exclude them from the main experimental sample in our analysis.[28]

Although the email sample is similar to the universe of taxpayers in many dimensions, there are some notable exemptions – for example, their compliance rates in 2022 are significantly higher.[29] In any case, the survey was designed to test some general drivers behind the main results, and we do not necessarily require this sample to be fully representative of the universe of taxpayers. Importantly, including this group in our main experimental estimation of the treatment effects on tax compliance yields consistent results (for more information, see Appendix B.2).

This survey was conducted between February and April 2023. Potential respondents received a first email on February 23, 2023, and a reminder on March 10, 2023. The response rate was 13.2%, which is high compared to benchmarks in similar survey-based tax compliance studies.[30] After excluding a small number of households who did not have property value assessments, we end up with a final sample of 1,732 survey responses. Table C.5 indicates that treatment and control groups are balanced in their observable characteristics even within the sample of survey respondents. In addition, there are no significant differences in the response rate to the survey between treated and control households.

The first goal of this exercise was to establish whether the treatment message had the expected effects on awareness of the reform. The second goal was to measure the effects (if any) on perceived tax fairness and other attitudes. Appendix J.2 presents the complete survey instrument, which we describe below. An important detail is that the survey took place significantly later than the letter: the median respondent completed the survey 52

[28] We have also excluded them in our control versus no letter estimates in Appendix D.2.

[29] The average compliance in the control group is 74.1% in the email sample compared to 47.9% in the main sample. While the email sample shows higher compliance rates, most of the other baseline characteristics are fairly similar between groups. For more details, see Appendix E.

[30] For example, Bergolo et al. (2020) report response rates of 8.9% in an online survey of taxpayers in Uruguay who received email invitations similar to ours.

days after they received the letter. Drawing on existing literature that documents the decay of information treatment effects over time (e.g. Cavallo et al., 2017; Bottan and Perez-Truglia, 2022), we were concerned that the treatment effects could have dissipated. For that reason, we reinforced the message by showing it to subjects in both the email invitation and on the second page of the survey.[31]

The participants were then asked a question about their awareness about the implementation of different recent municipal policies. Specifically, we asked them if they were aware of a measure that offered "discounts for low-valuation properties and additional charges for high-valuation properties" in the local property tax. This information was provided in the treatment message but not in the control letter, so we would expect different responses from the treatment and control groups. Respondents were also asked to rate the fairness of the municipal tax system on a 0–10 scale. Finally, to capture general preferences for redistribution, we included an adaptation of the General Social Survey question on agreement with the role of government in reducing the gap between the rich and the poor. As discussed below, we use this question as a falsification test: since it allegedly captures deeply rooted general preferences and not views on a specific policy, we should not expect our informational treatments to affect this response. We discuss these results in the following section.

4.3 Experimental Results: Reform Awareness and Perceptions

The validity of mailing experiments such as ours requires that households open and engage with the letters. In an extreme case, if taxpayers had not opened the letters, they would not have been exposed to our information treatment at all. We begin by presenting evidence that a significant share of subjects opened the letters and paid attention to their content. We first compare tax compliance between households that received a control letter and a small fraction that was randomly assigned to receive no letter at all. The results, presented in Appendix D.2 and summarized here, show that taxpayers with control letters consistently and substantially increased their tax compliance relative to households that received no letter at all. Overall, the results from this auxiliary experiment suggest that a significant share of taxpayers opened the letters we sent.[32] Importantly, this finding is consistent across poor, middle-income, and rich households, meaning that all three groups were equally likely to open the correspondence.

Next, we investigate whether the treatment message, compared to the control message,

[31] For a sample of an email invitation of a household in the treatment group, see Appendix J.1.

[32] The average difference of about 5 pp aligns with findings from a recent systematic review of similar experiments (Antinyan and Asatryan, 2024). It is worth noting that experiments of this type often find large effects because they reflect a substantial reminder effect of receiving a letter, beyond the content of the letter itself. In contrast, our experiment produces smaller effects because it compares subtle variations between two letter types, effectively netting out the basic reminder effect.

increased awareness of the reform as intended. In the survey, we asked respondents whether they knew about the reform. As depicted in panel (a) of Figure 5, only about 14.9% of the respondents in the control group indicate that they were aware of it, with relatively similar levels of knowledge among taxpayers in the three property valuation groups. This aligns with our prior expectation that taxpayer knowledge of the reform was low given the lack of public discussion about it. Panel (b) shows a consistent and significant treatment effect across all valuation groups (in gray) as well as separately for poor households (in blue), medium households (in green), and rich households (in red). Our information treatment significantly increased awareness of the reform in the three groups by about 8 pp on average.

The back of the tax bill letter also included information about two additional policies the Municipality had implemented—soft loans for businesses, and reduced paperwork for new businesses. We also asked respondents about their awareness of these policies in the survey. In contrast to the information about the reform's progressive nature (present only in treatment letters), both control and treatment letters included information about the business loans and paperwork policies. This allows us to use awareness of these latter policies as a falsification test.[33] The awareness about the business loans policy offers a simple falsification test of our information treatment (the results are very similar when considering the paperwork simplification policy). Panel (c) of Figure 5 indicates a relatively low level of knowledge about this policy among taxpayers in the control group, with only 12.4% stating that they knew about it (and evenly distributed between the property valuation groups). We test the specificity of our intervention by evaluating whether the information about the reform had some effect on awareness about this different policy, which would indicate the presence of social desirability bias or other sources of spurious results. Reassuringly, the results of the falsification test in Panel (d) of Figure 5 confirm that this was not the case. There are no treatment effects of information about the tax reform on participants' knowledge of this unrelated government program – the overall coefficient and those for the three groups are virtually zero. This absence of an effect on an unrelated policy supports our conclusion that the observed increase in tax reform awareness and any subsequent changes in behavior and stated perceptions can be directly attributed to our targeted information intervention, rather than to a general increase in policy awareness or other spurious effects.

Having established that taxpayers read and incorporated the information in our treatments, we can turn to a series of outcomes related to taxpayer perceptions of the tax system. The research design allows us to establish whether our intervention affected taxpayers' stated perceptions of the municipal tax system's fairness. Respondents were asked to assess the fairness of the distribution of the municipal tax rates between rich and poor

[33]This information was displayed at the top of the second page of the letters.

households on a scale from 0 to 10. Panel (a) of Figure 6 shows relatively high perceived levels of fairness reported by taxpayers in the control group, with an average stated perception of approximately 5.5, and evenly distributed between the three valuation groups. Interestingly, our information treatment increased this stated perception across the board, as depicted in panel (b) of Figure 6. There is a positive and significant treatment effect on stated perceptions in all valuation brackets of about 0.56 points on the scale 0–10, with fairly similar effects for poor, middle and rich households.[34]

However, priming or social desirability bias may explain these effects, with respondents receiving information about a progressive reform simply offering more positive responses. We test whether our treatment had any effect on the respondents' views on the role of government in reducing the gap between the rich and the poor, which allegedly represents deeply rooted general preferences. Reassuringly, the results in panel (d) of Figure 6 show that our information treatment did not influence respondents' redistribution preferences as captured by this question. This null effect supports our interpretation that the information treatment truly affected stated perceptions about specific taxes at the local level rather than a more general, or spurious, effect.

4.4 Experimental Results: Tax Compliance

The previous discussion indicates that the information treatment had an impact on taxpayers' awareness of the reform, among other results. We now turn to the other core outcome of our experimental setup: the cross-rate effects of the reform on actual taxpayer behavior, given by tax compliance from administrative records. Panel (a) of Figure 7 presents an event-study analysis of treatment effects on tax compliance (i.e., comparing tax payments between treatment and control households) at the most granular level, monthly payments. The outcome of interest is the probability of paying a tax bill no later than three months after the due date (see Section 2.2 for details). The results are estimated separately for poor, middle and rich households. The regression controls for pre-treatment household tax compliance and property valuation, and the estimates are clustered at the household level. The x-axis represents months before and after the intervention, while the y-axis measures treatment effects in percentage points.

Panel (a) of Figure 7 shows pre-reform levels and trends in tax compliance. These demonstrate successful random assignment: compliance is balanced between treatment and control groups within each valuation bracket, with only small and mostly non-significant differences. Regarding the post-treatment period, there is a clear positive and significant effect of the treatment on compliance for poor households, as witnessed

[34]These findings are aligned with previous work that has documented a general preference for progressive tax systems in both laboratory and survey settings (e.g., Durante et al. 2014; Stantcheva 2021; Hoy 2025).

by the six post-treatment coefficients around 0.5–1.0 pp. This indicates that the information on the progressive nature of the reform had a positive and significant effect on the poor's tax compliance over and above the direct own-rate effect estimated in the previous section.

The research design implies that the effect described here can be directly attributed to observing the tax changes for the three groups of the population. This does not seem to be the case for middle households: the differences in compliance between treated and controls are much smaller and not statistically significant at conventional levels. Finally, the results also indicate a negative effect for rich households, reaching approximately -0.5 pp and 5% statistical significance six months after treatment. In terms of the evolution of the effects, the patterns seen in Figure 7 suggest that the effects may gradually dissipate over time.[35] In contrast to the effects for poor households, there is a zero or slightly negative treatment effect for rich households, despite the positive treatment effect on their stated rating of the municipal tax system's fairness (more on this below).

To provide a more comprehensive view of our findings, we aggregated the monthly data into three-month periods before and after the treatment, while maintaining the distinction between valuation brackets. Panel (b) of Figure 7 presents the pooled 'Before Letter' and 'After Letter' coefficients. Again, the blue coefficient represents the pooled quarter estimate for poor households, indicating a positive effect of 0.808 percentage points of the treatment on tax compliance (significant at the 5% level) after the intervention, with a very precise null effect before our mailing. The green coefficient represents the effect for middle households, which is consistent with the pattern observed in the event study: there are no statistically significant differences in tax compliance between treated and control households before or after treatment. Finally, the estimates for rich households, displayed by a red coefficient, indicate a negative pooled estimate of -0.313 for the post-treatment period, though not significant at conventional levels. Pre-intervention, treated and control taxpayers reported similar compliance levels.

We conducted several robustness checks of these results. For example, we find similar patterns when we exclude from the regression control variables not related to pre-treatment payments, or when we do not include any control variables. The simple differences in tax compliance between treated and control households before and after the intervention for the three groups confirm the pattern in panel (a) of Figure 7 (see Appendix D.1 for details). We also find similar results when using an alternative sample of subjects (for instance, when including the email survey sample, or when including just residential properties and excluding commercial ones), when using an alternative defini-

[35] This finding aligns with existing literature correspondence experiments, showing that the impact of letters wanes over time, presumably because the information recedes from taxpayers' immediate memory (e.g., Bergolo et al., 2023). Also consistent with this interpretation, the gradual decline in treatment effects aligns with the results of the comparison between control letters and no letters.

tion of the outcome (allowing payments up to 6 months after the due date instead of only 3), or when excluding observations in property valuations below the 5th and above the 95th valuation percentiles. See Appendix B.2 for more details about these tests.

These results are all the more notable when taking into account that they are probably subject to substantial attenuation bias. The treatment consisted of an infographic and some text on the second page of the letter. It is likely that a significant share of households did not pay attention at all to the second page of the letter. While we provided evidence that a significant share of households were opening the letters, that does not necessarily mean that they read the letters carefully or fully. Thus, the effect of sending the information provision letters (0.808 pp for poor households, for instance) should be interpreted as an intention-to-treat (ITT) coefficient. The Average Treatment Effect on the Treated (ATET) is the ITT scaled by an adjustment factor that accounts for this imperfect treatment uptake. For taxpayers to be actually exposed to the informational treatment, the letters needed to arrive on time, be opened, read front and back, and recipients must have processed and understood the information conveyed. The recent literature on similar experiments estimates these effects for the United States, and use adjustment factors between 2 and 7.5 (Perez-Truglia and Cruces, 2017; Bottan and Perez-Truglia, 2025; Gerber et al., 2020; Nathan et al., 2020). Perez-Truglia and Cruces (2017) note that, according to the Environmental Protection Agency, nearly 50% of unsolicited mail is discarded unopened, determining a conservative non-uptake adjustment factor of 2.[36] This results in a lower bound of about 2 pp for the effect for the poor, a substantial impact on tax compliance.[37]

In summary, independently of any adjustment factors, our experimental results indicate that poor households increased when receiving information about the progressive nature of the reform, while rich households did not change their behavior (or slightly reduced their compliance at best). These effects of the information provided on tax compliance, which reflect actual behavior, contrast with those of the self-reported perceptions from the survey. The positive effect of the information treatment on the three groups' rating of the reform's fairness could have led us to expect a corresponding positive effect on compliance for middle and rich households similar to that observed for the poor. The effect of the information about the progressive nature of the reform on actual behavior thus appears to be mediated by how the reform affects households' own tax rates: poor

[36] This is a conservative approach, as the alternative approach using survey results yields a much higher adjustment factor–see Appendix D.4 for details.

[37] The own-rate effects (Section 3) could also be plausibly affected by experimental non-uptake. This occurs because not every taxpayer was necessarily aware of the change in their own tax liability. However, in the case of own-rate estimates, estimating the ATET is considerably more challenging and involves more assumptions. In particular, own-rate effects are potentially explained by a mix of a price effect (not affected by attenuation bias) and a tax morale effect (potentially affected), and thus estimating the scaling-up factor requires an assumption about the relative weight of each of the two. Therefore, we prefer not to report scaled-up (ATET) results for the own-rate effects.

households, who see their rates fall, respond to the information by further increasing their tax compliance (over and above the positive own-rate effect), while rich households, whose tax rates went up, pay the same or less. We compute these effects in terms of elasticities in the following section.

4.5 Cross-Rate Elasticities

The previous section presented and discussed estimates of the effects of our treatments on tax compliance. Because our informational treatments contained specific figures about the tax changes for each group, we can assess the magnitude of the cross-rate effects by expressing them in terms of behavioral elasticities—that is, by quantifying the change in compliance due to changes in others' tax rates.

Let β_j^{cross} represent the compliance effect for group j, obtained through the experimental results discussed above, where $j \in \{poor, rich\}$. Let C_j^{cross} denote the baseline compliance of each group in 2022,[38] and let $\frac{\Delta\tau_{-j}}{\tau_{-j}}$ represent the tax rate change for the opposite group, capturing the cross-group nature of the effect. For simplicity, we characterize the tax changes as a 30% decrease in the tax rate for poor households and a 15% increase for rich households, matching the information conveyed in the letters regarding the variable component of the tax.[39] The results are presented below, with 90% confidence intervals in brackets:[40]

$$\varepsilon_{poor}^{cross} = \frac{\frac{\beta_{poor}^{cross}}{C_{poor}^{cross}}}{\frac{\Delta\tau_{rich}}{\tau_{rich}}} = \frac{\frac{0.808}{55.2}}{0.15} = \underset{[0.040;0.158]}{0.098} \tag{3}$$

$$\varepsilon_{rich}^{cross} = \frac{\frac{\beta_{rich}^{cross}}{C_{rich}^{cross}}}{\frac{\Delta\tau_{poor}}{\tau_{poor}}} = \frac{\frac{-0.313}{54.8}}{\text{-0.3}} = \underset{[-0.018;0.055]}{0.019} \tag{4}$$

We estimate a significant cross-rate elasticity for poor households of 0.098, indicating that for each 1% increase in the tax rate of the rich households, poor households increase their compliance rate by 0.098% when they become aware of the change in the other group.[41] We also estimate a cross-rate elasticity for rich households of 0.019, although this is not statistically significant at conventional levels. These results are not scaled up

[38] The baseline compliance rates used as reference points are derived from the 2023 compliance rates of the control group for each valuation bracket.

[39] Our cross-rate elasticities are computed using the variable component in the denominator to maintain consistency and simplicity, as this was how the tax change was communicated in the treatment messages. Conversely, the denominator of the own-rate elasticities is based on the total tax.

[40] We calculated confidence intervals using 5,000 bootstrap iterations.

[41] Technically, this refers to a change in the variable component of the tax rate. However, to avoid overcomplicating the interpretation, we maintain the liability-based framework throughout the text.

for non-uptake and thus should be interpreted as ITT elasticities. Furthermore, using 5,000 bootstrap iterations, we show that the difference in elasticities between poor and rich households is statistically significant at the 10% level (p-value = 0.059).

This exercise reveals that the reaction to information about changes in others' taxes is substantial. To put the magnitude of the cross-rate elasticity in context, we compare it to the quasi-experimental estimates for the own-rate effect: a 1% increase in the tax rate of the rich has the same effect on the compliance of a poor household as lowering the poor household's own tax rate by 0.38% ($\varepsilon^{own}_{poor} \cdot 0.38\% = 0.098\%$). This indicates that these social factors should not be ignored when designing tax reforms. We attempt to quantify their relative importance in Section 5 below.

4.6 Replication: The 2024 Reform

Taking advantage of the new progressive reform of 2024, we conducted a second large-scale field experiment to assess whether the results of the 2023 experiment replicate. In addition to the changes to the variable components from the 2023 reform, the 2024 reform made the fixed component more progressive. Despite this difference, the municipality preferred to convey treatment messages similar to those of 2023, emphasizing that the new reform further reduced the tax for poor households, maintained it for middle households, and increased it for rich households. We re-randomized households into treatment and control letters within each of the property value groups.[42]

In addition to replicating our results, we took this opportunity to refine our experimental design by adjusting the 2024 control letter to make the difference between the information provided to the treatment and control groups even more subtle. One potential limitation of the 2023 control letter was that it displayed the magnitude of the progressivity discount for the household on the front page but not on the second page. The 2024 letters further minimized the distinction between treatment and control messages. In contrast to 2023, when only the treatment group received information by means of an infographic, in 2024 both groups received one. As in 2023, the infographic for the treatment group in 2024 informed the recipient of tax changes for the three groups (poor, middle and rich). In 2024, the control group also received a similar infographic, although it only featured one bar representing the magnitude of the tax change for the household's own group. For example, for poor households, the control message included an infographic with a single bar (indicating the discount for poor households), while the treatment message featured an infographic with three bars (showing the discount for poor households, the surcharge for rich households and no change for middle households). Examples of the 2024 experiment letters can be found in Appendix I.2.

[42]For details on the 2024 experimental design see Appendix F.1.

While the reform and letters were similar in 2023 and 2024, the broader environment differed. The 2024 reform and its letters were introduced during a turbulent economic and political period, in a presidential election year, which may have made the reform seem less significant relative to broader national events. Furthermore, the similarity between the reforms may have led some taxpayers in 2024 to misinterpret our communications as referring to the previous years reform, potentially weakening their response.

With these caveats in mind, we replicate our cross-rate analysis of the 2023 reform using data from the 2024 experiment. The complete set of results is presented in Appendices F.3 and F.4 and is summarized below. Consistent with the 2023 results, we again observe in 2024 a significant impact of the control letter relative to no letter. This suggests that despite the volatile environment, a substantial fraction of the subjects opened and paid attention to the letters. Most importantly, the difference in compliance between treatment and control letters in 2024 mirrors the results of the experiment in 2023. For poor households, we again find a significant treatment effect of 0.721 pp (p-value = 0.040), which is similar to and statistically indistinguishable from the corresponding effect of 0.808 pp (p-value = 0.006) estimated for the 2023 experiment. As in 2023, in the 2024 experiment we find a negative but statistically insignificant effect on rich households and an insignificant effect on middle households. The consistency of the findings between the two experiments, despite differences in the economic and political environment, underscores the robustness of our results.[43]

5 Counterfactual Analysis

5.1 Framework

In this section, we leverage our results to conduct a counterfactual analysis as a way to illustrate the policy implications of our framework, which incorporates the distinction between own- and cross-rate effects, and highlights the potential contribution of the latter to the standard analysis of behavioral responses to tax reforms. We consider the effects of a simplified hypothetical progressive reform that, similar to the actual reform that took place in January 2023, reduces the tax rate on poor households by some proportion $\%\Delta\tau_{poor}$ and increases the tax rate on rich households by $\%\Delta\tau_{rich}$. We calibrate these factors so that in a world without behavioral responses (i.e., with tax compliance held constant), the reform satisfies the following criteria: (i) it is revenue neutral; (ii) the rich pay an effective tax rate 0.5 pp higher than that of the poor. This outcome is achieved

[43] Due to a difference in the two reforms, there is a slight difference in definitions: in 2023 the cross-rate elasticities are computed using the variable component in the denominator, while in 2024 the denominator is based on the total tax.

by changing the tax rates by $\%\Delta\tau_{poor} = -45.5\%$ and $\%\Delta\tau_{rich} = +17.32\%$. We aim to understand the effects of this reform on effective tax progressivity and tax revenues under behavioral responses, and contrast it to a counterfactual scenario that ignores behavioral responses.[44]

For this hypothetical reform, we assume homogeneity within each property value bracket, allowing us to focus on average effects.[45] Let subscript $j \in \{poor, middle, rich\}$ denote poor, middle, and rich households. Let γ_j be the share of households in each of these three groups. Let superscript s denote the scenarios: $s = 0$ represents the status quo (i.e., before the reform), while $s = A$ is the *actual* post-reform scenario (accounting for behavioral responses) and $s = C$ is the *counterfactual* post-reform scenario without behavioral responses. Let $L_j^s > 0$ denote the average tax liability. For example, L_{poor}^0 is the average liability of poor households before the reform. Let $C_j^s \in (0, 1)$ denote the average tax compliance. For example, C_{poor}^0 is the average probability that a poor household pays its taxes in time before the reform. Lastly, let $T_j^s = L_j^s \cdot C_j^s$ be the amount of taxes paid on time. For this counterfactual analysis, we fix the values $\{\gamma_j, L_j^0, C_j^0\}\forall j$ to match the corresponding averages during the 12 months preceding the 2023 reform, from January through December 2022.[46]

The progressive reform affects taxes paid through two channels. The first is the mechanical channel: lowering tax rates for the poor reduces their tax liabilities, whereas increasing tax rates for the rich increases theirs. In the counterfactual scenario with no behavioral responses, this is the only channel at play. In the post-reform scenario with behavioral responses, there is an additional channel operating through compliance: i.e., keeping constant the new tax liabilities (L_j^s), the amount of taxes paid (T_j^s) varies due to changes in tax compliance (C_j^s). To compare the outcomes with and without behavioral responses, we adopt a sufficient statistic approach (Chetty, 2009). The behavioral responses can be summarized as a function of the four behavioral elasticities estimated above (ε_{poor}^{own}, ε_{rich}^{own}, $\varepsilon_{poor}^{cross}$ and $\varepsilon_{rich}^{cross}$):[47]

[44] Throughout this section, we use *effective* tax progressivity, which is different from *statutory* tax progressivity. The latter refers to the progression of tax rates as determined by law – essentially, how tax *liabilities* change with property values. Effective tax progressivity, on the contrary, reflects how tax *payments* change with property values, thus incorporating tax compliance. This distinction is critical to understanding the implications of tax reforms, as statutory progressivity does not always translate into effective progressivity.

[45] In the real reform, the impact of the reform on effective tax rates was heterogeneous because it applied only to the variable component– thus, in proportional terms, it affected some households more than others.

[46] In the real-world context of Tres de Febrero, tax compliance is effectively split into two components: nearly all households pay the fixed portion of the tax, largely due to its inclusion in the electricity bill (in 2023, the electricity bill mechanism covered nearly 90% of the fixed charge), while approximately half of households ultimately pay the variable component. However, for the sake of simplifying the counterfactual analysis, we assume a binary framework in which taxpayers pay the full amount or nothing at all.

[47] For the sake of simplicity, we are making a slight extrapolation assumption. In our empirical analysis,

$$C^A_{poor} = C^0_{poor} \cdot \left(1 + \varepsilon^{own}_{poor} \cdot \%\Delta\tau_{poor} + \varepsilon^{cross}_{poor} \cdot \%\Delta\tau_{rich}\right) \tag{5}$$

$$C^A_{rich} = C^0_{rich} \cdot \left(1 + \varepsilon^{own}_{rich} \cdot \%\Delta\tau_{rich} + \varepsilon^{cross}_{rich} \cdot \%\Delta\tau_{poor}\right) \tag{6}$$

Following the conservative estimates discussed in Section 4 above, we adjust the baseline cross-rate elasticities by a factor of two, to scale the intention-to-treat effects into treatment effects on the treated. With these uptake rates, we derive the two main outcomes of interest of the counterfactual exercise. The first outcome is the effective tax progressivity, given by the difference in tax rates between rich and poor households. The second outcome is per capita tax revenues.

Note that each C^A_j depends on the four behavioral elasticities. Since each of those elasticities is subject to sampling error, we use bootstrap for inference. Within each bootstrap sample, we estimate the four elasticities of interest and use those values to compute C^A_j following equations (5) and (6). We then use the distribution of the results across the bootstrap samples to calculate the 90% confidence intervals and p-values.

There are some implicit assumptions in this exercise that are worth noting. First, to simplify the exposition of the results, we assume that households in the middle property valuation group do not have behavioral responses.[48] Second, we are extrapolating the results from the own-rate elasticities. These elasticities are estimated by means of a regression discontinuity design and thus correspond to the local average treatment effects for households near the corresponding thresholds. We use these estimates for all taxpayers within each group, implicitly assuming that the effects estimated near the thresholds are good approximations for the average effects within each group.[49]

5.2 Results

The key results are summarized in Figure 8.[50] Panel (a) shows the results corresponding to the first outcome of interest: the effective progressivity of the tax. The x-axis displays various scenarios: "Pre-Reform" (baseline), "Post-Reform without BR" (assuming no behavioral responses), "Post-Reform BR from poor" (behavioral responses only from poor households), "Post-Reform BR from rich" (behavioral responses only from rich house-

the cross-rate elasticity is defined with respect to the variable component. For this counterfactual analysis, we are using this elasticity as our best approximation of the elasticity with respect to the total tax.

[48] For the own-rate channel, this is true by construction since the tax rates for this group were not affected by the reform and thus their compliance should not change either. The experimental estimates of the cross-rate effects for middle households were small and not statistically significant, so we assume that there is no behavioral response through this channel.

[49] Additionally, for the cross-rate elasticity, we are implicitly assuming that there is no treatment heterogeneity with respect to the amount owed.

[50] For more detailed results, see Appendix G.2.

holds) and "Post-Reform BR from poor and rich" (responses from both groups). The key takeaway from this figure is that a reform initially intended to be progressive may appear less so once behavioral responses are considered. The first bar from panel (a) shows that before the reform, rich taxpayers faced, on average, an effective tax rate that was 0.31 pp *lower* than that of poor taxpayers–more precisely, the rich paid an effective rate of 1.07% while the poor paid an effective rate of 1.38%. These effective rates indicate a slightly regressive tax system. The second bar shows that, ignoring behavioral responses, the reform would have made taxes significantly more progressive, with rich taxpayers paying an effective tax rate 0.5 pp *higher* than that of poor households. More precisely, rich households pay an effective tax rate of 1.25% versus 0.75% for poor households.

However, these previous results change substantially when behavioral responses are included. The third bar incorporates the behavioral responses of poor households only.[51] Since taxpayers in this group react by increasing their compliance, they end up paying more in taxes, narrowing the gap between the rich and the poor. More precisely, the behavioral responses of the poor *reduce* the effective tax progressivity by 0.113 pp compared to the situation without the behavioral responses of this group. This effect is precisely estimated and highly statistically significant (p-value<0.001). The fourth bar from panel (b) shows our results on tax progressivity when we incorporate the behavioral responses from the rich only. Since taxpayers in this group actually reduce their compliance, the total taxes paid by this group fall, thus lowering effective tax progressivity. In magnitude, the effect of the behavioral responses from the rich is slightly larger than the corresponding effect from the poor, reducing the effective tax progressivity by 0.127 pp (p-value = 0.048). The fifth and final bar incorporates the behavioral responses from both groups simultaneously. Since both effects reduce progressivity, the net result is a 0.240 pp reduction in effective tax progressivity compared to the scenario without behavioral responses (p-value = 0.002). In other words, due to behavioral responses, the effective progressivity is 48% lower than it would have been without these responses.

Panel (b) of Figure 8 mirrors panel (a) but focuses on per capita revenues instead of effective tax progressivity. Each bar shows the per-capita revenue under the different scenarios. The first bar from panel (b) shows that before the reform, the per capita revenue was AR$ 12,843. The second bar, corresponding to the post-reform without behavioral responses ($s = C$), shows the same per capita revenue as in the pre-reform scenario. This is by construction, as we calibrated the reform to be revenue-neutral under no behavioral responses. The third bar shows the per capita revenues after accounting for behavioral responses from the poor only. Since poor households increase compliance, this channel generates AR$ 201 in additional per capita revenue, an effect that is highly

[51] In practice, this operates by simply taking equation (5) and setting the behavioral elasticities corresponding to rich taxpayers to zero.

statistically significant (p-value < 0.001). In contrast, the fourth bar incorporates the behavioral responses from rich taxpayers only. Since this group lowers its compliance, their responses reduce tax revenue by AR$ 763 (p-value $= 0.048$). The fifth and final bar shows the results when allowing for behavioral responses from taxpayers in both groups. Since the two behavioral responses move in opposite directions, the net impact depends on which effect dominates. Here, the negative effect prevails, meaning that, despite the reform being designed to be revenue-neutral, behavioral responses reduce tax revenue. However, the direction and magnitude of this result should be interpreted cautiously, as it is borderline insignificant (p-value $= 0.145$).

Lastly, the results above distinguish between the behavioral responses of rich and poor households. Additionally, these responses can be further broken down into cross-rate and own-rate components. These additional results are presented in Appendix G.1.

6 Conclusions

Tax progressivity is a cornerstone of modern fiscal policy, widely adopted to reduce inequality and foster social equity. Although the redistributive effects of progressive taxation are well-documented, empirical evidence on its behavioral impacts–particularly regarding tax compliance–remains limited. This paper fills this gap by examining how progressive taxation shapes compliance behavior, combining quasi-experimental and experimental approaches in the context of a municipal property tax reform in Argentina.

Our analysis reveals asymmetric responses to tax rate changes across valuation groups: reductions in tax rates for poor households significantly increase compliance, while increases for rich households result in decreased compliance. These effects are amplified by social effects: when informed about tax increases on wealthy households, lower-valuation taxpayers exhibit further improvements in compliance, along with more favorable stated perceptions of tax fairness. These findings demonstrate that behavioral responses to tax progressivity extend beyond direct own-rate effects, suggesting that taxpayer decisions are shaped not only by pecuniary incentives but also by true perceptions of distributional equity within the tax system. In contrast, while middle and rich households also state improved perceptions of fairness under the progressive scheme, their compliance rates show, if anything, a negative response. The contrast between the stated and behavioral effects for rich and middle households serves as a cautionary tale of how stated and revealed preferences can diverge. Individuals may state that they prefer more progressive taxes, but talk is cheap, so they do not always put their money where their mouth is.

Our findings have implications for the design of progressive tax reforms. Counterfactual analysis shows that accounting for behavioral responses significantly reduces both

effective progressivity and total revenue compared to projections that ignore these responses. This highlights that policymakers, particularly in settings with limited enforcement, must account for varying compliance behaviors to achieve intended distributional and revenue objectives.

To apply our findings to other contexts, one must account for institutional and cultural differences. In low-enforcement settings, tax reforms have a greater potential to influence compliance, whereas high-enforcement contexts limit such effects. For example, the 40 largest U.S. cities collect nearly 95% of property taxes on time (Chirico et al., 2016), leaving minimal room for a progressive property tax reform to impact compliance. However, even in developed countries, tax enforcement can be imperfect. The Internal Revenue Service estimates that individuals underreport 63% of income subject to minimal third-party reporting, such as self-employment earnings (Cullen et al., 2021). This non-compliance rate is comparable to that observed in the Argentine setting studied in this paper. In such cases, progressive tax reforms could significantly affect tax evasion, even in developed economies. In addition, cultural or political factors can shape these behavioral responses. A progressive reform could have stronger or weaker effects depending on the prevailing ideological stance of a society or the level of trust in government.

From a broader perspective, property taxes represent one of the oldest forms of taxation and their design has not changed much over time (Chancel et al., 2022; Dray et al., 2023). Despite their crucial role in municipal finance worldwide, with local authorities heavily relying on real estate and land taxes, their structure has often been overlooked. In most countries, property taxes are either flat (proportional to value) or even regressive due to the prominence of fixed components, which stands in sharp contrast to the progressive nature of modern income and wealth taxation. However, the recent surge in research on wealth inequality has renewed interest in making property taxes more progressive. This paper examines a progressive property tax reform in Argentina, offering new evidence on behavioral responses to such reforms and their broader implications for tax compliance and fairness. Our findings suggest that while progressive property taxation can achieve redistributive goals, policymakers must carefully consider how taxpayers' responses—both in terms of compliance behavior and true perceptions of fairness—may affect the reform's ultimate impact on revenue and inequality. As research on wealth inequality and tax fairness evolves, exploring the potential of property tax reforms as a redistributive tool and their long-run efficiency implications, alongside other progressive tax instruments, remains an important avenue for future research.

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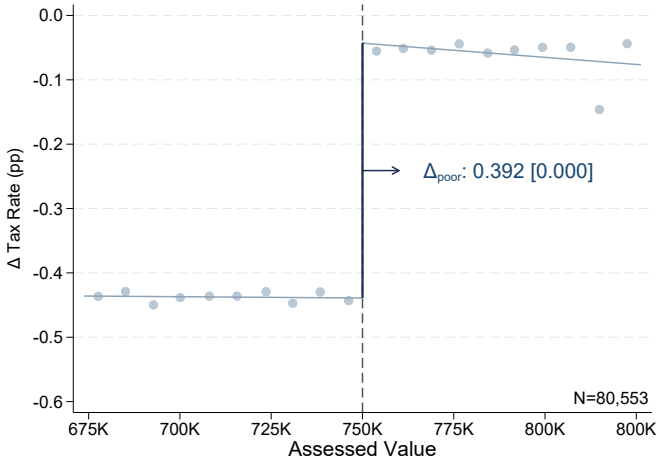
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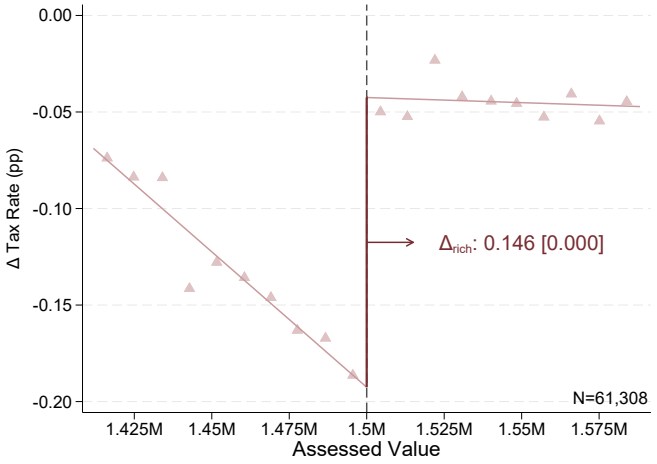
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Figure 1: Own-Rate RDD Effects on Tax Compliance for 2023

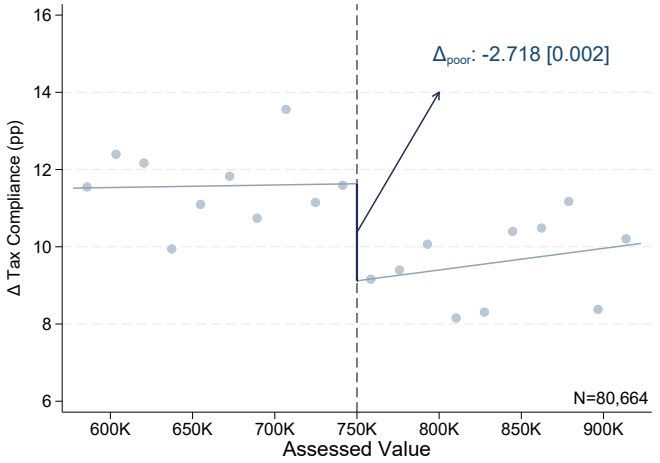
(a) First Stage: Poor Households



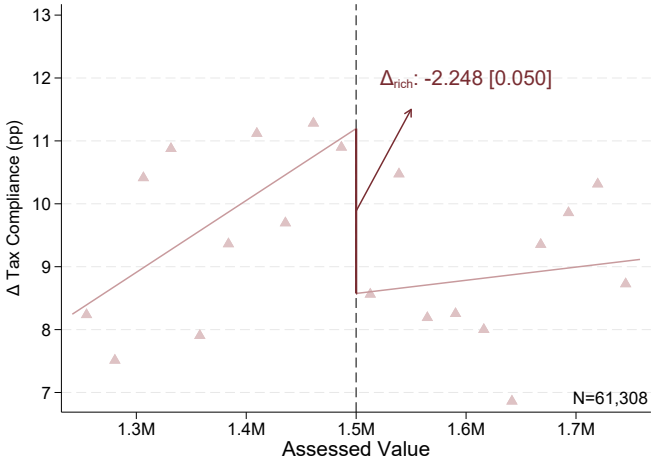
(b) First Stage: Rich Households



(c) Reduced Form: Poor Households



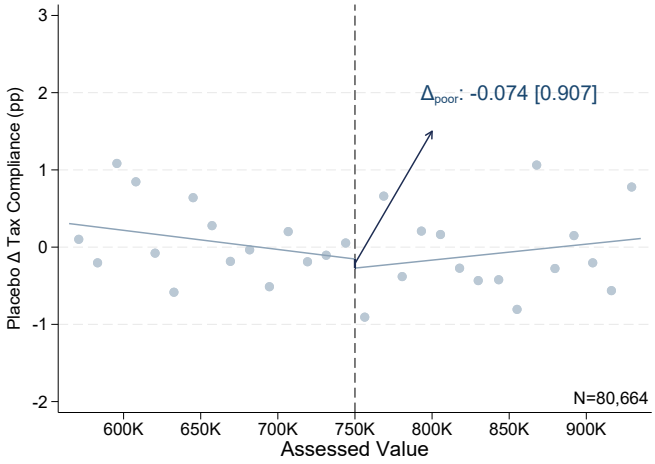
(d) Reduced Form: Rich Households



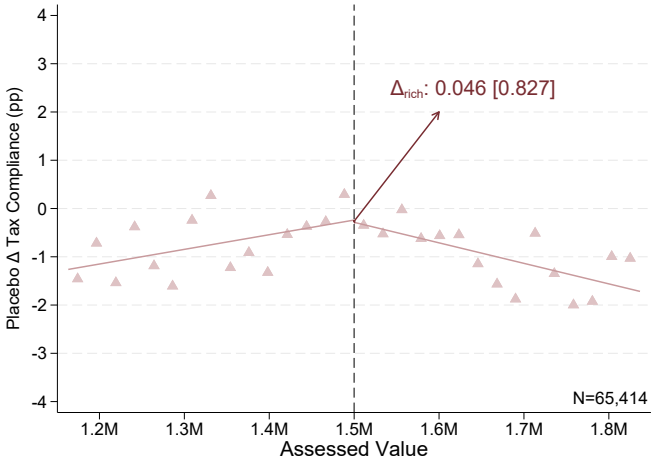
Note: This figure calculates the own-rate effects of the progressive property tax reform in 2023, using a two-cutoff RDD. The left figures (in blue) show the results for the poor at the cutoff of AR$ 750K. The right figures (in red) show the results for the rich at the cutoff of AR$ 1.5M. The running variable corresponds to the properties' cadastral values from 2021. The top panels present the first-stage change in tax rates around these thresholds. The outcome is the household-level change in the tax rate between the last semester of 2022 and the first semester of 2023. The bottom panels present the reduced form effect on tax compliance: how household-level payment rates changed around those thresholds. The outcome is the change in the proportion of monthly bills paid timely between the pre-reform and the post-reform semesters. We define timely payments as bills paid within three months after the due date. Each figure indicates the estimated effect around the cut-off point, where the p-value, taken from a robust bias-corrected inference, is indicated in brackets. So, the RDD point estimates correspond to a change in a time-differenced outcome, reminiscent of a difference-in-discontinuity approach.

Figure 2: RDD Falsification Test using a Placebo Reform Date

(a) Reduced Form: Poor Households



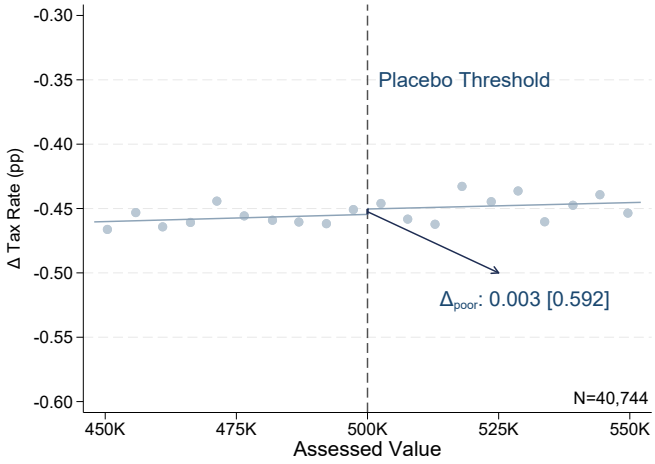
(b) Reduced Form: Rich Households



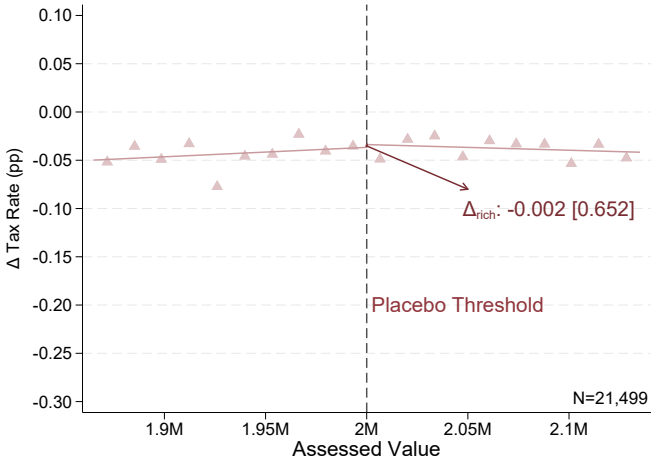
Note: This figure calculates the own-rate effects using a placebo reform date in the middle of 2023. Panels (a) and (b) present the reduced form RDD for poor and rich, in which we evaluate the change in tax compliance for both groups for placebo dates between the first semester of 2023 and the last semester of 2023. The x-axis corresponds to the cadastral valuation of the properties from 2021. Each figure indicates the estimated effect around the cut-off point, where the p-value, taken from a robust bias-corrected inference, is indicated in brackets.

Figure 3: RDD Falsification Test using Placebo Thresholds for 2023

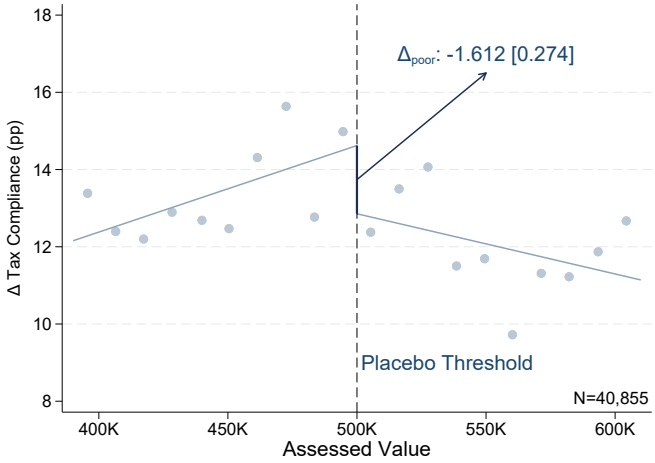
(a) First Stage: Poor Households



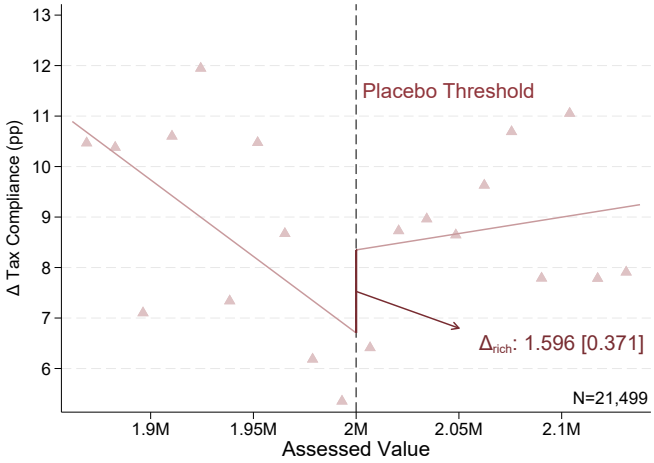
(b) First Stage: Rich Households



(c) Reduced Form: Poor Households



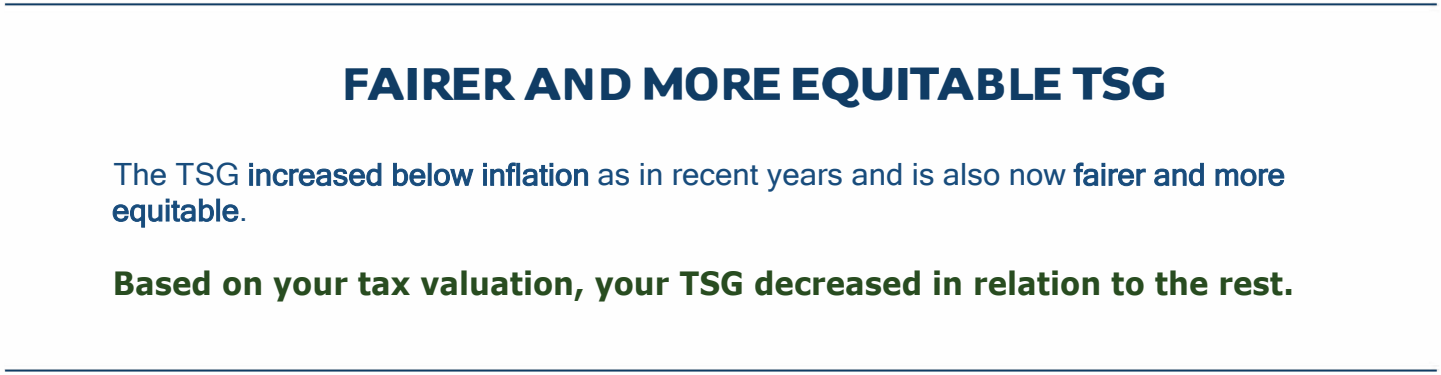
(d) Reduced Form: Rich Households



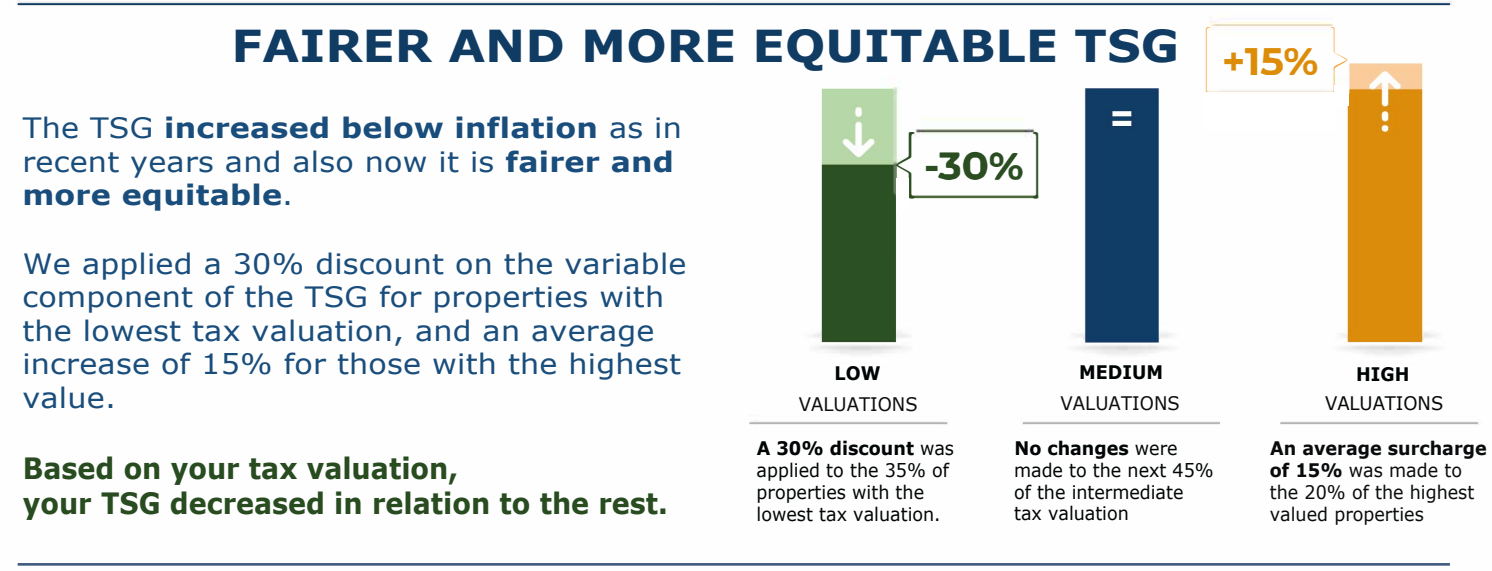
Note: This figure calculates the own-rate effects using placebo tax thresholds at AR$ 500K and AR$ 2M. Panels (a) and (b) represent the first stage RDD for poor and rich, respectively. These two figures evaluate the increase in the amounts owed between the last semester of 2022 and the first semester of 2023, in relation to the value of the property. Panels (c) and (d) represent the reduced form RDD for poor and rich, respectively. They evaluate the change in tax compliance for both groups between the last semester of 2022 and the first semester of 2023. The x-axis corresponds to the properties' cadastral values from 2021. Each figure indicates the estimated effect around the cut-off point, where the p-value, taken from a robust bias-corrected inference, is indicated in brackets.

Figure 4: Sample of Control and Treatment Messages: Poor Households

(a) Control Letter Message



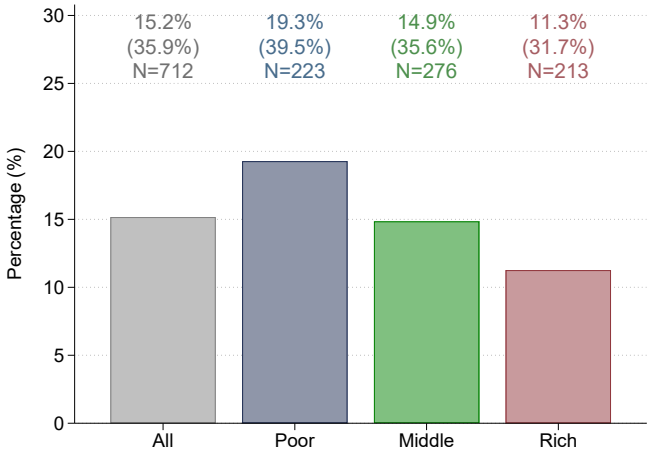
(b) Treatment Letter Message



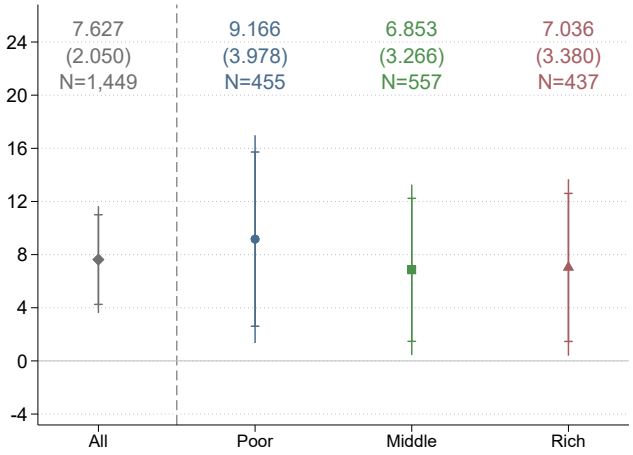
Note: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained on the second page of the mailer (for a full sample of the mailer, see Appendix I.1). TSG refers to *Tasa por Servicios Generales*, the local tax levied by the municipality.

Figure 5: Taxpayers' Awareness of the Reform: Survey Experiment

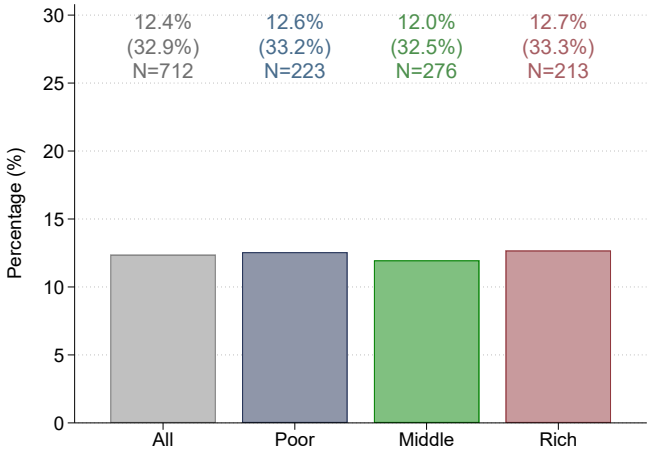
(a) Control: Awareness of Tax Reform



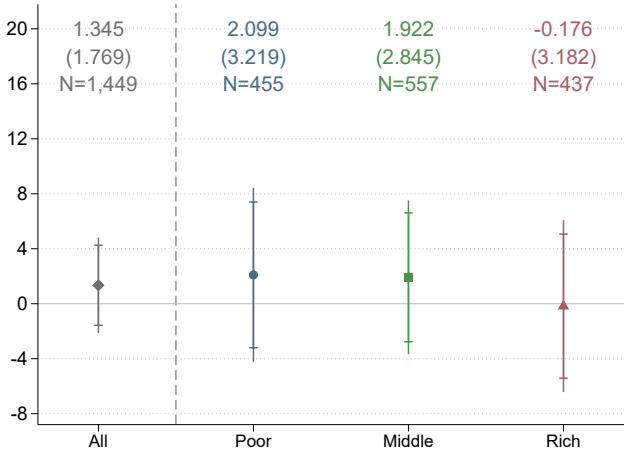
(b) ITT: Awareness of Tax Reform



(c) Control: Awareness of Business Loans



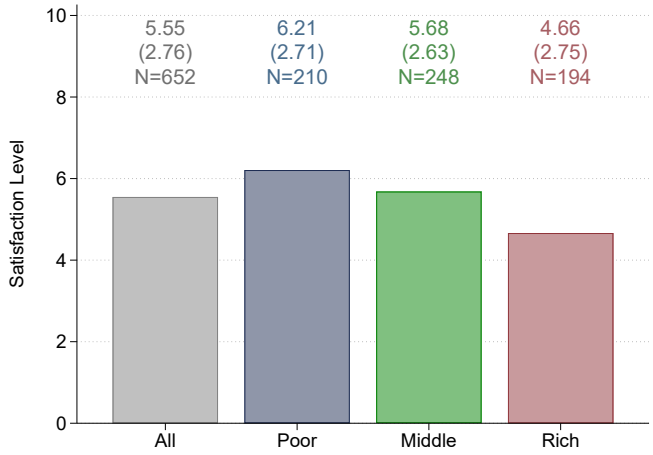
(d) ITT: Awareness of Business Loans



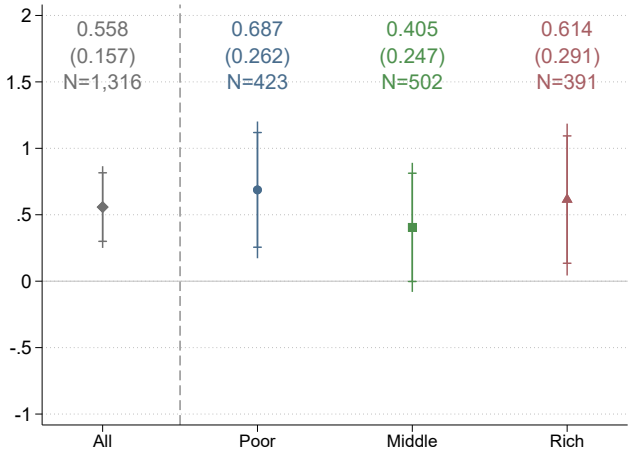
Note: This figure uses data from our online survey on taxpayers to assess awareness of the progressive tax reform. We invited a small subsample of subjects with email addresses on file to participate. The respondents received a list of recent policies and were asked to indicate which ones they had heard of. The left panels show the average knowledge of two different policies for households receiving the control letter. The right panels show the effects of the treatment letter on the awareness of the progressive reform and on the knowledge of the Tres de Febrero business loans. The vertical spikes denote 90% and 95% confidence intervals. The Figure also presents the means (left panels) and treatment effects and their standard errors (right panels).

Figure 6: The Effect on Fairness Perceptions: Survey Experiment

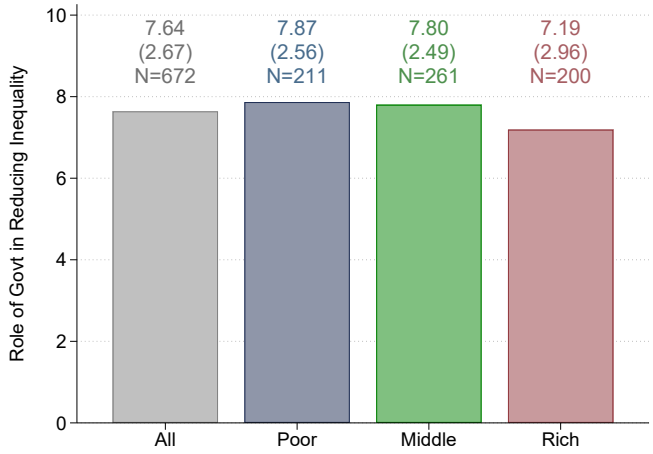
(a) Control: Tax Fairness



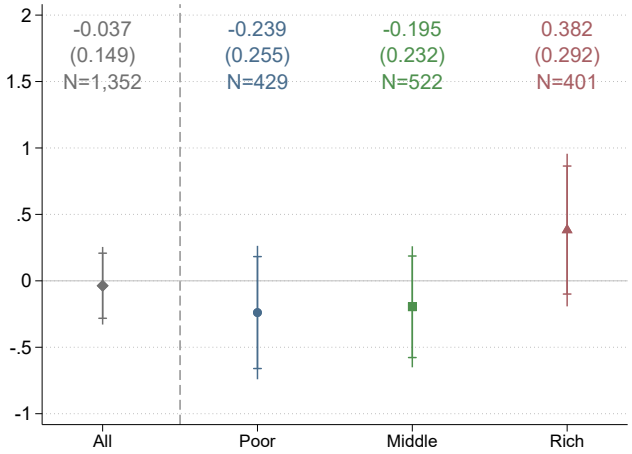
(b) ITT: Tax Fairness



(c) Control: Should Government Reduce Inequality



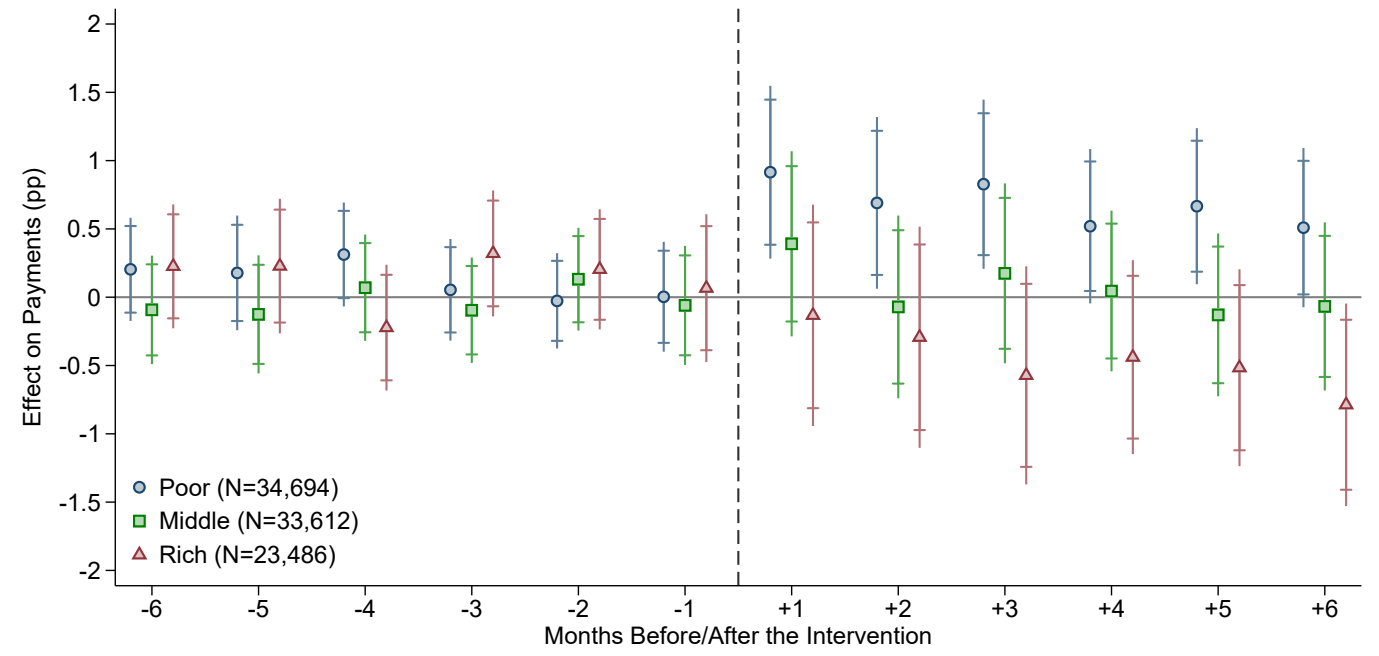
(d) ITT: Should Government Reduce Inequality



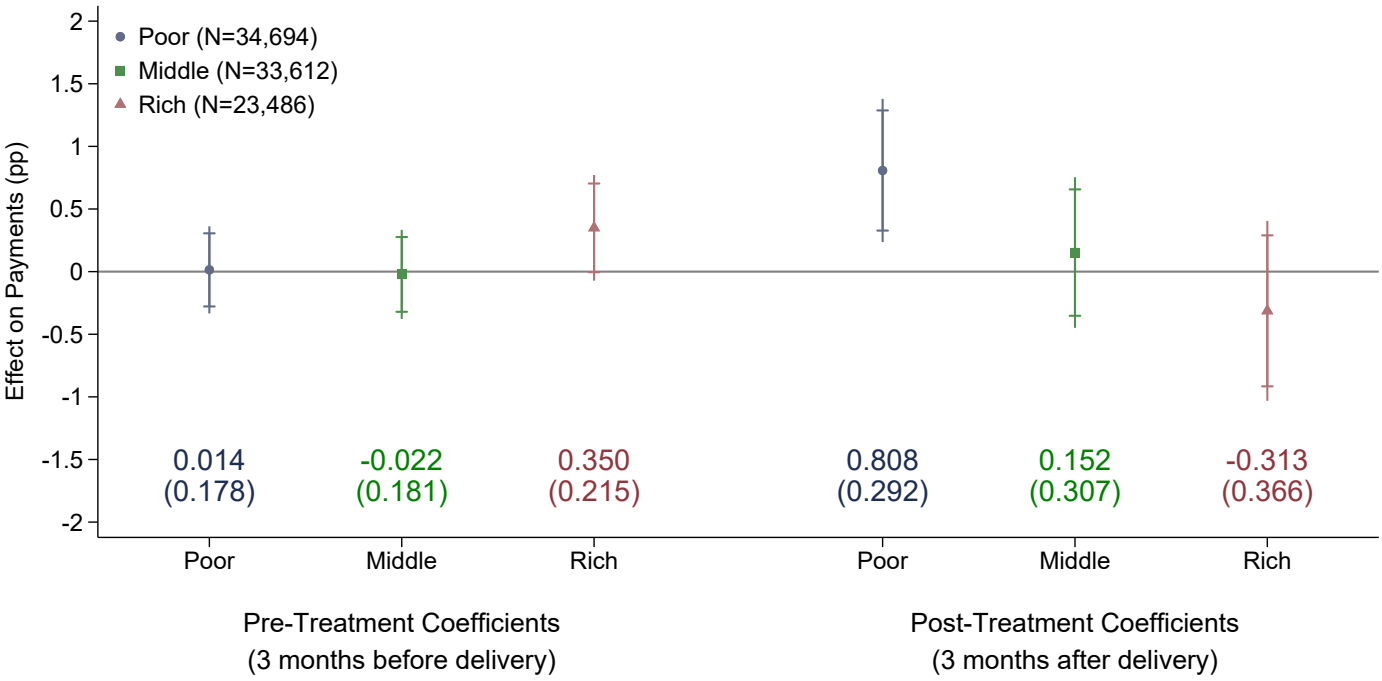
Note: This figure uses data from our online survey on taxpayers to estimate the impact of our progressivity information experiment on taxpayers' fairness perceptions. We asked respondents how fair they considered the distribution of the municipal tax rates between rich and poor households, on a scale from 0 to 10 (0 = very unfair, 10 = very fair) in the top panels, and their perception regarding the government's role in addressing inequality (0 = not be concerned, 10 = do everything to reduce it) in the bottom panels. The left panels show the average levels for households receiving the control letter. The right panels show the effects of the treatment letter on these survey questions comparing treated and control respondents. The vertical spikes denote 90% and 95% confidence intervals. The figure also presents the means (left panels) and treatment effects and their standard errors (right panels).

Figure 7: Cross-rate Effects of a Progressive Tax Reform on Own Tax Compliance

(a) Event-Study Analysis (Monthly Level)



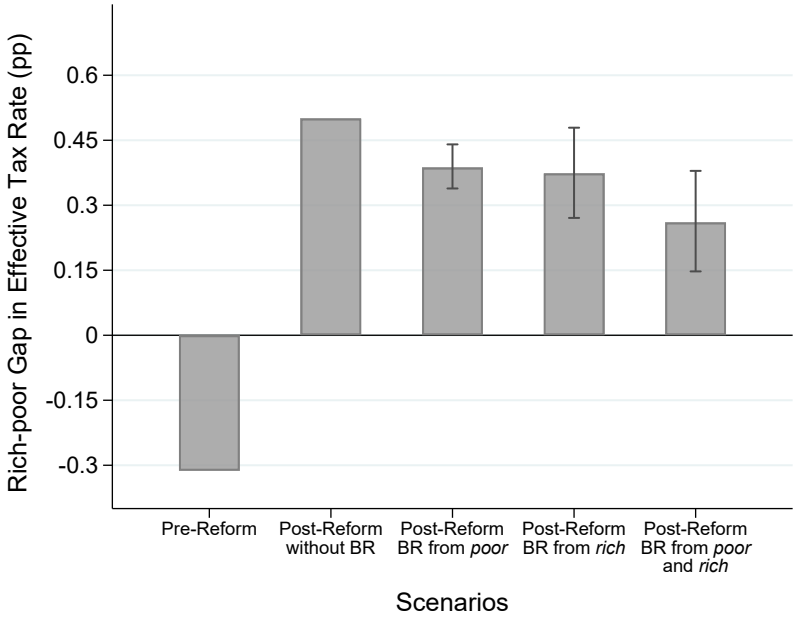
(b) Pre- and Post-Treatment Effects (Quarterly level)



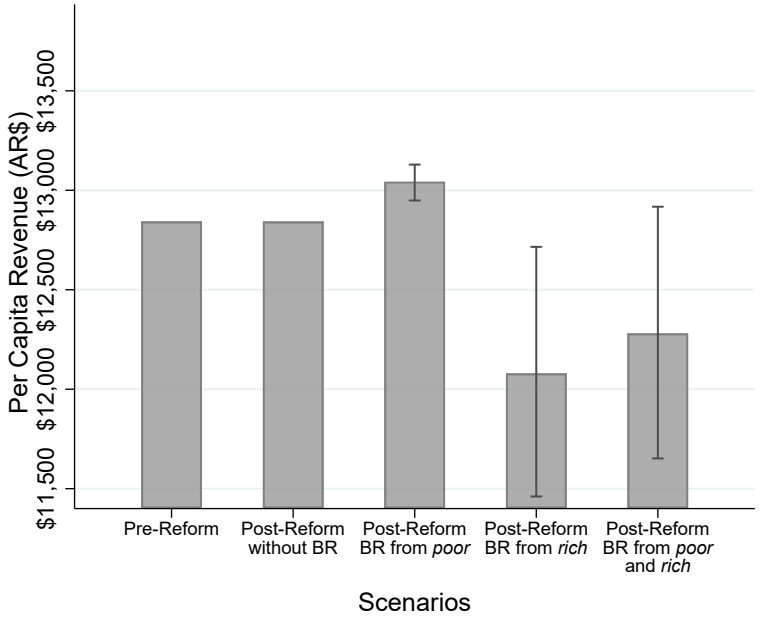
Note: This figure compares the likelihood of paying the monthly property tax bill between treatment and control. The control group received a letter with information solely about how the reform changed the household's own tax rate. The treatment group letter included additional information about how the reform affected the tax rates of other households (i.e., informing the progressive nature of the reform). See Figure 4 for an example. Panel (a) shows the dynamic effect of the treatment, while panel (b) shows the three-month pooled effects of the treatment (pre- and post-treatment). 'Poor' (in blue) denotes households with properties valued at AR$ 750K or less, 'Middle' (in green) denotes properties valued between AR$ 750K and AR$ 1.5M, and 'Rich' (in red) denotes properties valued more than AR$ 1.5M. We estimate the coefficient for each monthly bill in separate regressions, including the 95% and 90% confidence interval for each group. We report clustered standard errors for the pooled estimates in parentheses. The sample includes residential and commercial properties and excludes units that made payments for 2023 before we sent the letters. We control for the past 12 months payments in each pre-treatment regression, and for the 2022 monthly payments in each post-treatment regression. Additionally, we include controls for property valuation (in logs), a residential dummy, dummies indicating whether the property was an "always payer" or "never payer" in the previous year, and a dummy for properties that made an annual payment in the previous year. We included dummies equal to one for missing controls to retain all observations in the regressions. Standard errors clustered at the individual taxpayer level.

Figure 8: Counterfactual Analysis: Tax Progressivity and Revenue with and without Behavioral Responses

(a) Effective Tax Progressivity



(b) Tax Revenue (Per Capita)



Note: This figure uses our key point estimates to analyze the effects of a revenue-neutral progressive reform on effective tax progressivity and tax revenues in the presence of behavioral responses (BR), and we contrast it to a counterfactual scenario where behavioral responses are muted. Panel (a) shows the rich-poor effective tax rate gap, assuming different scenarios of behavioral responses (BR). Panel (b) shows the per capita revenue effects, assuming once again different scenarios of behavioral responses. The vertical spikes denote 90% confidence intervals obtained from a 5,000 repetitions bootstrap. Once behavioral responses are factored in, tax progressivity may not increase as much as intended, and a reform that was supposed to be revenue-neutral may not be so.

Online Appendix (For Online Publication Only)

"From Flat to Fair? The Effects of a Progressive Tax Reform"

Ajzenman, Cruces, Perez-Truglia, Tortarolo and Vazquez-Bare

February 23, 2025

A Details about the Institutional Context

A.1 Local Property Tax

Tres de Febrero is a major urban municipality in the Greater Buenos Aires metropolitan area, Argentina.[52] Our study focuses on a local property tax known as *Tasa por Servicios Generales* (TSG), which funds public services such as public spaces, street lighting, urban sanitation, security, and the fire department, among others.[53] This type of tax is common across all Argentine municipalities and serves as the primary source of local revenue. Tres de Febrero is no exception, with the TSG accounting for 20% of its total resources and 45% of its own-source revenue in 2021.

TSG bills are issued monthly and are due during the first weeks of each billing period.[54] Alternatively, residents have the option of making an advance annual payment in the first months of the year instead of monthly installments.

The municipal property tax consists of variable and fixed components. Before the 2022-24 progressive reforms, the tax liability for a residential taxpayer i was calculated as follows:

$$\begin{aligned}\text{Monthly tax}_i = &\Big[\text{Cadastral value}_i \times \text{Tax rate}/12 + \text{Fixed charge}\Big] \\ &\times \text{Module factor}\end{aligned} \tag{A.1}$$

The variable component is calculated by applying a tax rate to each property's assessed value. Tax rates vary across eight property-use categories defined by the municipality: residential, industrial, commercial, wholesale establishments, mixed-use residential with commerce or factory, empty lots, civil entities, and religious entities. In 2022, the statutory tax rates ranged from 0.42% to 3.22% (Table A.1), with commercial properties bearing a higher tax burden through tax rates that generally exceed those applied to residential properties. Property assessments are based on the cadastre maintained by the Revenue

[52] As of 2022, Tres de Febrero had approximately 365,000 residents and 115,000 households, representing 1.2% of Argentina's total population.

[53] When we asked in our survey experiment about satisfaction with the provision of services by the municipality, on a scale from 0 to 10, we found that the degree of satisfaction is around 5.8 units, so it is not excessively high or low.

[54] Since 2023, the municipality has introduced mid-year adjustments to keep up with growing inflation.

Agency of the Province of Buenos Aires (ARBA).

The fixed components of the tax correspond to specific services, including security, health, fire departments, and maintenance of public spaces. These components are measured in 'modules,' with each module valued at AR$ 10 in 2022 (see Table A.2). While cadastral values are seldom updated, both the variable and fixed charges are adjusted annually through a module factor.[55]

Figure A.1 illustrates that for properties in the bottom decile, fixed charges comprise 73% of the tax bill and variable charges 27%. In contrast, for the top decile, fixed charges account for only 14% of the bill, with variable charges making up 86%.

A.2 The Progressive Tax Reform

For political and equity reasons, the municipality implemented progressive reforms to the municipal property tax during 2022-24. The 2022 and 2023 reforms modified the *variable* component of equation (A.1), while the 2024 reform transformed the previously uniform *fixed* component into a group-specific charge. Each reform used assessed property value thresholds to determine eligibility.[56]

Our analysis focuses on the 2023 and 2024 reforms, as they were more substantive and enabled real-time randomized communication campaigns. However, for completeness and transparency, we describe all three reforms below.

The initial 2022 reform increased the variable component of equation (A.1) by 10% for properties valued above AR$ 1.5M. However, this adjustment was based on outdated assessments and received no public communication or media coverage.

The 2023 reform introduced a four-tier structure. It applied a 30% discount on the variable component of equation (A.1) for properties assessed at AR$ 750K or below, kept it unchanged for properties between AR$ 750K and AR$ 1.5M, retained the 10% surcharge for properties valued between 1.5M and AR$ 3M (now based on an updated cadastre), and imposed a 20% surcharge for properties above AR$ 3M. For a *residential* taxpayer i in cadastral value bracket b, the monthly tax became:

[55] Additionally, one of the four fixed components–the security module–varies by property type: for residential properties, it remains strictly fixed, whereas for commercial properties, it starts at the same minimum but can increase based on a percentage of the total tax owed. If this percentage yields an amount lower than the minimum, the business is charged the minimum amount.

[56] The thresholds defining tax brackets remained constant across all three years. Due to amortization, assessed property values decreased by roughly 4% between 2022-23 and by 1% between 2023-24.

$$\text{Monthly tax}_i = \Big[\text{Cadastral Value}_i \times \text{Tax rate}/12 \times \text{ProgreV}_{b(i)} + \text{Fixed charge}\Big] \times \text{Module factor} \tag{A.2}$$

where $\text{ProgreV}_{b(i)}$ represents the progressivity factor applied to the *variable* component, which varies by cadastral value bracket $b(i)$ as follows:

$$\text{ProgreV}_{b(i)} = \begin{cases} 0.7 & \text{Cadastral Value}_i \leq \text{AR\$ 750K} \\ 1 & \text{AR\$ 750K} < \text{Cadastral Value}_i \leq \text{AR\$ 1.5M} \\ 1.1 & \text{AR\$ 1.5M} < \text{Cadastral Value}_i \leq \text{AR\$ 3M} \\ 1.2 & \text{Cadastral Value}_i > \text{AR\$ 3M} \end{cases} \tag{A.3}$$

Lastly, the 2024 reform transformed the previously uniform *fixed* component of equation (A.2) into differentiated charges by property value (see Table A.2): approximately a 16% reduction for properties at or below AR$ 750K; no change for properties between AR$ 750K and AR$ 1.5M; approximately a 39% increase for properties between AR$ 1.5M and AR$ 3M; and approximately a 64% increase for properties above AR$ 3M. Thus, for a residential taxpayer i in cadastral value bracket b, the monthly tax is:

$$\text{Monthly tax}_i = \Big[\text{Cadastral Value}_i \times \text{Tax rate}/12 \times \text{ProgreV}_{b(i)} + \text{Fixed charge} + \text{ProgreF}_{b(i)}\Big] \times \text{Module factor} \tag{A.4}$$

where $\text{ProgreF}_{b(i)}$ is the progressivity factor applied to the *fixed* part (Table A.2), which varies by cadastral value bracket $b(i)$ as follows:

$$\text{ProgreF}_{b(i)} = \begin{cases} -100 & \text{Cadastral Value}_i \leq \text{AR\$ 750K} \\ 0 & \text{AR\$ 750K} < \text{Cadastral Value}_i \leq \text{AR\$ 1.5M} \\ 240 & \text{AR\$ 1.5M} < \text{Cadastral Value}_i \leq \text{AR\$ 3M} \\ 390 & \text{Cadastral Value}_i > \text{AR\$ 3M} \end{cases} \tag{A.5}$$

These reforms introduced exogenous variation in effective tax rates at the discontinuities, enabling us to estimate the own-rate effects of tax changes (see Section 3). Additionally, we leveraged the timing of these reforms to conduct an information RCT, varying how different aspects of the reform were communicated to households across brackets. This experimental design allows us to assess the cross-rate effects of tax changes, as detailed in Section 4.

Table A.1: Property Type and Statutory Tax Rates (STR)

Category	N Properties		Statutory Tax Rates (%)				
	N	%	2018	2019	2020	2021	2022-2024
Residential	109,729	82.8	0.30	0.50	0.69	0.93	1.21
Residential with commerce/factory	10,073	7.6	0.45	0.75	1.04	1.4	1.82
Commercial	7,425	5.6	0.70	1.18	1.68	2.27	2.95
Empty lot	1,166	0.9	0.70	1.27	1.84	2.48	3.22
Industrial	3,699	2.8	0.70	1.22	1.77	2.39	3.11
Civil entities	220	0.2	0.10	0.17	0.24	0.32	0.42
Religious entities	170	0.1	0.10	0.17	0.24	0.32	0.42
Wholesale establishments	1	0.0	0.45	0.76	1.10	1.49	1.94
Assessed values ARBA (year)			2018	2018	2018	2018	2021

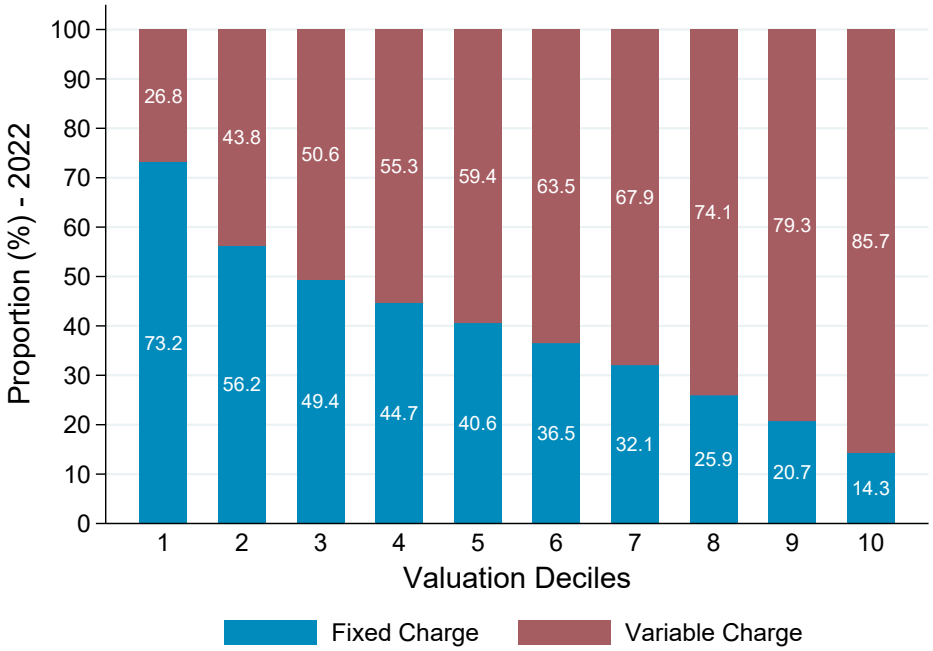
Note: This table reports statutory tax rates by category of property as defined in the tax code (*Ordenanza Impositiva*) every year. The first two columns display the number of properties in 2018. The tax base used to calculate the monthly fee corresponds to the assessed value in 2018 provided by the provincial tax authority (ARBA). Despite an annual inflation of 47% in 2018, 54% in 2019, and 36% in 2020, this base remained unadjusted. To compensate, the municipality increased the tax rates.

Table A.2: Fixed Charges (measured in modules)

	2022	2023	2024			
			Cadastral Values			
			≤ AR$ 750K	AR$ 750K-1.5M	AR$ 1.5M-3M	> AR$ 3M
Unit modules (AR$ 10 each):						
Security	25	25	25	25	25	25
Public spaces	14.5	14.5	9.5	14.5	26.5	33.5
Health	20	20	15	20	32	40
Firefighters	1.5	1.5	1.5	1.5	1.5	1.5
Total modules	61	61	51	61	85	100
Tax change (fixed charge)		*Unchanged*	-16%	*Unchanged*	39%	64%
Module factor	1	1.6	3.6–9	3.6–9	3.6–9	3.6–9

Note: The municipality adjusts the tax bill annually for inflation. Since 2024, they have adjusted the bill every other month to keep up with the rising inflation. The module factor is 3.6 in January, 5.1 in March, 7 in May, 8.4 in July, and 9 in September.

Figure A.1: Fixed and Variable Charges of the Property Tax Bill, by Deciles



Note: This figure shows the relative importance of variable and fixed components of the property tax bill by deciles of assessed values, for the year 2022.

B Robustness Checks

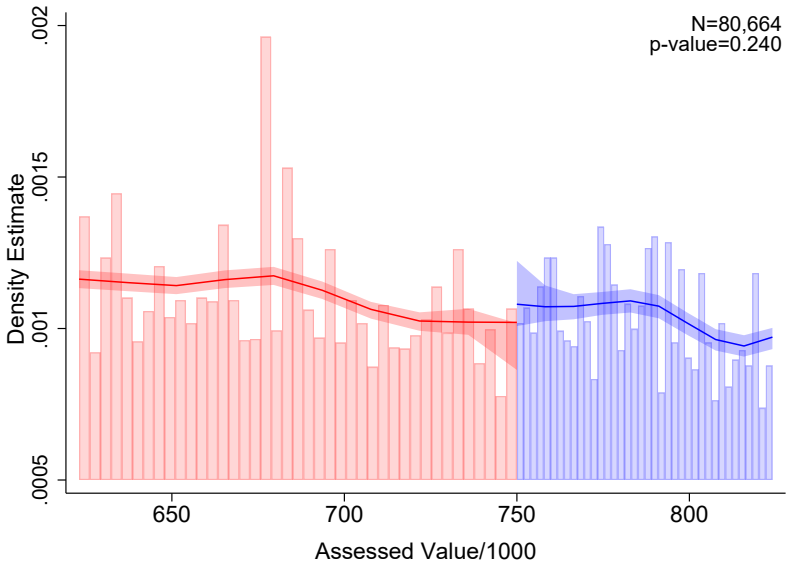
B.1 Manipulation Test

We conduct the standard McCrary (2008) manipulation test to assess the continuity of the running variable's density function based on the methods proposed by Cattaneo et al. (2018, 2020). Intuitively, taxpayers might attempt to influence their own-rate treatment status by adjusting their property's assessed values to fall just above the AR$ 750K threshold or below the AR$ 1.5M threshold— zones where the tax rate remained unchanged. In practice, however, such manipulation is virtually impossible, as assessed values are calculated and reported by the provincial tax authority (ARBA).

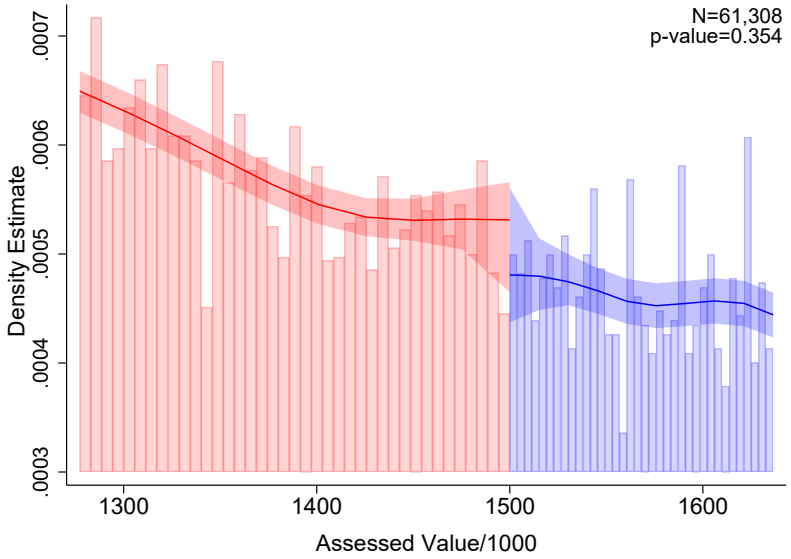
The McCrary density test and accompanying figures reveal no evidence of unusual bunching around the thresholds that determine treatment eligibility. While we detect a slight discontinuity for poor properties, the institutional context suggests this is unlikely to reflect deliberate manipulation. Property values are based on fiscal valuations, which are typically established by governmental authorities and cannot be directly altered by individual property owners. We, therefore, consider this result spurious and conclude that it does not compromise the validity of the design. Overall, this evidence reinforces the validity of the regression discontinuity design used to estimate own-rate effects in Section 3.

Figure B.1: Manipulation Test of Assessed Values around the Discontinuities: 2023

(a) Poor Cutoff - 2023



(b) Rich Cutoff - 2023



Note: Panels (a) and (b) show the McCrary (2008) manipulation test (with their corresponding p-value) of the property assessed values at the low-value threshold (poor) and the high-value threshold (rich) in 2023. The shaded areas represent the 95% confidence intervals. The x-axis represents assessed property value in AR$ thousands.

B.2 Alternative Specifications

We conduct a series of robustness checks on the own-rate and cross-rate effects presented in Sections 3 and 4. The results indicate that our findings are highly robust.

Table B.1 presents multiple robustness checks for the RDD analysis used to estimate own-rate effects. These checks incorporate: (i) additional pre-treatment controls, (ii) the use of a uniform kernel, (iii) including just households in the sample, (iv) alternative definitions of the outcome variable and (v) including in the sample only those households which received a control or treatment letter, respectively. The estimates remain stable across these specifications, supporting the validity of the main findings.

Table B.2 focuses on robustness checks for the experimental analysis used to estimate cross-rate effects. These tests examine the sensitivity of the results to: (i) including only pre-treatment payment controls, (ii) incorporating the email sample into the regressions, (iii) including just households in the sample, (iv) using an alternative definition of the outcome, and (v) excluding observations with property values in the top and bottom 5 percentiles to mitigate the influence of outliers. The findings remain stable across all specifications.

The consistency of results across these robustness checks strengthens confidence in the reliability of the main conclusions.

Table B.1: Robustness Tests: Own-Rate Effects

	Robustness Checks for RDD Results						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect of Reform on Tax Compliance:							
Low-Valuation HHs	−2.627***	−1.531**	−3.194***	−2.515***	−2.514***	−3.931***	−1.358
	[0.002]	[0.025]	[0.001]	[0.005]	[0.004]	[0.001]	[0.240]
						[pval diff.=0.142]	
High-Valuation HHs	−1.983*	−1.313	−2.194*	−2.800**	−2.149**	−1.444	−3.308*
	[0.067]	[0.118]	[0.052]	[0.023]	[0.040]	[0.569]	[0.064]
						[pval diff.=0.320]	
Effect of Reform on Tax Rates:							
Low-Valuation HHs	0.400***	0.393***	0.401***	0.366***	0.400***	0.400***	0.384***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
						[pval diff.=0.246]	
High-Valuation HHs	0.155***	0.158***	0.160***	0.126***	0.155***	0.144***	0.148***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
						[pval diff.=0.706]	
Specification							
Additional Controls		✓					
Uniform Kernel			✓				
Just Households				✓			
Alternative outcome (6 months of late payment)					✓		
Just Control Sample						✓	
Just Treatment Sample							✓
Observations (Compliance)							
Low-Valuation HHs	30,589	31,332	20,756	28,208	35,344	30,821	30,487
High-Valuation HHs	64,624	55,410	64,624	54,648	64,624	15,009	16,243
Observations (Rates)							
Low-Valuation HHs	12,964	9,313	17,053	15,934	12,964	30,821	30,487
High-Valuation HHs	64,624	55,410	64,624	54,648	64,624	7,067	7,411
Average Tax Compliance (Baseline)							
Low-Valuation HHs	59.8%	59.7%	60.5%	60.9%	59.9%	57.2%	56.7%
High-Valuation HHs	56.7%	56.6%	56.5%	58.3%	56.7%	60.5%	60.0%
Average Tax Rate (Baseline)							
Low-Valuation HHs	2.2%	2.2%	2.2%	2.1%	2.2%	1.8%	1.9%
High-Valuation HHs	1.9%	1.9%	1.9%	1.7%	1.9%	2.2%	2.2%

Note: This table summarizes Figure 1 using different specifications for the estimates. Column (1) shows the main estimates, column (2) shows the estimates including additional controls such as the payment in previous periods of the corresponding bill, column (3) uses an uniform kernel rather than a triangular one, column (4) includes just households in the sample, column (5) uses a alternative outcome that includes the possibility of paying 6 months after the due date instead of only 3 months later. Finally, columns (6) and (7) focus exclusively on the control and treatment samples, respectively, while also reporting the p-value from the test of differences between them estimated through a 5,000 repetitions bootstrap. Each column indicates the estimated effect around the cut-off point, where the p-value, taken from a robust bias-corrected inference, is indicated in brackets.

Table B.2: Robustness Tests: Cross-Rate Effects

	Robustness Checks for Experimental Results					
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of Reform on Tax Compliance:						
Low-Valuation HHs	0.808***	0.795***	0.698**	0.756**	0.570**	0.806**
	(0.292)	(0.295)	(0.278)	(0.304)	(0.290)	(0.320)
Medium-Valuation HHs	0.152	0.170	0.132	0.247	0.166	0.152
	(0.307)	(0.309)	(0.286)	(0.320)	(0.304)	(0.307)
High-Valuation HHs	−0.313	−0.421	−0.245	−0.177	−0.254	−0.446
	(0.366)	(0.369)	(0.341)	(0.418)	(0.367)	(0.405)
Specification						
Just pre-treat payment controls		✓				
Email Sample Included			✓			
Just Households				✓		
Alternative outcome (6 months of late payment)					✓	
No valuations below/above the 5th/95th percentile						✓
Observations						
Low-Valuation HHs	34,694	34,694	38,678	31,999	34,694	29,747
Medium-Valuation HHs	33,612	33,612	38,325	30,945	33,612	33,612
High-Valuation HHs	23,486	23,486	26,716	17,945	23,486	19,323
Average Tax Compliance (Baseline)						
Low-Valuation HHs	54.5%	54.5%	57.3%	54.5%	54.5%	55.5%
Medium-Valuation HHs	56.0%	56.0%	59.1%	56.5%	5.06%	56.0%
High-Valuation HHs	56.1%	56.1%	59.0%	57.1%	56.1%	55.8%

Note: This table summarizes Figure 7 using different specifications for the estimates. Column (1) shows the main estimates, column (2) excludes every other control not related to pre-treatment payments, column (3) includes the e-mail survey sample, column (4) includes only residential properties in the sample (excludes commercial properties), column (5) uses an alternative outcome that includes the possibility of paying 6 months after the due date instead of only 3 months later, and column (6) excludes observations with valuations below the 5th percentile and above the 95th percentile of valuations. Standard errors clustered at the individual level in parenthesis.

C Balance Tables

The balance tables compare baseline characteristics across treatment and control groups (Tables C.1 and C.3), as well as between control and no-letter groups (Tables C.2 and C.4), for the experiments conducted in 2023 and 2024. At the request of the authorities, the no-letter pool in the 2023 experiment consists exclusively of businesses, restricting the control-versus-no-letter comparison to this group. We also compare baseline characteristics across treatment and control groups for our complementary survey (Table C.5).

Table C.1 presents the results for the 2023 experiment, highlighting that key variables such as payment shares for 2021 and 2022, assessed property values, and tax rates are statistically balanced between treatment and control groups. The p-values across all variables confirm no significant differences, ensuring group comparability. Table C.3 replicates this analysis for the 2024 experiment, yielding similar results and further reinforcing the reliability of the randomization process.

For the control letter versus no letter comparison, Table C.2 evaluates the 2023 experiment, showing no significant imbalances in pre-treatment characteristics like payment shares and assessed property values, as indicated by the p-values. Table C.4 extends this comparison to the 2024 experiment, and while the difference is statistically significant for 5 of the pre-treatment comparisons, this significance is driven by the large sample size, while the actual difference remains small in magnitude. Moreover, Table C.4 reports 22 tests, implying that, by chance, 2 or 3 are likely to be statistically significant at the 10% level. Nevertheless, we include these variables as controls in the baseline specification to mitigate potential biases.

Finally, Table C.5 also compares our treatment groups across different valuations for our survey experiment. Overall, we see few significant differences between groups.

Table C.1: Balance Table: 2023 Experiment

		All			Poor			Middle			Rich		
	All	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value
Share Household (%)	88.1	88.3	88.0	0.168	92.3	92.1	0.491	92.0	92.1	0.855	76.8	76.1	0.199
	(32.4)	(32.2)	(32.5)		(26.6)	(26.9)		(27.1)	(27.0)		(42.2)	(42.7)	
Share of Payments (2021) (%)	44.0	44.2	43.9	0.294	42.4	41.9	0.306	44.7	45.0	0.530	46.1	45.1	0.116
	(45.8)	(45.8)	(45.8)		(45.4)	(45.5)		(45.7)	(45.8)		(46.4)	(46.1)	
Share of Payments (2022) (%)	44.0	44.2	43.8	0.236	42.1	41.6	0.299	44.7	44.9	0.733	46.4	45.6	0.134
	(45.3)	(45.3)	(45.4)		(44.9)	(44.9)		(45.3)	(45.4)		(45.8)	(45.8)	
2023 Assessed Value (million AR$)	129.8	127.8	131.8	0.296	47.5	47.4	0.814	105.5	105.8	0.169	279.4	292.5	0.381
	(587.5)	(391.8)	(733.7)		(18.9)	(18.8)		(21.1)	(21.3)		(754.9)	(1431.9)	
2022 Tax Rate (%)	2.80	2.80	2.80	0.87	4.00	4.01	0.96	2.08	2.08	0.73	2.06	2.07	0.52
	(4.34)	(4.20)	(4.49)		(6.61)	(7.09)		(0.59)	(0.50)		(0.79)	(0.81)	
Share Poor (%)	37.8	37.7	37.8	0.751	-	-	-	-	-	-	-	-	-
	(48.5)	(48.5)	(48.5)										
Share Rich (%)	25.6	25.4	25.8	0.201	-	-	-	-	-	-	-	-	-
	(43.6)	(43.5)	(43.7)										
Observations	91,792	46,082	45,710		17,394	17,300		16,982	16,630		11,706	11,780	

Note: This balance table presents the difference between control and treatment groups across different categories (All, Poor, Middle, and Rich). Standard deviation in parenthesis. The p-value columns reflect the results of difference tests between control and treatment groups. Results are omitted in categories where comparisons between groups are not applicable.

Table C.2: Balance Table Letter vs No Letter (Businesses Only): 2023 Experiment

		All			Poor			Middle			Rich		
	All	No Letter Business	Control Business	P-value	No Letter Business	Control Business	P-value	No Letter Business	Control Business	P-value	No Letter Business	Control Business	P-value
Share of Payments (2021) (%)	42.1	41.9	42.3	0.657	42.6	42.6	0.982	36.6	38.3	0.322	44.2	44.1	0.895
	(45.8)	(45.9)	(45.8)		(45.8)	(45.8)		(45.0)	(44.9)		(46.1)	(46.2)	
Share of Payments (2022) (%)	43.6	43.8	43.4	0.707	45.0	43.7	0.479	38.9	39.6	0.652	45.7	45.2	0.687
	(45.9)	(46.0)	(45.7)		(45.9)	(45.4)		(45.6)	(45.4)		(46.2)	(46.0)	
2023 Assessed Value (million AR$)	238.6	235.3	241.9	0.713	42.9	42.9	0.993	111.0	111.2	0.857	394.2	404.5	0.773
	(936.4)	(836.4)	(1027.1)		(18.5)	(18.7)		(21.7)	(21.4)		(1163.1)	(1429.1)	
2022 Tax Rate (%)	3.68	3.68	3.68	0.94	5.10	5.04	0.64	3.22	3.26	0.43	3.21	3.22	0.55
	(2.08)	(2.37)	(1.74)		(4.18)	(2.34)		(0.92)	(1.63)		(0.98)	(0.92)	
Share Poor (%)	24.7	24.7	24.7	0.994	-	-	-	-	-	-	-	-	-
	(43.1)	(43.1)	(43.1)										
Share Rich (%)	50.1	49.8	50.3	0.597	-	-	-	-	-	-	-	-	-
	(50.0)	(50.0)	(50.0)										
Observations	10,831	5,425	5,406		1,339	1,334		1,384	1,352		2,702	2,720	

Note: This balance table presents the difference between control and no letter groups across different categories (All, Poor, Middle, and Rich). Table shows only Businesses, since every household received a letter in the 2023 experiment. Standard deviation in parenthesis. The p-value columns reflect the results of difference tests between control and treatment groups. Results are omitted in categories where comparisons between groups are not applicable.

Table C.3: Balance Table: 2024 Experiment

		All			Poor			Middle			Rich		
	All	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value
Share Household (%)	84.7	84.8	84.7	0.879	89.5	89.6	0.713	89.3	89.0	0.396	70.2	70.3	0.891
	(36.0)	(35.9)	(36.0)		(30.6)	(30.5)		(30.9)	(31.2)		(45.7)	(45.7)	
Share of Payments (2022) (%)	41.5	41.7	41.3	0.295	39.4	39.3	0.859	42.3	41.9	0.437	44.4	43.9	0.365
	(45.1)	(45.1)	(45.1)		(44.6)	(44.6)		(45.2)	(45.1)		(45.8)	(45.8)	
Share of Payments (2023) (%)	51.0	51.0	51.0	0.937	50.7	50.6	0.840	51.3	51.3	0.991	51.0	51.0	0.908
	(46.4)	(46.4)	(46.4)		(46.5)	(46.5)		(46.1)	(46.3)		(46.4)	(46.4)	
2024 Assessed Value (million AR$)	119.6	119.1	120.1	0.548	47.1	47.0	0.522	105.2	105.5	0.334	257.3	261.4	0.495
	(238.0)	(110.4)	(317.5)		(19.1)	(18.9)		(21.1)	(21.2)		(145.1)	(620.0)	
2023 Tax Rate (%)	2.66	2.65	2.68	0.31	3.50	3.58	0.32	2.06	2.07	0.21	2.15	2.13	0.06
	(4.16)	(3.79)	(4.50)		(5.92)	(7.05)		(0.44)	(0.46)		(0.87)	(0.85)	
Share Poor (%)	39.8	39.8	39.9	0.657	-	-	-	-	-	-	-	-	-
	(49.0)	(48.9)	(49.0)										
Share Rich (%)	24.3	24.3	24.3	0.996	-	-	-	-	-	-	-	-	-
	(42.9)	(42.9)	(42.9)										
Observations	92,616	46,179	46,437		18,366	18,535		16,585	16,612		11,228	11,290	

Note: This balance table presents the difference between control and treatment groups across different categories (All, Poor, Middle, and Rich). Standard deviation in parenthesis. The p-value columns reflect the results of difference tests between control and treatment groups. Results are omitted in categories where comparisons between groups are not applicable.

Table C.4: Balance Table Letter vs No Letter: 2024 Experiment

		All			Poor			Middle			Rich		
	All	No Letter	Control	P-value	No Letter	Control	P-value	No Letter	Control	P-value	No Letter	Control	P-value
Share Household (%)	84.7	83.9	84.8	0.105	89.3	89.5	0.759	88.1	89.3	0.101	69.3	70.2	0.496
	(36.0)	(36.8)	(35.9)		(30.9)	(30.6)		(32.4)	(30.9)		(46.1)	(45.7)	
Share of Payments (2022) (%)	41.6	40.9	41.7	0.241	39.3	39.4	0.926	42.6	42.3	0.797	40.8	44.4	0.010
	(45.1)	(45.1)	(45.1)		(44.6)	(44.6)		(45.3)	(45.2)		(45.7)	(45.8)	
Share of Payments (2023) (%)	50.9	49.8	51.0	0.087	49.8	50.7	0.416	50.7	51.2	0.604	48.4	51.0	0.073
	(46.4)	(46.4)	(46.3)		(46.5)	(46.5)		(46.1)	(46.1)		(46.7)	(46.4)	
2024 Assessed Value (million AR$)	119.1	119.0	119.1	0.932	46.3	47.2	0.073	105.3	105.2	0.854	252.8	257.3	0.302
	(109.8)	(103.0)	(110.4)		(19.2)	(19.4)		(21.3)	(21.2)		(122.7)	(145.1)	
2023 Tax Rate (%)	2.65	2.68	2.65	0.60	3.57	3.50	0.64	2.08	2.07	0.17	2.20	2.16	0.15
	(3.84)	(4.23)	(3.79)		(6.69)	(5.92)		(0.46)	(0.44)		(0.96)	(0.94)	
Share Poor (%)	39.7	38.9	39.8	0.240	-	-	-	-	-	-	-	-	-
	(48.9)	(48.8)	(48.9)										
Share Rich (%)	24.4	24.8	24.3	0.442	-	-	-	-	-	-	-	-	-
	(42.9)	(43.2)	(42.9)										
Observations	51,070	4,870	46,200		1,894	18,368		1,768	16,602		1,208	11,230	

Note: This balance table presents the difference between control and no letter groups across different categories (All, Poor, Middle, and Rich). Standard deviation in parenthesis. The p-value columns reflect the results of difference tests between control and treatment groups. Results are omitted in categories where comparisons between groups are not applicable.

Table C.5: Balance Table: Complementary Survey

		All			Poor			Middle			Rich		
	All	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value
Share Household (%)	93.0	91.9	94.1	0.068	95.3	97.1	0.279	96.8	97.0	0.879	81.8	87.7	0.058
	(25.5)	(27.3)	(23.6)		(21.2)	(16.8)		(17.7)	(17.2)		(38.6)	(32.9)	
Share of Payments (2021) (%)	81.5	80.3	82.7	0.129	77.7	82.0	0.153	81.5	82.7	0.629	81.2	83.3	0.456
	(32.6)	(33.8)	(31.5)		(35.8)	(33.0)		(32.5)	(30.8)		(33.4)	(30.8)	
Share of Payments (2022) (%)	84.0	83.5	84.5	0.466	81.8	87.7	0.014	84.6	82.1	0.278	83.6	84.1	0.868
	(28.9)	(29.0)	(28.9)		(30.0)	(25.1)		(27.9)	(30.9)		(29.6)	(29.7)	
2023 Assessed Value (million AR$)	135.0	132.0	137.9	0.243	51.3	52.7	0.326	107.0	105.7	0.420	247.1	261.2	0.163
	(105.6)	(95.5)	(114.6)		(16.0)	(15.6)		(21.4)	(21.1)		(95.0)	(131.9)	
2022 Tax Rate (%)	2.285	2.323	2.249	0.159	3.133	2.937	0.165	1.997	2.001	0.795	1.940	1.858	0.135
	(1.093)	(1.204)	(0.975)		(1.838)	(1.390)		(0.235)	(0.210)		(0.650)	(0.602)	
Share Poor (%)	30.7	30.2	31.3	0.619	-	-	-	-	-	-	-	-	-
	(46.1)	(45.9)	(46.4)										
Share Rich (%)	30.6	29.8	31.4	0.479	-	-	-	-	-	-	-	-	-
	(46.1)	(45.8)	(46.4)										
Observations	1,732	849	883		256	276		340	330		253	277	

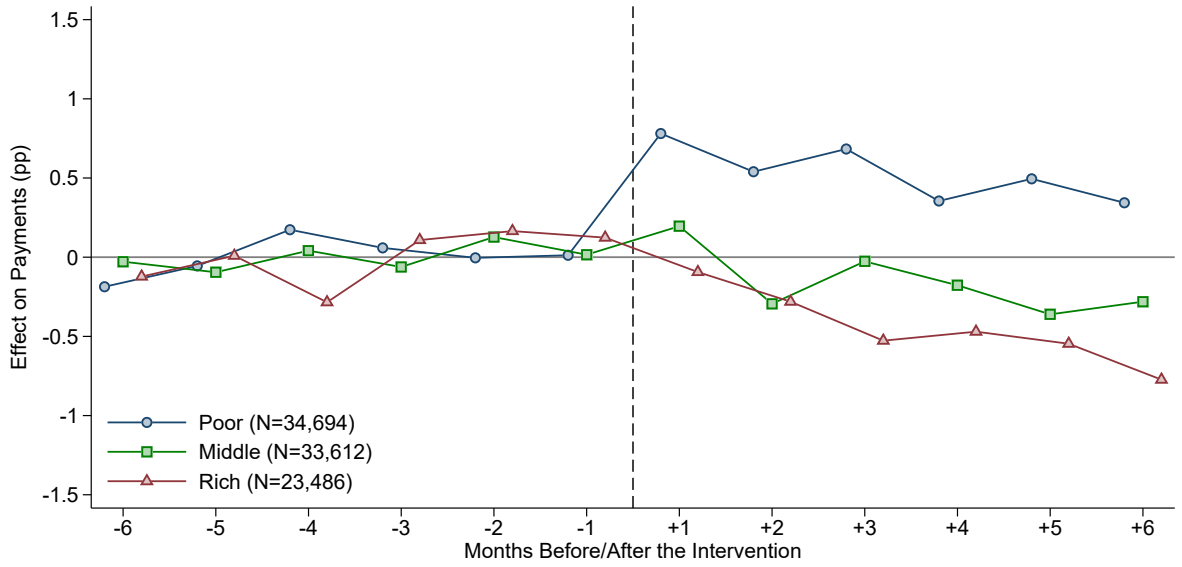
Note: This balance table presents the difference between control and treatment groups across different categories (All, Poor, Middle, and Rich) for our survey experiment. Standard deviation in parenthesis. The p-value columns reflect the results of difference tests between control and treatment groups. Results are omitted in categories where comparisons between groups are not applicable.

D Additional Experimental Results

D.1 Differences in Payment Rates

This section examines the additional experimental results comparing the treatment and control groups in terms of tax payment behavior. As shown in Figure D.1, six months prior to the intervention, the payment differences between the treatment and control groups were minimal, indicating no significant pre-existing disparities in compliance behavior. However, a notable shift was observed post-treatment, especially among lower-valuation taxpayers. Specifically, an increase in the number of payments was recorded, with lower-value properties in the treatment group showing substantially higher compliance rates compared to the control group. In contrast, taxpayers in the high-value bracket experienced a slight decrease in compliance, while those in the middle bracket displayed no significant behavioral changes.

Figure D.1: Difference in Payment Rates between Control and Treatment Letters in 2023



Note: This figure compares the likelihood of paying the monthly bill between treatment and control. The blue line denotes poor households with assessed values below AR$ 750K. The green line denotes middle households with assessed values between AR$ 750K and AR$ 1.5M. The red line denotes rich households with assessed values above AR$ 1.5M. We normalized each series by the average pre-treatment difference. We estimate the coefficient for each monthly bill in separate regressions. The sample includes residential and commercial properties and excludes units that made payments for 2023 before we sent the letters.

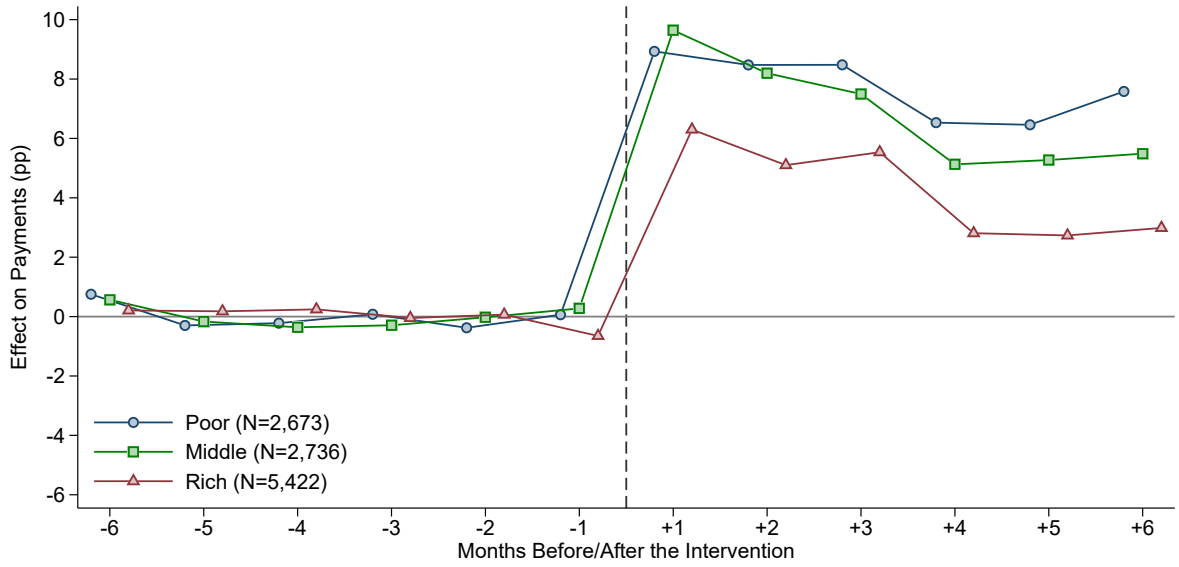
D.2 Control Letter vs No Letter

This section compares the effects of receiving a control letter versus receiving no letter on tax compliance. Two figures illustrate the findings, focusing on changes in pre- and post-treatment payment behaviors across different valuation brackets.

Figure D.2 shows that before the intervention, payment patterns were similar between groups that received a letter and those that did not, confirming the lack of significant differences before treatment. However, after treatment, properties that received a control letter showed higher compliance rates than those that received no letter, suggesting the mere receipt of a letter influences compliance behavior.

Figure D.3 presents a comprehensive analysis of treatment effects on tax compliance behavior. Panel (a) examines compliance rates before and after the intervention across the two groups. The findings show a noticeable increase in payments for those receiving a control letter during the first two months of payments, with comparable patterns observed across properties of lower, middle, and high value. This increase corresponds to a rise of approximately 4 to 6 percentage points in the payment share for these months. Panel (b) pools data into two-month pre- and post-treatment periods to enhance statistical power. The analysis reveals no significant differences in compliance rates before treatment, but post-treatment, the groups receiving control letters consistently exhibit higher compliance rates across all valuation brackets.

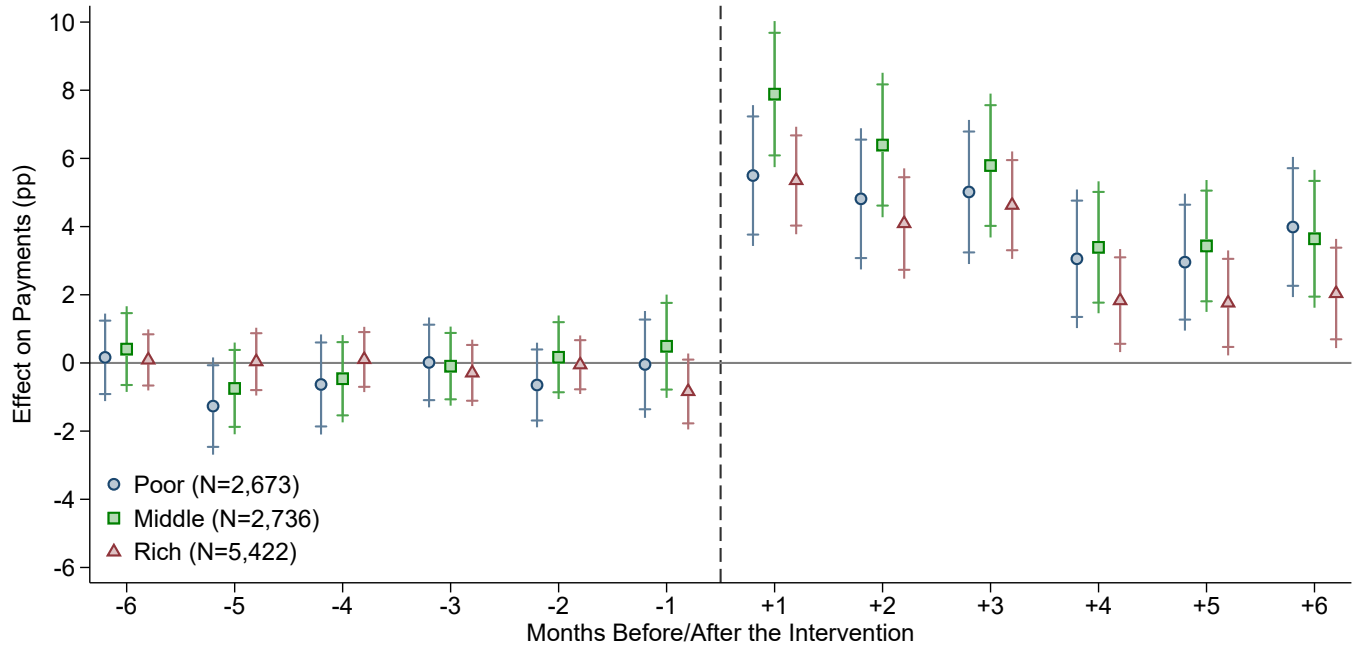
Figure D.2: Difference in Payment Rates between Control and No-Letter Groups in 2023



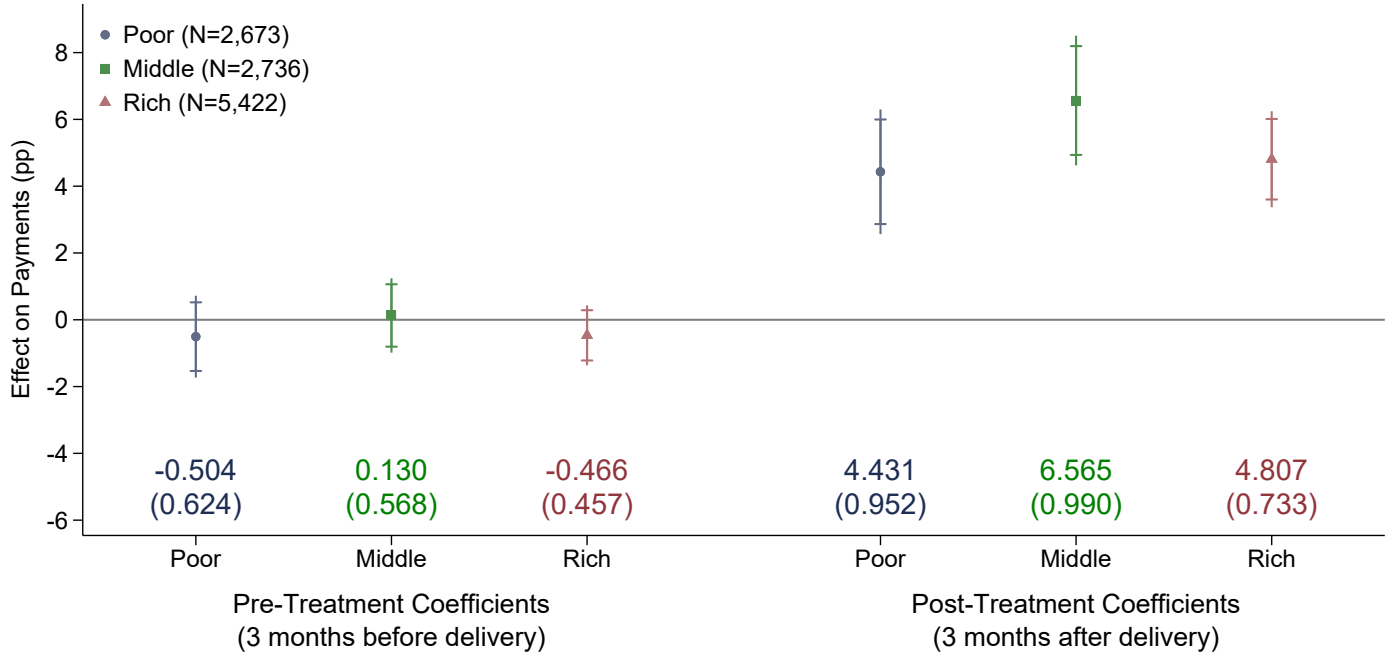
Note: This figure compares the likelihood of paying the monthly bill between treatment and control, normalized by the average pre-treatment difference. We estimate the coefficient for each monthly bill in separate regressions. The sample includes residential and commercial properties and excludes units that made payments for 2023 before we sent the letters.

Figure D.3: The Effect of Receiving a Letter on Own Tax Compliance

(a) Event-Study Analysis (Monthly Level)



(b) Pre- and Post-Treatment Effects (Quarterly level)



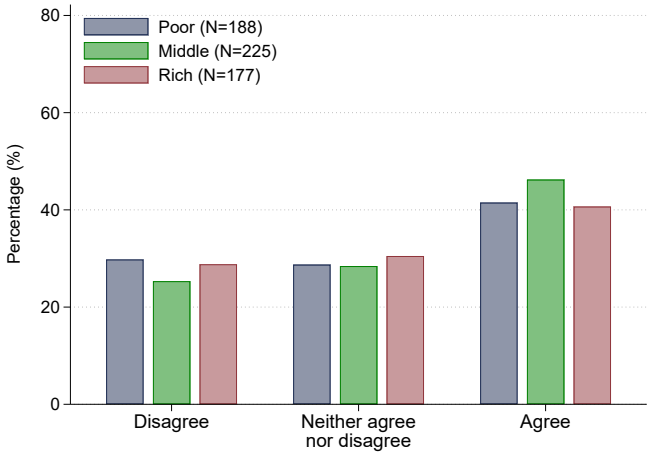
Note: This figure compares the likelihood of paying the monthly property tax bill between the control and the no-letter groups. The control group received a letter with information solely about how the reform changed the household's own tax rate, while the no-letter group received none. Panel (a) shows the dynamic effect of the letters, while panel (b) shows the two months pooled effects of the letter (pre and post-treatment). 'Poor' (in blue) denotes households with properties valued at AR$ 750K or less, 'Middle' (in green) denotes properties valued between AR$ 750K and AR$ 1.5M, and 'Rich' (in red) denotes properties valued more than AR$ 1.5M. We estimate the coefficient for each monthly bill in separate regressions, including the 95% and 90% confidence interval for each group. Clustered standard errors for the pooled estimates in parenthesis. The sample includes only shops (the only ones targeted to receive no letter) and excludes units that made payments for 2023 before we sent the letters. We control for the past 12 months payments in each pre-treatment regression, and for the 2022 monthly payments in each post-treatment regression. Additionally, we include controls for property valuation (in logs), a residential dummy, dummies indicating whether the property was an "always payer" or "never payer" in the previous year, and a dummy for properties that made an annual payment in the previous year. We included dummies equal to one for missing controls to retain all observations in the regressions. Standard errors clustered at the individual taxpayer level.

D.3 Support for Redistribution among Survey Respondents

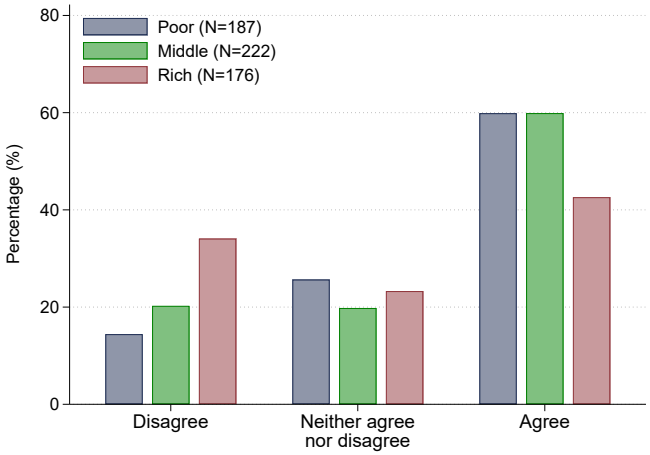
Figure D.4 presents some descriptive statistics from the survey, specifically about the taxpayers' preferences for redistribution. Panel (a) shows support for reducing taxes on low-valuation properties, with respondents evaluating whether lower taxes for the poorest households would be justified despite reducing available funds for public services, while panel (b) examines support for increasing taxes on high-properties properties, asking whether the richest should contribute more to increase public revenue. The results indicate that while a substantial share of respondents agrees with lower taxes for the poor, support is even stronger for raising taxes on the rich. Panel (c) presents responses to a separate question, where participants were asked whether they support a simultaneous tax cut for the poorest and a tax increase for the richest, keeping overall public revenue unchanged, showing that the majority of taxpayers favor a more progressive tax system overall.

Figure D.4: Support for Different Changes in the Tax

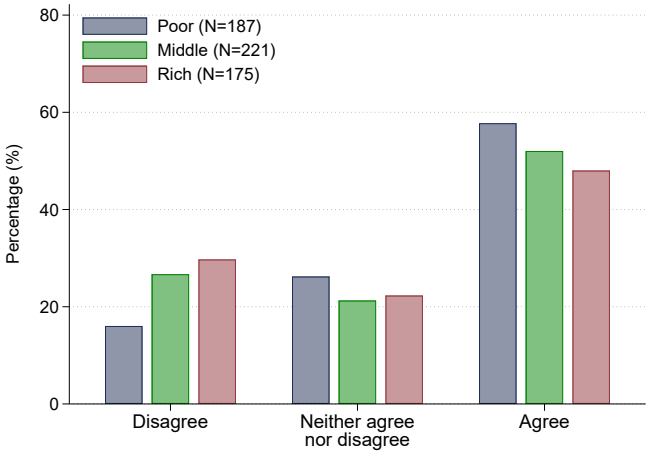
(a) Lower Tax for Poor



(b) Higher Tax for Rich



(c) Lower Tax for Poor, Higher Tax for Rich



Note: Variables constructed from respondents' agreement with statements regarding the fairness of potential TSG reforms, based on the 2023 survey experiment (see Appendix J.2 for the full survey instrument). The original variable ranged from 0 to 10. Respondents who rated between 0 and 4 were classified as "Disagree", those who rated 5 as "Neither agree nor disagree", and those who rated between 6 and 10 as "Agree". Only respondents in the control group are included.

D.4 Experimental Uptake: Alternative Estimates of Reading Rates

The discussion of the results in Section 4.4 indicates that we may adjust our ITT estimated by a letter reading rate to obtain the Average Treatment Effect on the Treated, since it is likely that a significant share of households did not pay attention at all to the second page of the letter. The main body of the paper uses a conservative adjustment factor to accounts for treatment non-uptake of 2, in line to the existing literature (Perez-Truglia and Cruces, 2017; Bottan and Perez-Truglia, 2025; Gerber et al., 2020; Nathan et al., 2020) and with estimates from the Environmental Protection Agency.

In our context, we can use the evidence on awareness of the reform from the survey sample. Specifically, Figure 5 (panels (a) and (b)) present the treatment effect on the probability of knowing about the reform, and we can use this treatment effect as a proxy for experimental uptake. If 19.3% of poor households in the control group were aware of the reform and 100% in the treatment group were exposed to the information, we would expect awareness to increase by 80.7 pp due to our treatment (from 18.8% to 100%). However, we identify an effect of approximately 9.2 pp (panel (b)), implying a reading rate or uptake of 11.4% ($\frac{9.2}{80.7}$), or a non-uptake adjustment factor of around 8.8.

This adjustment factor suggests that the treatment ATET on the poor was 7.1 pp. This high estimate is probably an upper bound we must interpret cautiously, but it complements the lower bound used in the main body of the paper. The reading rates we estimate from the survey have several limitations, including the fact that the specific question used to measure awareness included different options and reforms, potentially confusing some respondents. Moreover, some taxpayers might have known about the progressive reform, but did not pay attention to the survey.

E Comparison of Sample Subgroups

Table E.1 presents summary statistics for key taxpayer attributes across different subgroups. Column (1) describes the full sample of taxpayers with registered property valuations, while column (2) focuses on the main sample used in the experimental analysis. Column (3) reports statistics for taxpayers with registered email addresses, who were eligible for the survey and excluded from the experimental analysis, as discussed in Section 4.2. Finally, columns (4) and (5) compare survey respondents and non-respondents. This table shows that that while survey participants exhibit higher compliance levels, their observable characteristics remain largely comparable to those of the broader sample. Taxpayers in the email sample (column (3)) show significantly higher compliance rates in 2022 compared to the universe (column (1)), with payment shares of approximately 47.9% in the full sample versus 74.1% in the email sample. Survey respondents (column (5)) demonstrate even higher compliance, with a payment share of 84.1%. In contrast, there are smaller differences in other observable characteristics such as assessed property values and tax rates. For example, taxpayers in the universe have a somewhat higher tax rate (2.8%) than those in the email sample (2.37%), shown respectively in columns (1) versus (3).

Table E.1: Summary Statistics by Sample Group

	(1) Universe	(2) Main	(3) All Email	(4) Responded Survey?	(5)
				No	Yes
Share Household (%)	84.3	88.7	89.9	89.4	93.0
	(36.4)	(31.7)	(30.1)	(30.7)	(25.5)
Share of Payments (%)	47.9	47.4	74.1	72.6	84.1
	(45.4)	(45.4)	(35.6)	(36.2)	(28.8)
Assessed Value (AR$)	137.2	133.0	133.1	132.4	137.9
	(589.3)	(583.2)	(182.6)	(191.7)	(104.8)
Tax Rate (%)	2.80	2.75	2.37	2.38	2.28
	(4.04)	(4.12)	(1.39)	(1.43)	(1.08)
Share Poor (%)	37.1	37.3	33.6	34.1	30.7
	(48.3)	(48.4)	(47.2)	(47.4)	(46.1)
Share Rich (%)	26.6	25.8	27.5	27.0	30.6
	(44.2)	(43.7)	(44.6)	(44.4)	(46.1)
Observations	113,956	91,786	13,145	11,422	1,723

Note: This table presents summary statistics for the different sample subgroups. Columns (1) and (2) show the full universe and the main sample, respectively. Column (3) includes taxpayers with registered email addresses, who were eligible for the survey. Columns (4) and (5) compare non-respondents and respondents. Differences in compliance rates are the most pronounced, while other characteristics remain relatively stable across groups. Standard deviations are reported in parentheses. All statistics reflect pre-treatment characteristics from December 2022. However, nine observations in the survey sample are missing from this table, likely due to taxpayers deregistering from the system in 2022.

F Replication of the 2023 Findings in 2024

F.1 Experimental Design in the 2024 Reform

The municipality implemented a second progressive tax reform in 2024. We re-randomized households into treatment and control letters within each of the property value groups – that is, we did not keep the treatment status of 2023 in 2024.[57] A minor difference for the 2024 experiment with respect to 2023 is that we excluded from the treatment and control letters the "empty lot", "civil entities", "religious entities" and "wholesale establishments" categories (around 1% of the sample). In the 2024 design, we also included within each of the poor, middle and rich households groups a small subsample of households that did not receive any letter (3.6% of the total) to gauge the pure effect of receiving a letter (see results from this exercise in Appendix F.4) – in 2024 we included no letter households across the whole sample, whereas the analysis of no letter in 2023 in Appendix D.2 was limited to commercial properties only. Finally, we excluded from the mailing experiment sample the 17.35% of the sample that had a registered email with the Municipality, and we reserved this sample for a survey about perception of taxes and rates which we did not want to contaminate with the information from the treatment and control letters.

This new reform built upon the 2023 setup, maintaining the core principles of increasing progressivity while refining the communication strategy to taxpayers. As explained in Section 4.6 above, we took this opportunity to refine our experimental design by adjusting the 2024 control letter to make the difference between the treatment and control letters even more subtle.

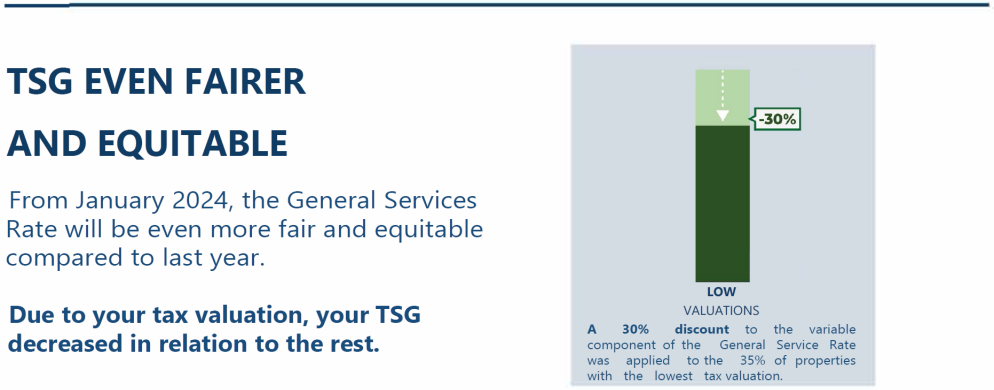
An example of the treatment and control infographic for poor households is shown in Figure F.1, while Appendix I.2 provides a full sample letter along with the treatment and control infographics for middle and rich households. One important detail is the information about the discounts and surcharges. In the 2023 reform, the tax liability adjustments were expressed as a 30% reduction for lower-valuation households and a 15% increase for higher-valuation households in the variable component. The 2024 reform further adjusted the tax system by modifying the fixed component, resulting in an additional approximate 16% reduction for lower-valuation households and an additional increase for higher-valuation households that ranges from approximately 39% to 64%. However, rather than communicating these specific changes, the municipality opted to frame the 2024 policy change as a continuation of the 2023 reform. To ensure continuity and minimize confusion, the municipality framed the reform as a cumulative change in tax liability from 2022 to 2024, rather than focusing on the very technical detail of the

[57] The results from the 2024 experiment discussed below remain virtually unchanged if we control for the household's treatment status in 2023.

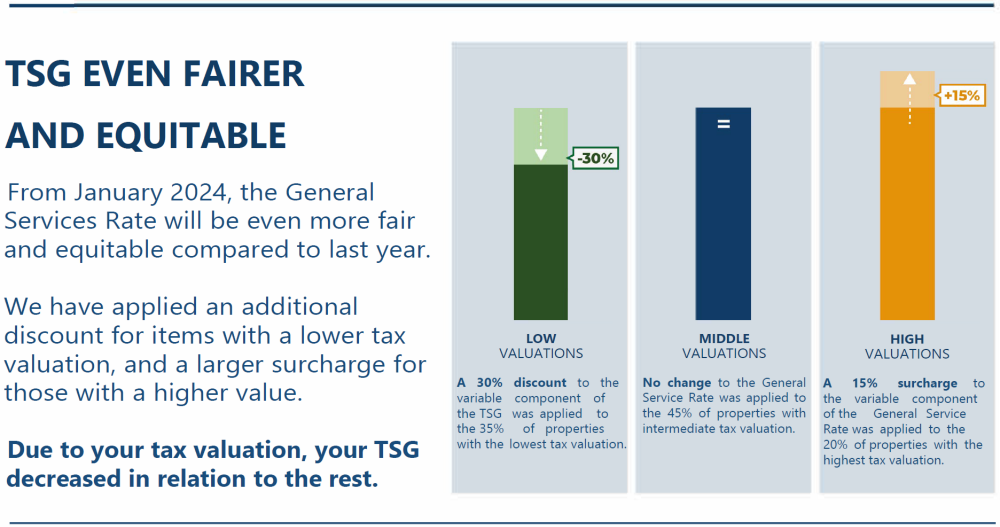
fixed component adjustment introduced in 2024. The justification for this approach was that when considering the total tax liability from 2022 to 2024, the cumulative effect of the reforms translated into an approximate 22.3% reduction for poor households and a 17.7% increase for rich households. Since these numbers closely aligned with the 30% reduction and 15% increase communicated in 2023, the municipality chose to maintain this language to avoid taxpayer confusion. However, while the municipality aimed for clear communication, an unfortunate error occurred during the final letter distribution at the page setup stage. The "small print" below the infographic mistakenly read *variable component* (as in the 2023 letters) instead of *tax liability*. Importantly, this typo was consistent across both treatment and control letters. Given that it appeared in the "fine print", we believe it was likely unnoticed by taxpayers and had a negligible impact on their response to the letters.

Figure F.1: Sample of Control and Treatment Messages: Poor Households, 2024 Experimental Design

(a) Control Letter Message



(b) Treatment Letter Message



Note: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained on the second page of the mailer.

F.2 Own-Rate Effects in the 2024 Reform

In this section, we replicate the regression discontinuity design analysis conducted for the 2023 tax reform to evaluate the effects of the 2024 reform. The 2024 analysis uses the same approach, estimating first- and reduced form effects of the tax changes at the valuation thresholds to assess behavioral responses in terms of tax compliance. This replication serves to validate the robustness of the findings across years and examine any differences in the effects due to contextual variations.

The 2024 reduced-form compliance effects, shown in Figure F.2, closely resemble those from 2023, highlighting the consistency of taxpayer behavioral responses. However, among rich households, the coefficient is similar in magnitude to its 2023 counterpart but is borderline statistically insignificant (p-value = 0.109). Additionally, the first-stage estimates of tax rate changes are slightly larger in 2024, reflecting stronger discontinuities at the thresholds. Figure F.3 presents the placebo tests for 2024, which perform well overall, confirming that the observed effects are not driven by spurious factors. Furthermore, the density estimates in Figure F.4 closely resemble those from 2023.

Finally, we replicate the elasticities estimated in equations (1) and (2) using data from 2024. β^{own} represents the reduced form effect from the RDD, α denotes the first stage effect, C^{own} represents the compliance of middle households (just above or below the cutoff), and τ represents the tax rate of middle households (just above or below the cutoff). Both elasticities are expressed in terms of two-month to two-month changes (given dataset limitations in 2024), comparing periods before and after the reform. Specifically, the percentage change in compliance is derived from the RDD's reduced form results, while the percentage change in tax rates comes from the first stage of the RDD. We establish baseline compliance and tax rates using 2024 levels for middle households, focusing on middle properties situated near the thresholds – above for poor households and below for rich households. The computed elasticities are as follows:[58]

$$\varepsilon^{own}_{poor} = \frac{\frac{\beta^{own}_{poor}}{C^{own}_{poor}}}{\frac{\alpha_{poor}}{\tau_{poor}}} = \frac{\frac{-2.559}{35.3}}{\frac{0.567}{2.252}} = \underset{[-0.529;-0.087]}{\text{-0.288}} \tag{F.1}$$

$$\varepsilon^{own}_{rich} = \frac{\frac{\beta^{own}_{rich}}{C^{own}_{rich}}}{\frac{\alpha_{rich}}{\tau_{rich}}} = \frac{\frac{-2.605}{35.6}}{\frac{0.294}{1.894}} = \underset{[-1.083;-0.038]}{\text{-0.471}} \tag{F.2}$$

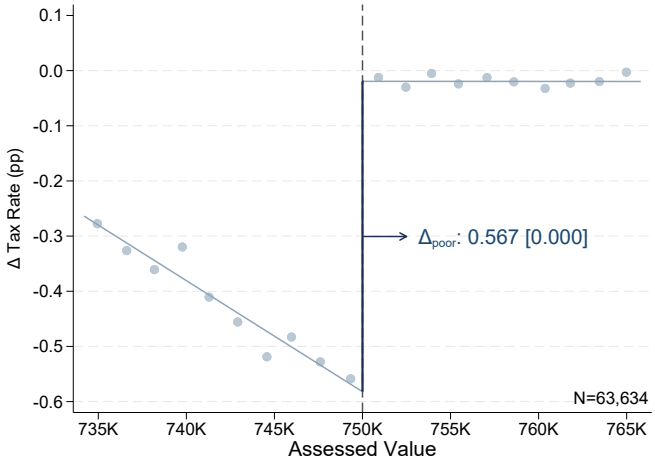
We estimate a significant own-rate elasticity for poor households of -0.288, indicating that for every 1% decrease in their tax rate, compliance increases by 0.288%. Similarly, the own-rate elasticity for rich households is -0.471, implying that for every 1% increase

[58] We also calculated confidence intervals at 90% significance using 5,000 bootstrap iterations for these elasticities.

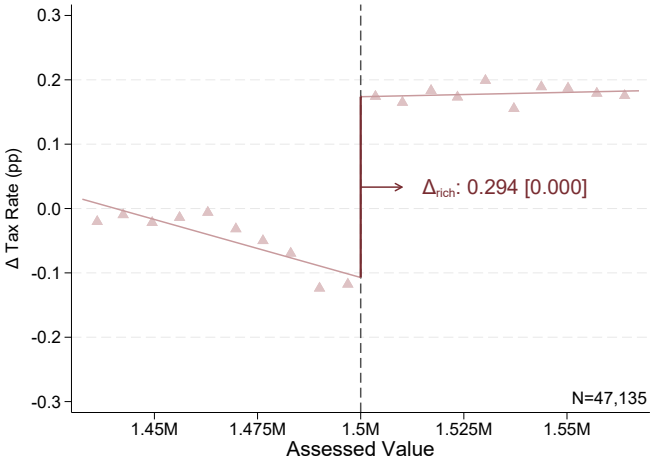
in their tax rate, compliance decreases by 0.471%. To test whether the difference in elasticities between the two groups is statistically significant, we conducted 5,000 bootstrap iterations, which revealed no significant difference (p-value = 0.678).

Figure F.2: 2024 RDD: Own-Rate Effects in Tax Compliance

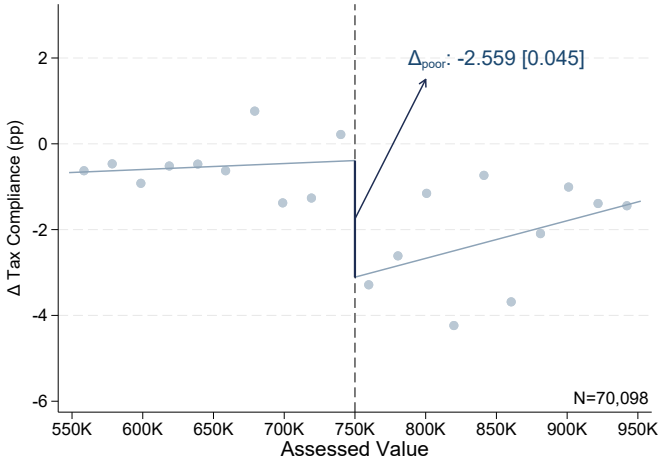
(a) First Stage: Poor Households



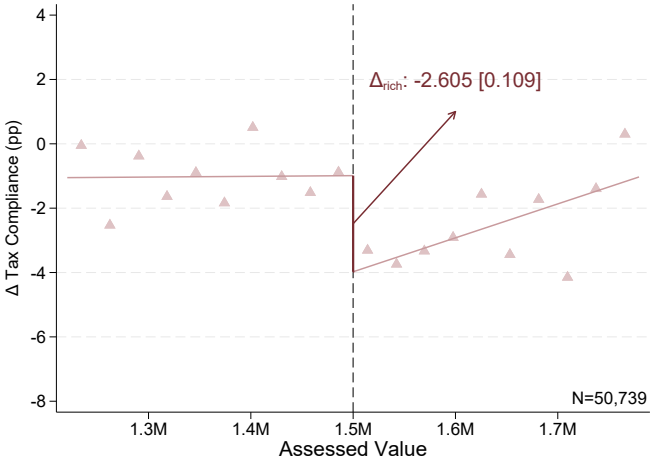
(b) First Stage: Rich Households



(c) Reduced Form: Poor Households



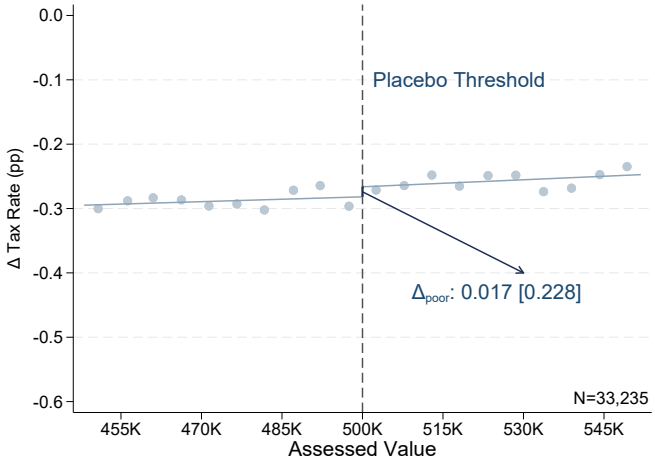
(d) Reduced Form: Rich Households



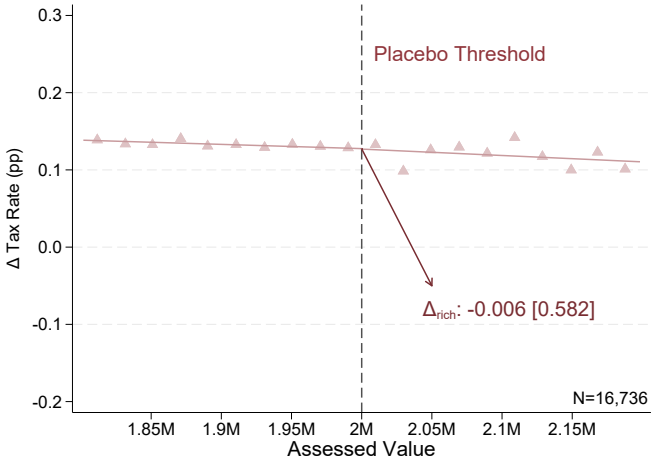
Note: Panels (a) and (b) represent the first stage for poor and rich, in which we evaluate the increase in the amounts owed between the last bimester of 2023 and the fist bimester of 2024, in relation to the value of the property, in the cut-off of AR$ 750K and AR$ 1.5M, respectively. Panels (c) and (d) represent the reduced form for poor and rich, in which we evaluate the change in tax compliance for both groups between the last bimester of 2023 and the fist bimester of 2024. In panels (a), (b), (c) and (d), the x-axis corresponds to the tax valuation of the properties at 2024 values. Each figure indicates the estimated effect around the cut-off point, where the p-value, taken from a robust bias-corrected inference, is indicated in parentheses.

Figure F.3: 2024 Falsification Test: Placebo Thresholds in RDD Analysis

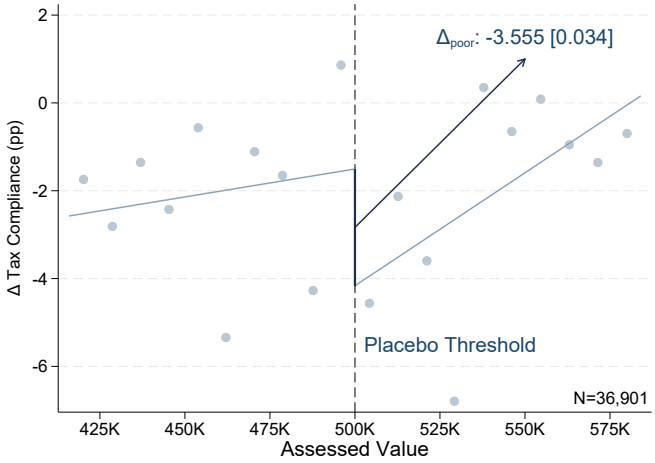
(a) First Stage: Poor Households



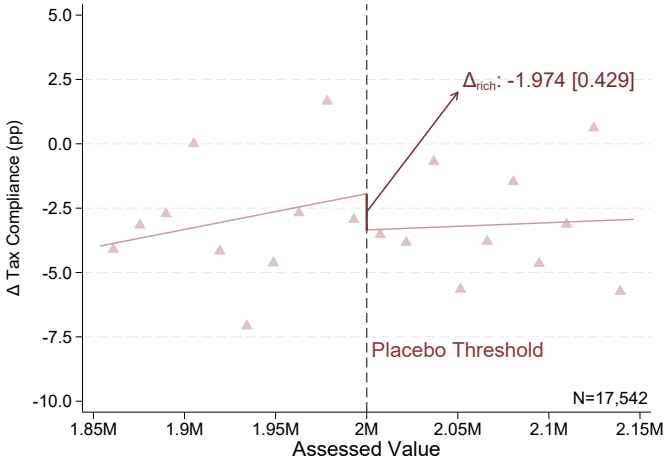
(b) First Stage: Rich Households



(c) Reduced Form: Poor Households



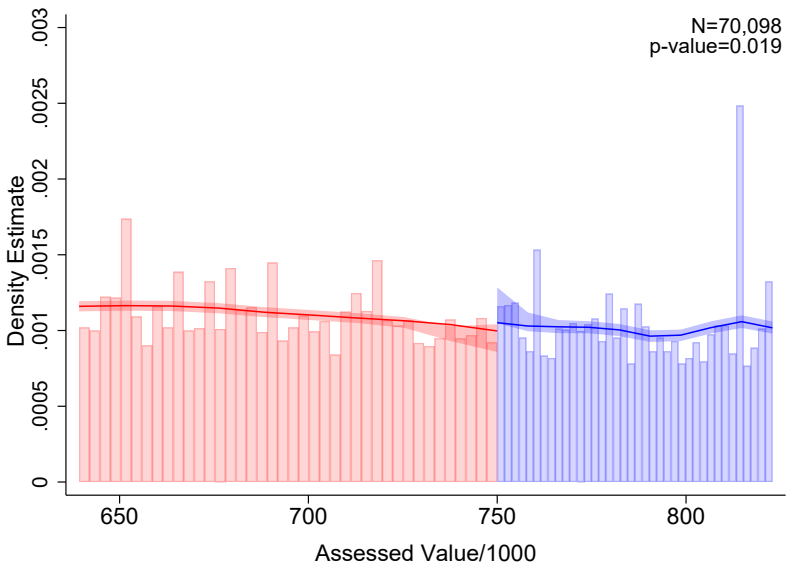
(d) Reduced Form: Rich Households



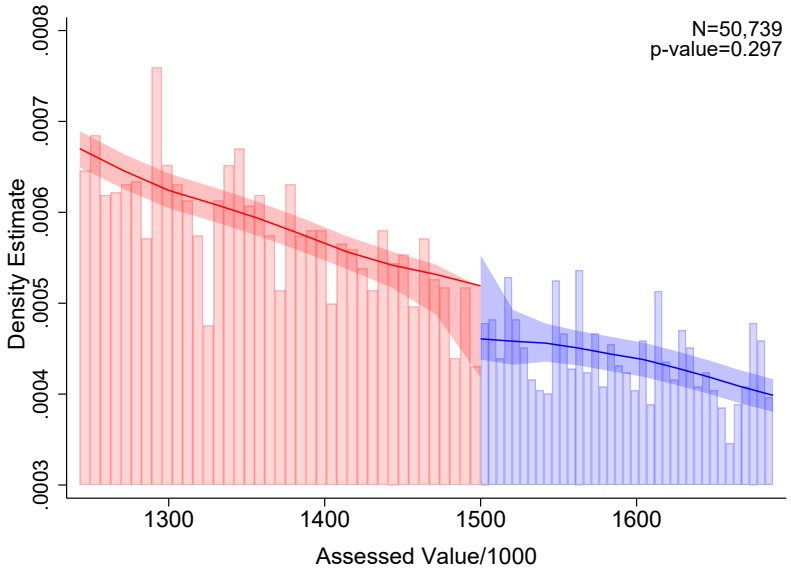
Note: Panels (a) and (b) represent the first stage RDD for poor and rich, in which we evaluate the increase in the amounts owed between the last bimester of 2023 and the fist bimester of 2024, in relation to the value of the property, in the cut-off of AR$ 500K and AR$ 2M, respectively. Panels (c) and (d) represent the reduced form RDD for poor and rich, in which we evaluate the change in tax compliance for both groups between the last bimester of 2023 and the fist bimester of 2024. The x-axis corresponds to the tax valuation of the properties at 2024 values. Each figure indicates the estimated effect around the cut-off point, where the p-value, taken from a robust bias-corrected inference, is indicated in brackets.

Figure F.4: Manipulation Test of Assessed Values around the Discontinuities: 2024

(a) Poor Cutoff - 2024



(b) Rich Cutoff - 2024



Note: Panels (a) and (b) show the McCrary (2008) manipulation test (with their corresponding p-value) of the property assessed values at the low-value threshold (poor) and the high-value threshold (rich) in 2024. The shaded areas represent the 95% confidence intervals. The x-axis represents assessed property value in AR$ thousands.

F.3 Cross-Rate Effects in the 2024 Field Experiment

This section presents estimates of the cross-rate effects by comparing treatment and control groups in terms of tax compliance behavior for the 2024 replication. As Figure F.5 shows, during the months preceding the intervention, compliance rates between the treatment and control groups showed no statistically significant differences, indicating no pre-existing differences in payment behavior. After the letters were sent, however, compliance among lower-value properties in the treatment group increased, aligning with the 2023 findings. Conversely, compliance among high and middle households in the treatment group showed no statistically significant changes.

Figure F.6 provides a detailed analysis of compliance effects, combining a month-by-month evaluation with pooled pre- and post-treatment data. Panel (a) shows that pre-treatment estimates are statistically insignificant across all valuation brackets, confirming the robustness of the experimental design. Post-treatment, poor households exhibit increased compliance in the two months following the intervention, significant at the 10% level, while rich properties show slightly negative but statistically insignificant effects. Panel (b) shows the pooled analysis, analyzing two months before and after the intervention, and similarly reveals no significant pre-treatment differences but does reveal a compliance increase among lower-value properties that received information on the reform's progressivity, with no statistically significant changes for rich properties, consistent with the findings from 2023.

Finally, we also replicate the elasticities estimated in equations (3) and (4) using data from 2024 and our new experimental estimates. β^{cross} represents the effect obtained through the experimental design for each property valuation group, C^{cross} denotes the compliance of each group in 2023, and $\frac{\Delta\tau}{\tau}$ represents the tax rate change of the other group. For simplicity, we characterize the tax changes as a 30% decrease in the tax rate for poor households and a 15% increase for rich households, matching the information conveyed (regarding the fixed component of the tax) in the letters sent. In computing these cross-rate elasticities, we use the percentage rate change of the opposite group in the denominator, reflecting the cross-group nature of the effect. The baseline compliance rates, used as reference points, are derived from the 2024 compliance rates of the control group for each valuation bracket. The computed elasticities are as follows:[59]

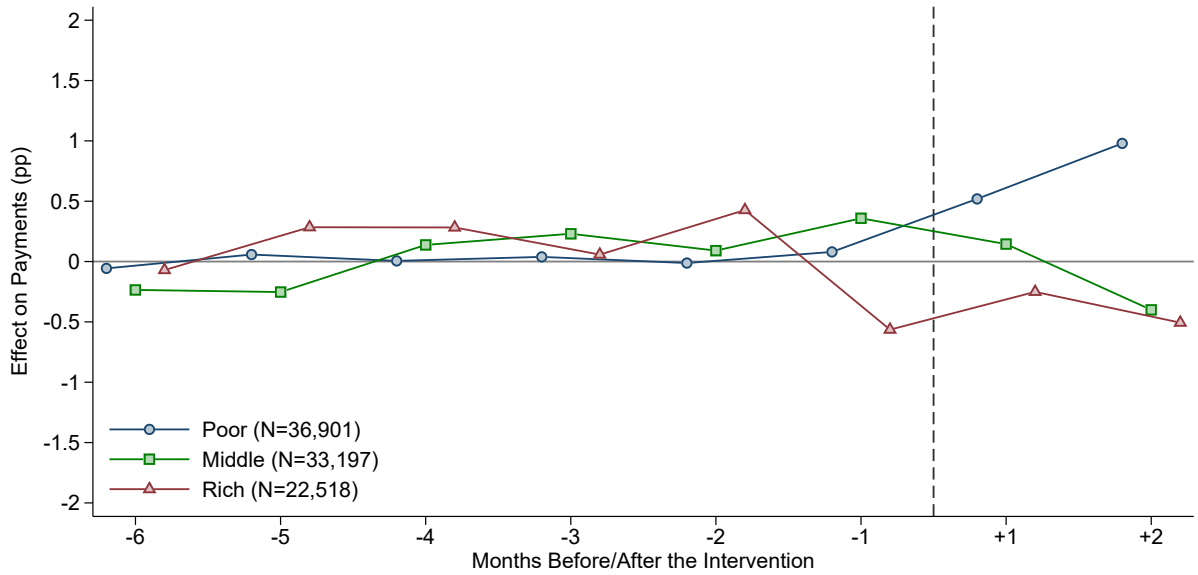
$$\varepsilon^{cross}_{poor} = \frac{\frac{\beta^{cross}_{poor}}{C^{cross}_{poor}}}{\frac{\Delta\tau_{rich}}{\tau_{rich}}} = \frac{\frac{0.721}{38.4}}{0.15} = \underset{[0.025;0.229]}{0.125} \tag{F.3}$$

[59] We also calculated confidence intervals at 90% significance using 5,000 bootstrap iterations for these elasticities.

$$\varepsilon_{rich}^{cross} = \frac{\frac{\beta_{rich}^{cross}}{C_{rich}^{cross}}}{\frac{\Delta\tau_{poor}}{\tau_{poor}}} = \frac{\frac{-0.409}{32.1}}{\text{-0.3}} = \underset{[-0.024;0.110]}{0.042} \tag{F.4}$$

We estimate a significant cross-rate elasticity for poor households of 0.125, indicating that for each 1% increase in the tax rate of the rich households, poor households increase their compliance rate by 0.125% when they become aware of the change in the other group. We also estimate a cross-rate elasticity for rich households of 0.042, although this is not statistically significant at conventional levels. These results are not scaled-up for non-uptake and thus should be interpreted as ITT elasticities. Lastly, we tested whether the difference in elasticities between the two groups is statistically significant, as we did in 2023, but found no evidence of significant differences for this year (p-val=0.271).

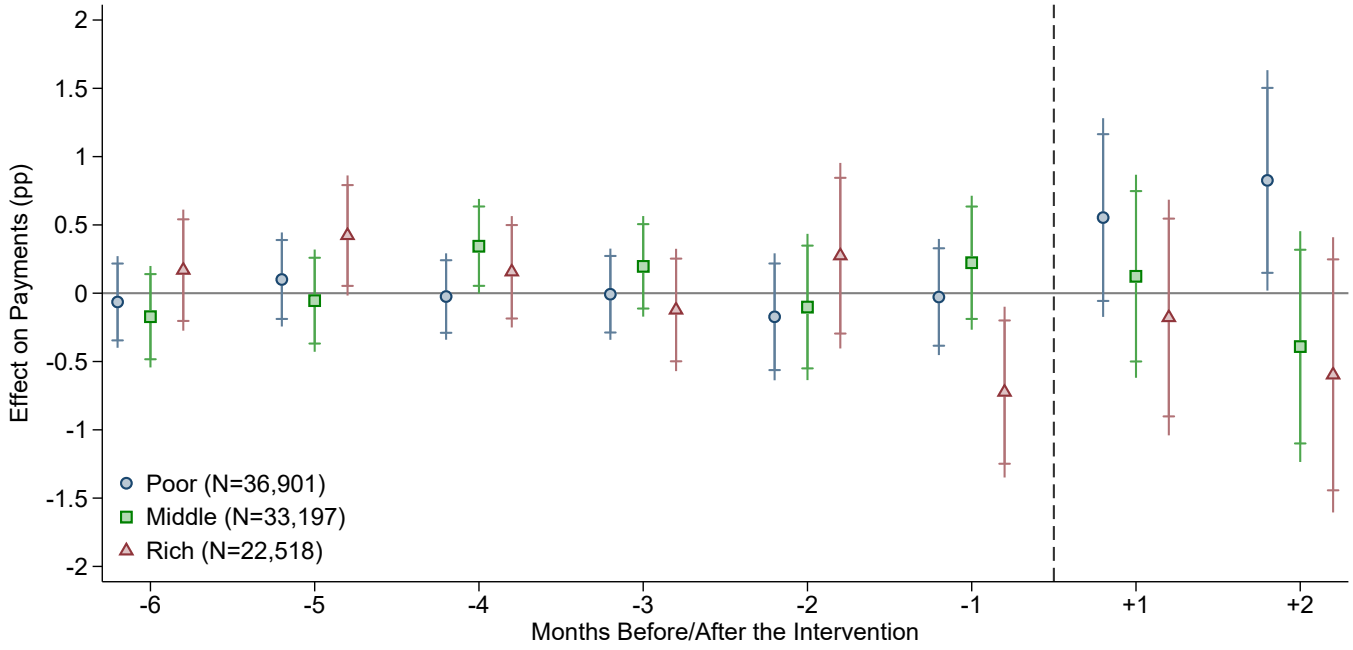
Figure F.5: Difference in Payment Rates between Control and Treatment Letters: 2024



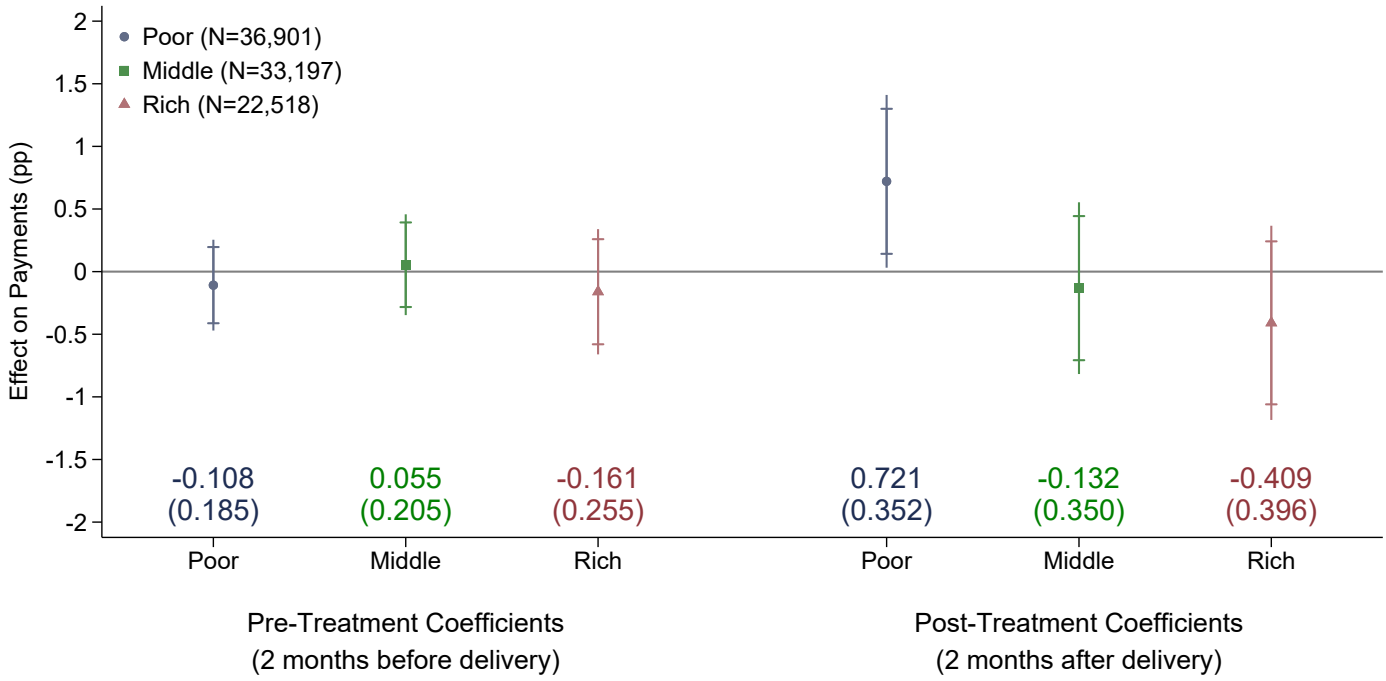
Note: This figure compares the likelihood of paying the monthly bill between treatment and control, normalized by the average pre-treatment difference. We estimate the coefficient for each monthly bill in separate regressions. The sample includes residential and commercial properties and excludes units that made payments for 2024 before we sent the letters.

Figure F.6: Cross-rate Effects of a Progressive Tax Reform on Own Tax Compliance: 2024

(a) Event-Study Analysis (Monthly Level)



(b) Pre- and Post-Treatment Effects (Bimester level)



Note: This figure compares the likelihood of paying the monthly property tax bill between treatment and control for the 2024 experiment. The control group received a letter with information solely about how the reform changed the household's own tax rate. The treatment group letter included additional information about how the reform affected the tax rates of other households (i.e., informing the progressive nature of the reform). See Figure F.1 for an example. Panel (a) shows the dynamic effect of the treatment, while panel (b) shows the two months pooled effects of the treatment (pre- and post-treatment). 'Poor' (in blue) denotes households with properties valued at AR$ 750K or less, 'Middle' (in green) denotes properties valued between AR$ 750K and AR$ 1.5M, and 'Rich' (in red) denotes properties valued more than AR$ 1.5M. We estimate the coefficient for each monthly bill in separate regressions, including the 95% and 90% confidence interval for each group. Clustered standard errors for the pooled estimates in parenthesis. The sample includes residential and commercial properties and excludes units that made payments for 2024 before we sent the letters. We control for the past 12 months payments in each pre-treatment regression, and for the 2023 monthly payments in each post-treatment regression. Additionally, we include controls for property valuation (in logs), a residential dummy, dummies indicating whether the property was an "always payer" or "never payer" in the previous year, and a dummy for properties that made an annual payment in the previous year. We included dummies equal to one for missing controls to retain all observations in the regressions. Standard errors clustered at the individual taxpayer level.

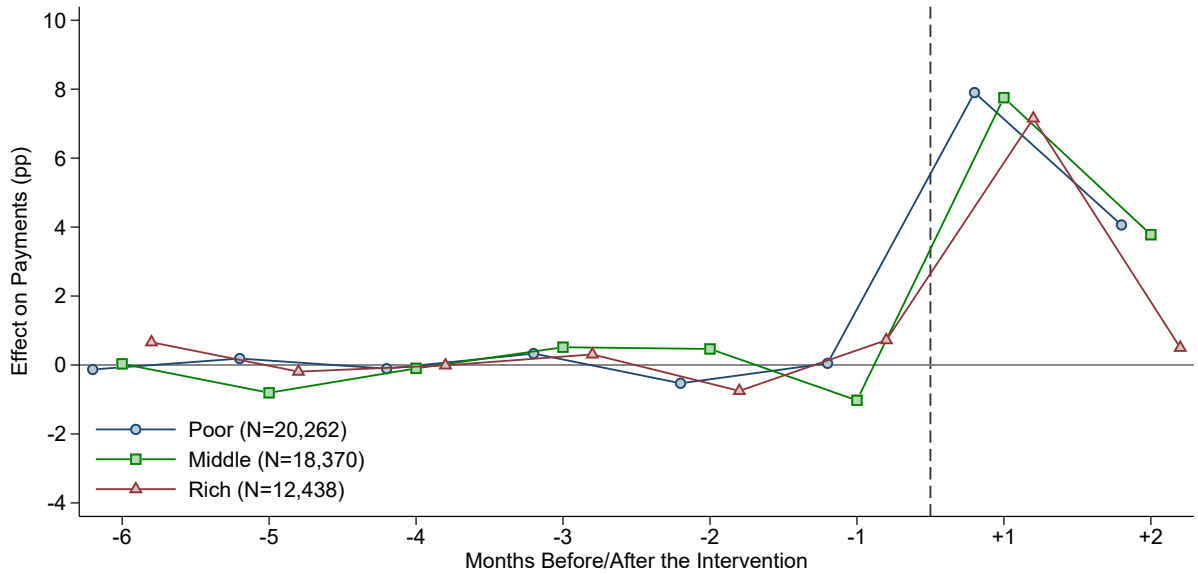
F.4 Control Letter vs No Letter

The analysis of this section replicates the structure and findings of the 2023 analysis, which compared compliance rates between groups receiving control letters and those receiving no letters in Appendix D.2. The results for 2024, illustrated in Figure F.7, show that pre-treatment payment patterns were statistically indistinguishable between these groups, aligning with Figure D.2 in the 2023 analysis. This establishes a valid baseline for assessing the treatment effects.

Post-treatment, properties that received a control letter exhibited higher compliance rates than properties receiving no letter, as seen in panel (a) of Figure F.8. This mirrors the 2023 results, where panel (a) of Figure D.3 demonstrated a clear increase in payments within the first two months after the intervention. For 2024, our data shows compliance increased by approximately 8 percentage points in the first month, with a substantially smaller effect in the second month. Panel (b) of Figure F.8 confirms these patterns when analyzing pooled data, in line with the findings shown in panel (b) of Figure D.3.

In conclusion, the results of this section validate and replicate the findings from the 2023 analysis.

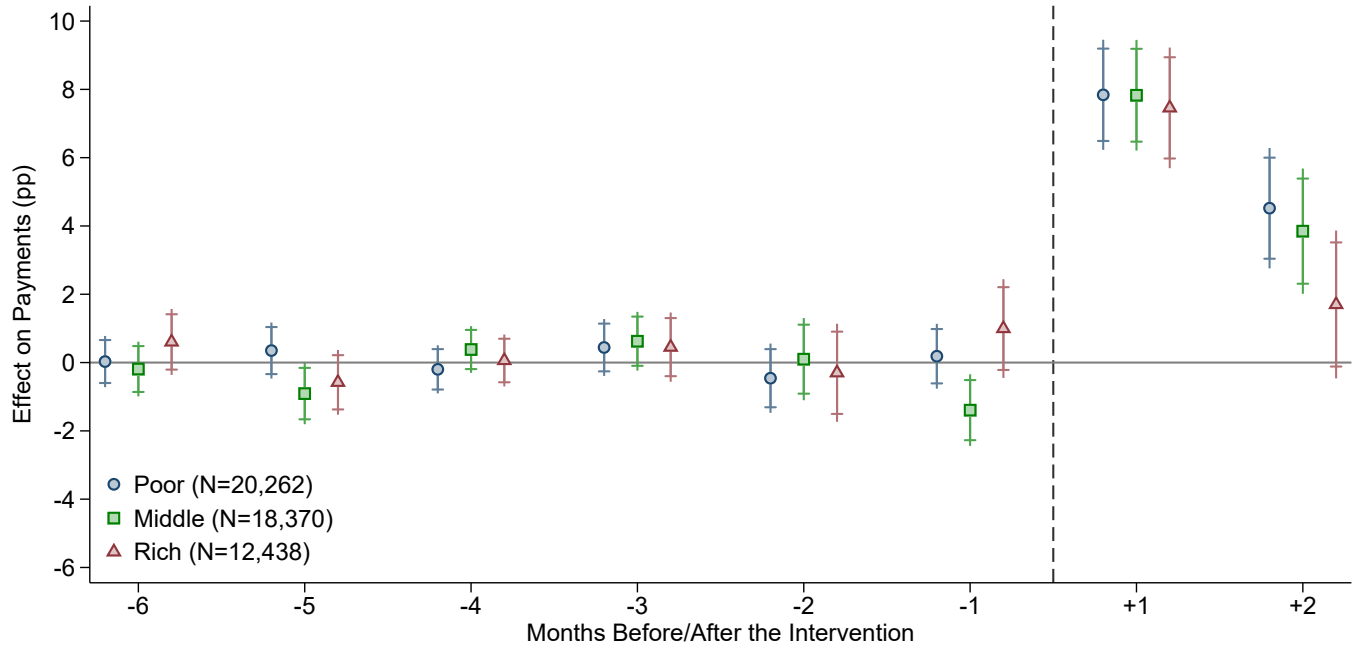
Figure F.7: Difference in Payment Rates between Control and No-Letter: 2024



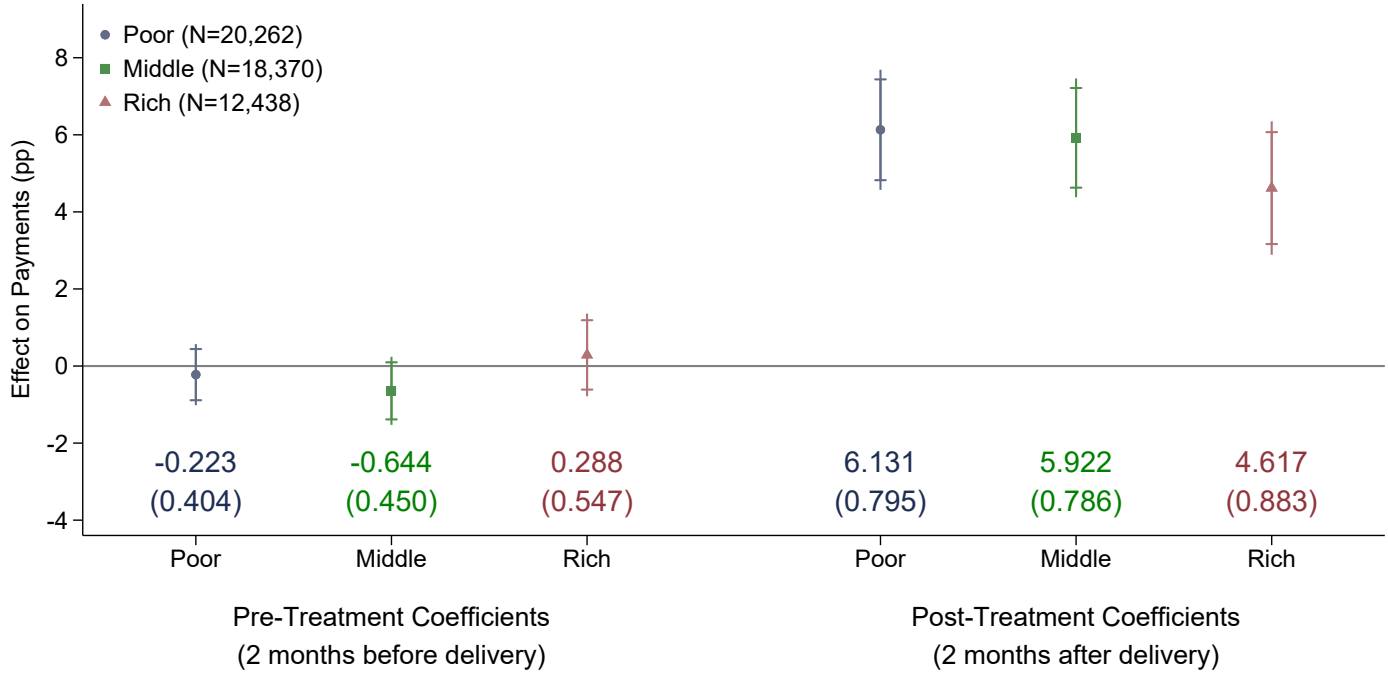
Note: This figure compares the likelihood of paying the monthly bill between treatment and control, normalized by the average pre-treatment difference. We estimate the coefficient for each monthly bill in separate regressions. The sample includes residential and commercial properties and excludes units that made payments for 2024 before we sent the letters.

Figure F.8: The Effect of Receiving a Letter on Own Tax Compliance: 2024

(a) Event-Study Analysis (Monthly Level)



(b) Pre- and Post-Treatment Effects (Bimester level)



Note: This figure compares the likelihood of paying the monthly property tax bill between control and the no letter group. The control group received a letter with information solely about how the reform changed the household's own tax rate, while the no letter group received none. Panel (a) shows the dynamic effect of the letters, while panel (b) shows the two months pooled effects of the letter (pre- and post-treatment). 'Poor' (in blue) denotes households with properties valued at AR$ 750K or less, 'Middle' (in green) denotes properties valued between AR$ 750K and AR$ 1.5M, and 'Rich' (in red) denotes properties valued more than AR$ 1.5M. We estimate the coefficient for each monthly bill in separate regressions, including the 95% and 90% confidence interval for each group. Clustered standard errors for the pooled estimates in parenthesis. The sample includes residential and commercial properties and excludes units that made payments for 2024 before we sent the letters. We control for the past 12 months payments in each pre-treatment regression, and for the 2023 monthly payments in each post-treatment regression. Additionally, we include controls for property valuation (in logs), a residential dummy, dummies indicating whether the property was an "always payer" or "never payer" in the previous year, and a dummy for properties that made an annual payment in the previous year. We included dummies equal to one for missing controls to retain all observations in the regressions. Standard errors clustered at the individual taxpayer level.

G Counterfactual Analysis

G.1 Relative Contributions of Own-Rate and Cross-Rate Effects to the Total Behavioral Response

Our framework provides a way to assess the relative contributions of the cross-rate and own-rate effects to the overall behavioral response to the tax reform. The public finance literature typically only accounts for the own-rate effects (Saez and Zucman, 2023), and this analysis offers a measure of the quantitative importance of these additional effects and the consequences of ignoring them. An advantage of our setup is that it allows us to measure and compare own and social effects within a unified framework and with the same metric.

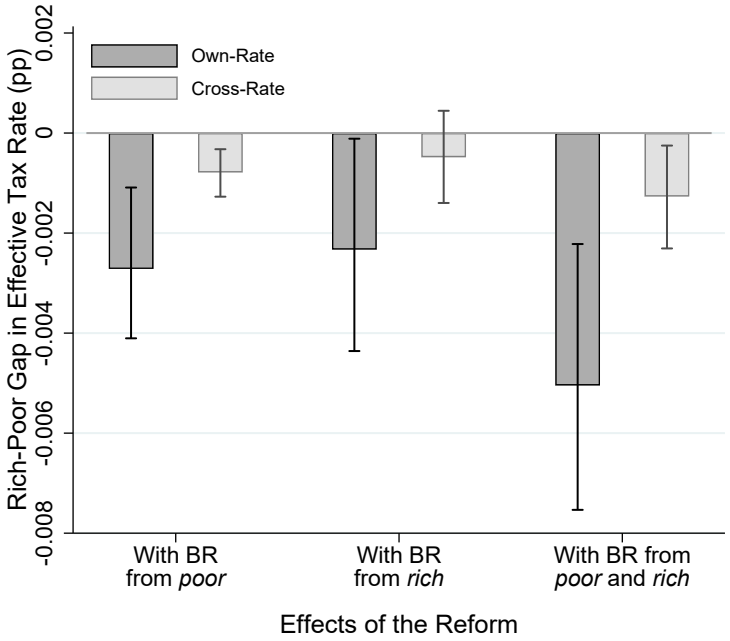
We now present the methodology used to separate the effects of changes in taxpayers' own rates (own-rate effects) from changes in others' rates (cross-rate effects) and their contribution to compliance and revenue outcomes.

Figure G.1 illustrates how these effects drive divergences in these outcomes. Panel (a) of Figure G.1 shows that effective progressivity–adjusted for compliance changes–is significantly lower. This reduction is primarily driven by own-rate effects, with cross-rate effects contributing to a smaller degree. The direction and magnitude of both the own-rate and cross-rate effects are similar for the poor (low property value bracket) and the rich (high property value bracket), with both exhibiting a negative impact on compliance for these groups. The combined result shows a significant decline in the progressivity of the reform.

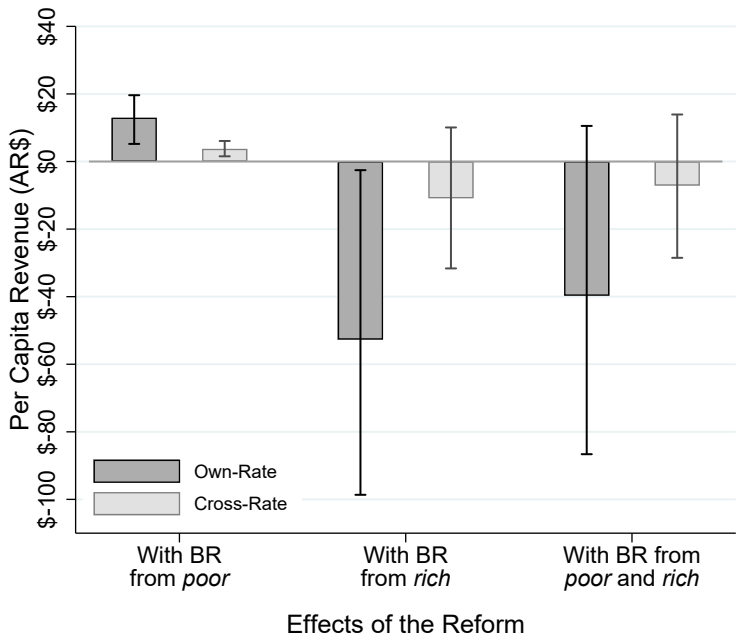
In contrast, panel (b) of Figure G.1 highlights that revenue increases for poor households through both the own-rate and cross-rate channels. Conversely, revenue declines for rich households through these same channels, though the cross-rate effect for rich properties is not statistically significant. When combining the effects across both groups, the negative impact of the own-rate and cross-rate effects tend to dominate. Nevertheless, the combined result lacks statistical precision at conventional significance levels.

Figure G.1: Own-Rate vs Cross-Rate: Rich-Poor Gap and Per Capita Revenue

(a) Rich-Poor Gap: Decomposition



(b) Per Capita Revenue: Decomposition



Note: Estimates made using Administrative Tax Data from Tres de Febrero. Panel (a) shows the decomposition by own-rate and cross-rate of the rich-poor gap, assuming different scenarios of behavioral responses. Panel (b) shows the decomposition by own-rate and cross-rate of the per capita revenue effects, assuming once again different scenarios of behavioral responses. 90% confidence intervals obtained from a 5,000 repetitions bootstrap.

G.2 Additional Details of the Counterfactual Analysis

This section explains the structure and interpretation of the detailed results presented in Table G.1, which summarizes key outcomes of the counterfactual analysis under different scenarios. Each panel represents a specific policy context: the pre-reform scenario, the post-reform scenario without behavioral responses, and the post-reform scenario with behavioral responses that vary by group.

The table is structured along three dimensions: columns, rows, and panels. The columns report key variables. L_j^s represents the average tax liability for each group j in scenario s, while C_j^s denotes the proportion of properties that comply with tax payments on time in the given scenario. The total effective taxes paid by each group, T_j^s, is computed as the product of the average tax liability and the compliance rate.

The rows classify households into three groups based on property valuation: *poor* (low-valuation properties), *middle* (middle-valuation properties), and *rich* (high-valuation properties). Additionally, the table includes a weighted sum, which averages the values across groups based on population shares, and a measure of the rich-poor gap, which captures the difference between the rich and the poor in each scenario. Specifically, the rich-poor gap in taxes paid is computed by normalizing the effective taxes paid by the average valuation of each group.

The panels distinguish between different policy contexts. Panel (a) presents the pre-reform scenario, reflecting the status quo before any policy changes. The weighted sum in this panel represents total per-capita tax revenue, while the rich-poor gap provides a baseline measure of inequality. Panel (b) considers the post-reform scenario assuming no behavioral responses, isolating the mechanical effect of tax rate changes without accounting for adjustments in compliance. In contrast, panels (c), (d), and (e) incorporate behavioral responses, differentiating cases where adjustments in compliance occur only among the poor, only among the rich, or in both groups. These estimates include confidence intervals (e.g., [49.9%; 52.3%]), derived from 5,000 bootstrap iterations.

For our analysis, the most relevant elements of the table are the rich-poor gap and the weighted sum of taxes paid. The rich-poor gap quantifies the difference in taxes paid between the rich and the poor, serving as a key measure of inequality before and after the reform. Meanwhile, the weighted sum represents the average tax revenue per person, which is crucial for understanding the reforms impact on overall tax collection. By comparing these values across the pre-reform and post-reform scenarios, with and without behavioral responses, we can clearly see how changes in compliance and tax rates affect both revenue and inequality.

Table G.1: Counterfactual Analysis: No Behavioral Responses vs Behavioral Responses

	(1) L_j^0	(2) C_j^0	(3) T_j^0
Panel (a): Pre-Reform			
$j = \text{Poor}^{(i)}$	14,161	46.6%	6,597
$j = \text{Middle}^{(ii)}$	22,159	49.3%	10,929
$j = \text{Rich}^{(iii)}$	48,071	50.3%	24,156
Weighted $\text{Sum}^{[(i)+(ii)+(iii)]}$	26,090	48.6%	12,843
Rich-Poor $\text{Gap}^{[(iii)-(i)]}$	33,910	3.7%	−0.313
	L_j^C	C_j^C	T_j^C
Panel (b): Post-Reform without BR			
$j = \text{Poor}^{(i)}$	7,718	46.6%	3,595
$j = \text{Middle}^{(ii)}$	22,159	49.3%	10,929
$j = \text{Rich}^{(iii)}$	56,397	50.3%	28,340
Weighted $\text{Sum}^{[(i)+(ii)+(iii)]}$	25,915	48.6%	12,843
Rich-Poor $\text{Gap}^{[(iii)-(i)]}$	48,679	3.7%	0.5
	L_j^{AP}	C_j^{AP}	T_j^{AP}
Panel (c): Post-Reform, BR from *poor*			
$j = \text{Poor}^{(i)}$	7,718	53.6%	4,137
	–	[50.3%; 56.6%]	[3,880; 4,367]
$j = \text{Middle}^{(ii)}$	22,159	49.3%	10,929
	–	–	–
$j = \text{Rich}^{(iii)}$	56,397	50.3%	28,340
	–	–	–
Weighted $\text{Sum}^{[(i)+(ii)+(iii)]}$	25,915	51.2%	13,044
	–	[49.9%; 52.3%]	[12,949; 13,129]
Rich-Poor $\text{Gap}^{[(iii)-(i)]}$	48,679	−3.3%	0.387
	–	[−6.3%; 0.0%]	[0.339; 0.441]
	L_j^{AR}	C_j^{AR}	T_j^{AR}
Panel (d): Post-Reform, BR from *rich*			
$j = \text{Poor}^{(i)}$	7,718	46.6%	3,595
	–	–	–
$j = \text{Middle}^{(ii)}$	22,159	49.3%	10,929
	–	–	–
$j = \text{Rich}^{(iii)}$	56,397	45.2%	25,473
	–	[41.0%; 49.4%]	[23,149; 27,862]
Weighted $\text{Sum}^{[(i)+(ii)+(iii)]}$	25,915	47.2%	12,079
	–	[46.1%; 48.3%]	[11,461; 12,715]
Rich-Poor $\text{Gap}^{[(iii)-(i)]}$	48,679	−1.4%	0.373
	–	[−5.5%; 2.8%]	[0.271; 0.479]
	L_j^A	C_j^A	T_j^A
Panel (e): Post-Reform, BR from *poor* and *rich*			
$j = \text{Poor}^{(i)}$	7,718	53.6%	4,137
	–	[50.3%; 56.6%]	[3,880; 4,367]
$j = \text{Middle}^{(ii)}$	22,159	49.3%	10,929
	–	–	–
$j = \text{Rich}^{(iii)}$	56,397	45.2%	25,473
	–	[41.0%; 49.4%]	[23,149; 27,862]
Weighted $\text{Sum}^{[(i)+(ii)+(iii)]}$	25,915	49.8%	12,280
	–	[48.1%; 51.4%]	[11,652; 12,918]
Rich-Poor $\text{Gap}^{[(iii)-(i)]}$	48,679	−8.4%	0.260
	–	[−13.5%; −2.9%]	[0.147; 0.379]

Note: Estimates made using Administrative Tax Data from Tres de Febrero. Panel (a) shows the pre-reform estimates. Panel (b) shows the post-reform estimates without behavioral responses. Panel (c) shows the post-reform estimates with behavioral responses for poor households only. Panel (d) shows the post-reform estimates with behavioral responses for rich households only. Panel (e) shows the post-reform estimates with behavioral responses for both groups. Additionally, to compute the rich-poor gap of the paid taxes we divided the effective taxes paid by each group for the average valuation of each group. 90% confidence intervals obtained from a 5,000 repetitions bootstrap.

H Forecast Survey

This section presents the findings from a forecast survey conducted with 39 experts to evaluate their predictions about the effects of the progressive tax reform on tax compliance. The survey provided participants with comprehensive information about the experimental design, including details about the tax reform, randomized messaging interventions, and the primary outcome measure: changes in tax compliance across valuation brackets. Experts were asked to estimate the expected effects and indicate their confidence in these predictions (the full questionnaire can be found in Appendix J.3).

The sample of experts consisted primarily of senior academics, with 27 Professors, 8 Assistant Professors, and 4 researchers and postdoctoral fellows, as shown in Figure H.1, panel (a). Most participants had backgrounds in Economics, with a smaller number coming from Business (panel (b)). Notably, approximately 85% of the experts reported previous research experience in taxation, while 40% conducted research on preferences for redistribution, providing a strong foundation for making informed predictions (panels (c) and (d)).

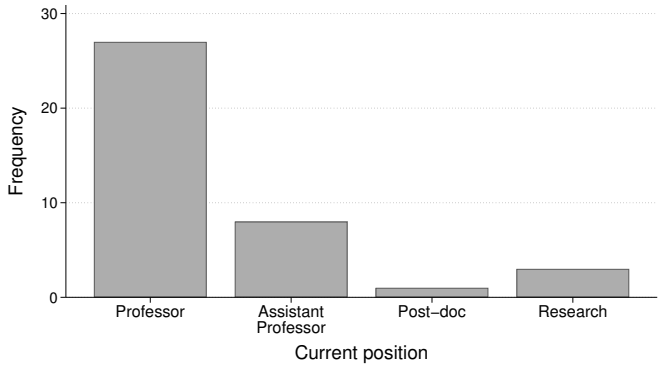
Figure H.2 shows that experts' predictions revealed significant variability across valuation groups. For poor properties, the majority expected a positive compliance response, attributing this to increased perceptions of fairness arising from the reform. However, for middle properties, predictions were more mixed, with some experts expecting no change and others anticipating increases in compliance. For rich properties, opinions varied significantly: while some predicted slight improvements in compliance, others forecasted decreases or no changes in behavior, reflecting uncertainty about the role of fairness perceptions in this group.

The survey revealed substantial dispersion in the predicted treatment effects, both in terms of magnitude and direction. Only a small number of experts predicted outcomes that aligned closely with the actual experimental findings. Furthermore, the majority of experts expressed low confidence in their predictions, describing themselves as only "slightly confident" or "somewhat confident" across all valuation brackets.

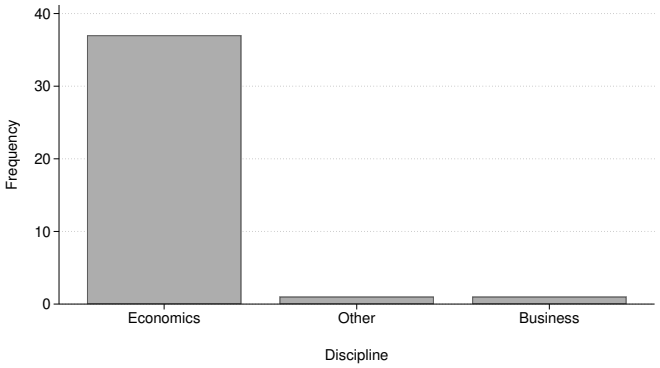
Overall, the survey results underscore the distinctive nature of the experimental findings. Although experts accurately anticipated positive compliance effects among poor properties, they significantly overestimated the strength of the treatment's impact. In contrast, the responses for medium- and high-value properties were less predictable, with expert forecasts diverging from the observed results. These findings highlight the importance of empirical evidence in capturing the effects of informing taxpayers about progressivity reforms.

Figure H.1: Descriptive Statistics: Forecast Survey

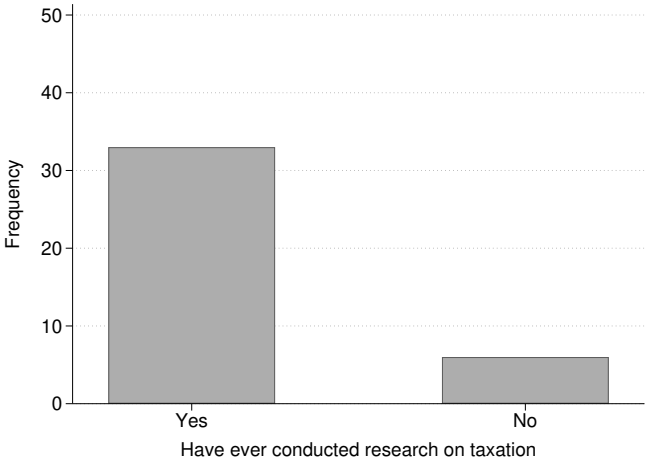
(a) Current Positions of Participants



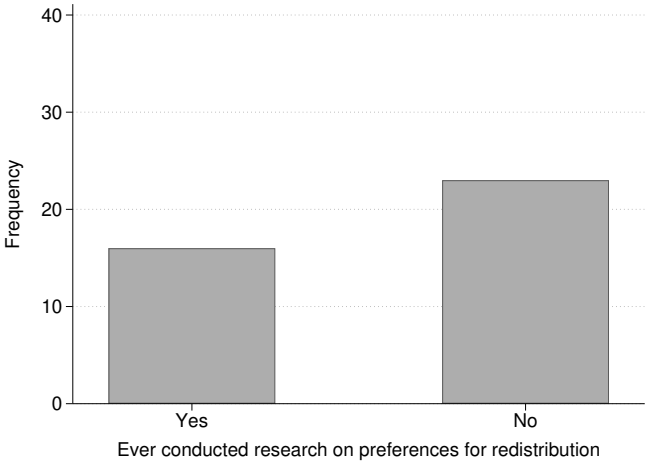
(b) Disciplines of Participants



(c) Research on Taxation



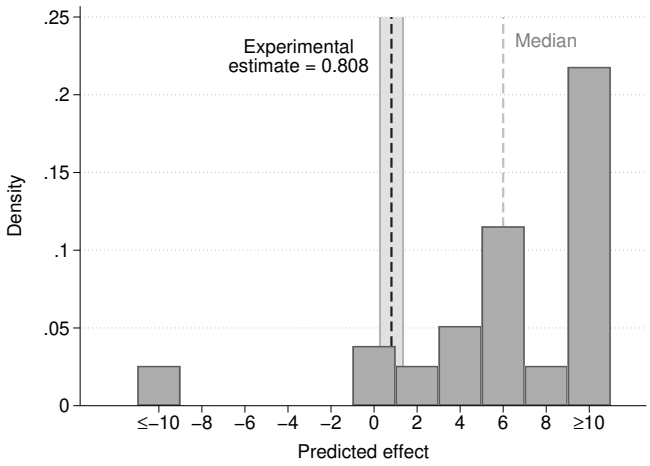
(d) Research on Redistribution



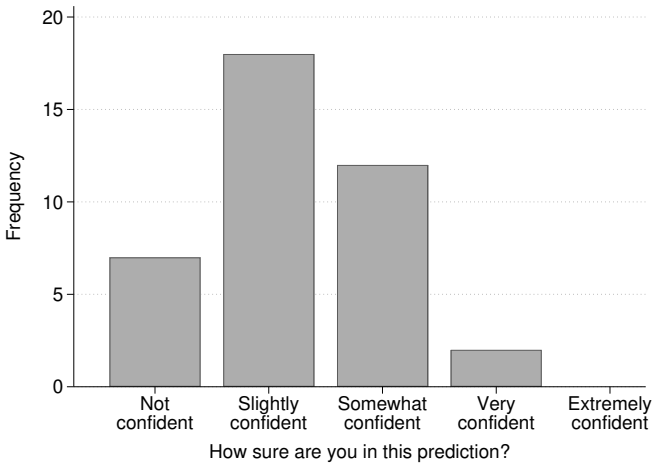
Note: These figures summarize the descriptive statistics of the expert sample from the forecast survey. Panel (a) shows the distribution of participants' current positions, while panel (b) presents the disciplines they specialize in. Panels (c) and (d) display the proportion of participants who have conducted research on taxation and preferences for redistribution, respectively.

Figure H.2: Predictions and Confidence Levels: Forecast Survey

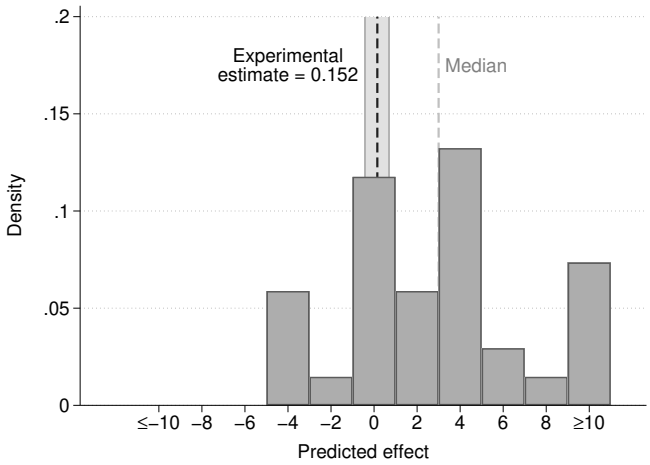
(a) Predictions: Poor-Households



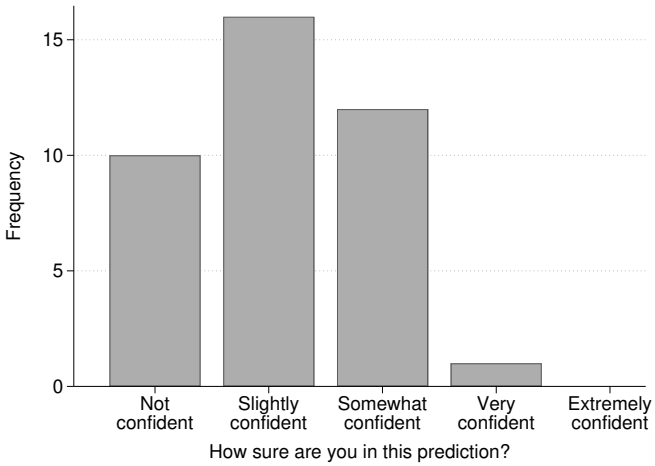
(b) Confidence: Poor-Households



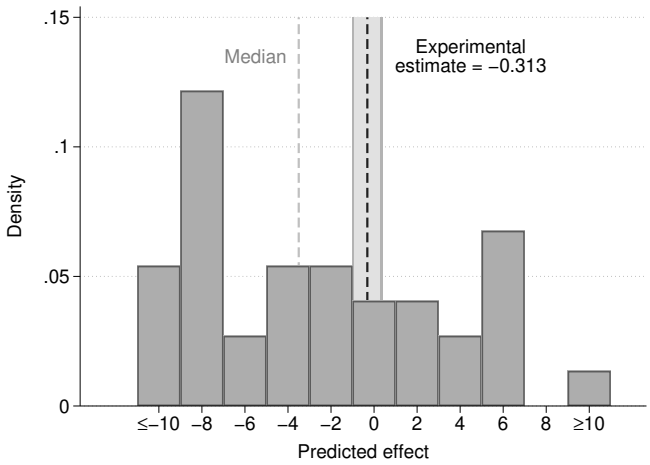
(c) Predictions: Middle-Households



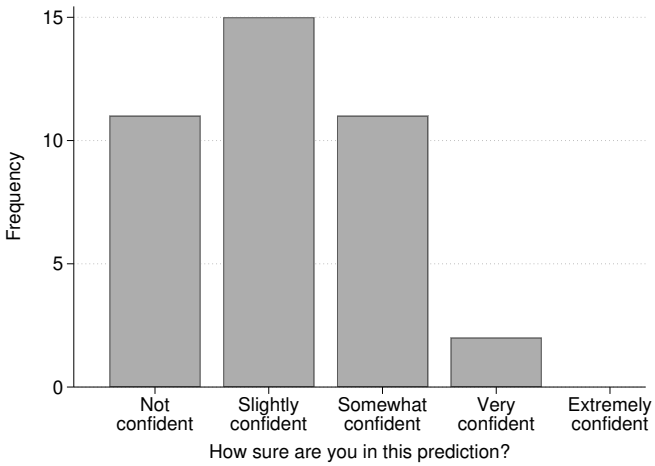
(d) Confidence: Middle-Households



(e) Predictions: Rich-Households



(f) Confidence: Rich-Households



Note: Panels (a), (c), and (e) show the distribution of predictions for poor, middle, and rich households, respectively. Panels (b), (d), and (f) illustrate the levels of confidence experts had in their predictions for each valuation group. The results reflect the expectations and uncertainty regarding the effects of the tax reform on compliance across property valuations. Shaded areas around the experimental estimate correspond to the 90% and 95% confidence intervals.

I Sample Letters

I.1 Sample Letters: 2023 Field Experiment (in English)

This section provides an English translation of the sample letters used in the 2023 field experiment, detailing the messaging strategies and content included in the communications. Both letters contained procedural information to facilitate taxpayer understanding and compliance, which included a detailed breakdown of monthly tax amounts, specific payment deadlines, and contact information for a helpline and email support. Importantly, our intervention through letters was designed to be subtle enough not to generate experimenter demand effects.

On the one hand, the control letter offered a straightforward summary of the tax changes, focusing exclusively on the recipient's situation. Crucially, the control letter avoided references to the reform's broader redistributive goals, maintaining a neutral tone that limited the information to immediate personal implications.

On the other hand, the treatment letter included the same personalized details as the control letter, but expanded its scope to emphasize the progressive nature of the tax reform. It explicitly framed the changes as part of a broader initiative to improve the progressivity of the tax system. The letter detailed not only how the reform affected the recipient, but also highlighted its impact on other taxpayer groups. This was designed to affect perceptions of fairness and encourage compliance. To enhance clarity and engagement, the treatment letter incorporated graphical elements that visually depicted changes in tax rates across different property valuation brackets.

The differences in information between the control and treatment letters were central to the experimental design. By comparing the responses to these letters, the study aimed to isolate the impact of emphasizing fairness and redistribution on taxpayer behavior.

Municipality of Tres de Febrero

Tasa por Servicios Generales

MR/MRS: S[redacted]O ALBERTO J.

ADDRESS:
[redacted]L 399 - JOSÉ LEÓN SUÁREZ 1655

LOCATION:
[redacted]985

ACCOUNT: [redacted]

CADASTRAL NOMENCLATURE
Cir: 4 [redacted]1

RATE: 0.0121 **CATEGORY:** 1

VALUATION: $682.124

Monthly calculation

GENERAL SERVICES	$	1.100,50
PROGRESSIVITY CORRECTION	$	-330,15
ADD. SECURITY	$	400,00
ADD. HEALTH	$	320,00
ADD. CEDEM MAINTENANCE	$	232,00
ADD. VOLUNTEER FIREFIGHTERS	$	23,20
PUBLIC LIGHTING ANTICIPATION	$	-877,00
TOTAL:	**$**	868,55

ANNUAL PAYMENT

TWO MONTHS DISCOUNT

DUE DATE 15/02/2023
AMOUNT $ 8.685,50

2000001[redacted]230215

Scan the QR and pay through Mercado Pago

[redacted]

FIRST THREE MONTHS PAYMENTS

If you made the annual payment, waive the following fees

JANUARY

DUE DATE 15/02/2023 **AMOUNT** $ 868,55

2023001[redacted]30215

[redacted]

FEBRUARY

DUE DATE 15/02/2023 **AMOUNT** $ 868,55

2023002[redacted]30215

[redacted]

MARCH

DUE DATE 15/03/2023 **AMOUNT** $ 868,55

2023003[redacted]30315

[redacted]

This is the annual invoice for the TSG and for the first three months of 2023. It is the only time in the year that you will receive it printed at home. Keep in mind that you can download it and pay for it from **mi3f .tresdefebrero.gov .ar.**

Remember that if instead of paying the annual fee you do it month by month and you have no debt, you will receive a bonus for July's fee.

Municipality of Tres de Febrero

LESS TAXES, MORE WORK

At Tres de Febrero we are on the side of those who produce and generate employment, that is why we take measures that benefit those who invest.

FREE ABILITATIONS

For any sector or size.



NO OBSTRUCTIONS OR BUREAUCRACY

We eliminated more than 400 fees and simplified 30 thousand procedures per year.

WE HELP THOSE WHO GIVE WORK

Credits and tax benefits for those who hire Tres de Febrero workers.

---

mi3F

SIMPLER FOR YOU

All your experience with the Municipality in one place

With your user you can:

- Load urban reports from your neighborhood
- Pay your municipal taxes
- Carry out procedures online

Desde tu usuario podés:

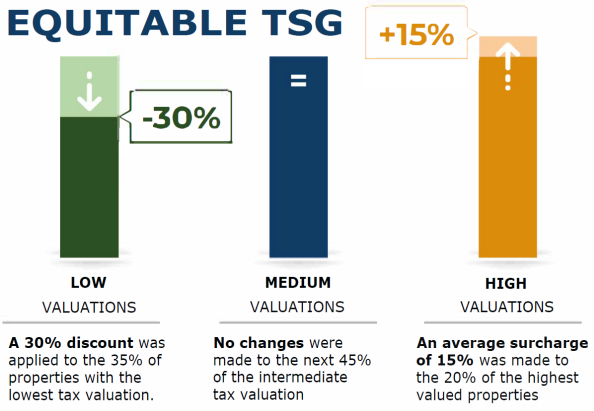
Register for one time only at **mi3f.tresdefebrero.gov.ar**

FAIRER AND MORE EQUITABLE TSG

The TSG **increased below inflation** as in recent years and also now it is **fairer and more equitable**.

We applied a 30% discount on the variable component of the TSG for properties with the lowest tax valuation, and an average increase of 15% for those with the highest value.

Based on your tax valuation, your TSG increased in relation to the rest.

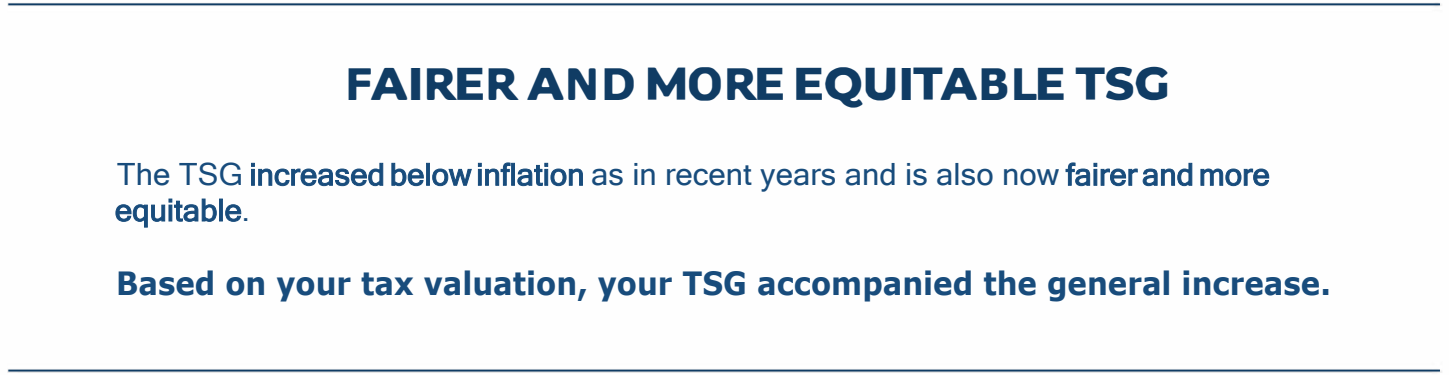


TELL US WHAT YOU THINK ABOUT OUR TAX POLICY

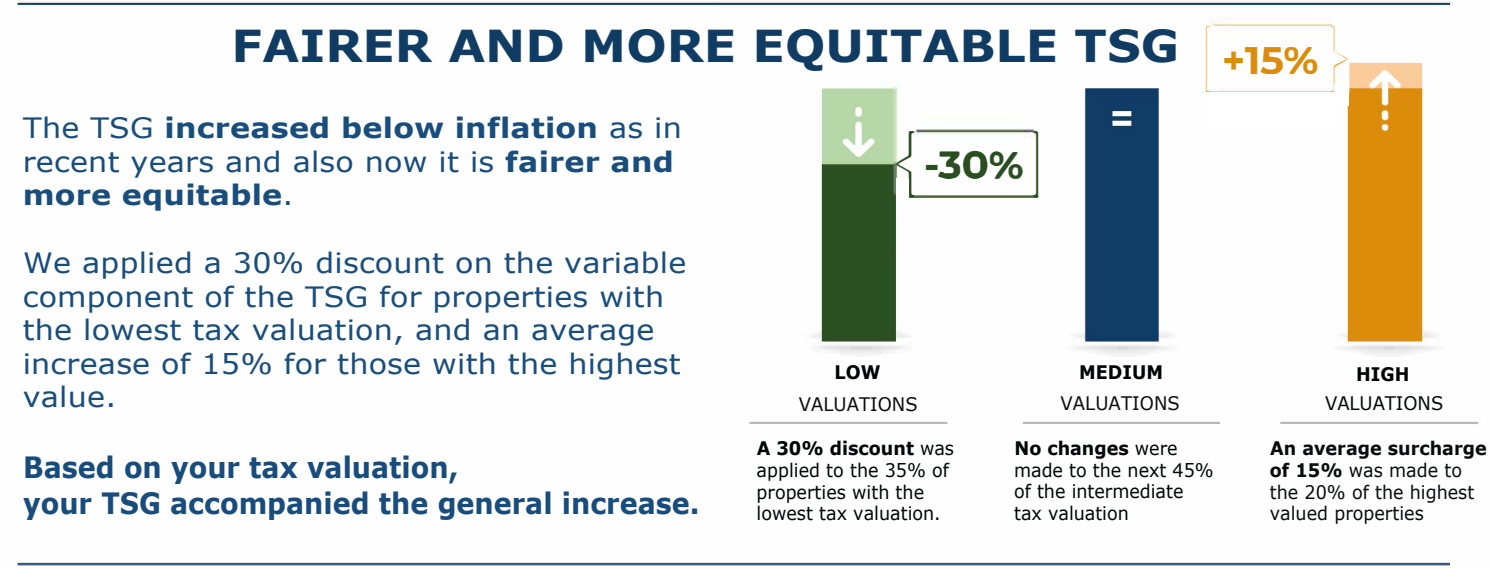
Knowing what you thought helps us improve.
Scan the QR or go to opinion.tresdefebrero.gov.ar/tasas2023

Figure I.1: Sample of Control and Treatment Messages: Middle Households

(a) Control Letter Message



(b) Treatment Letter Message



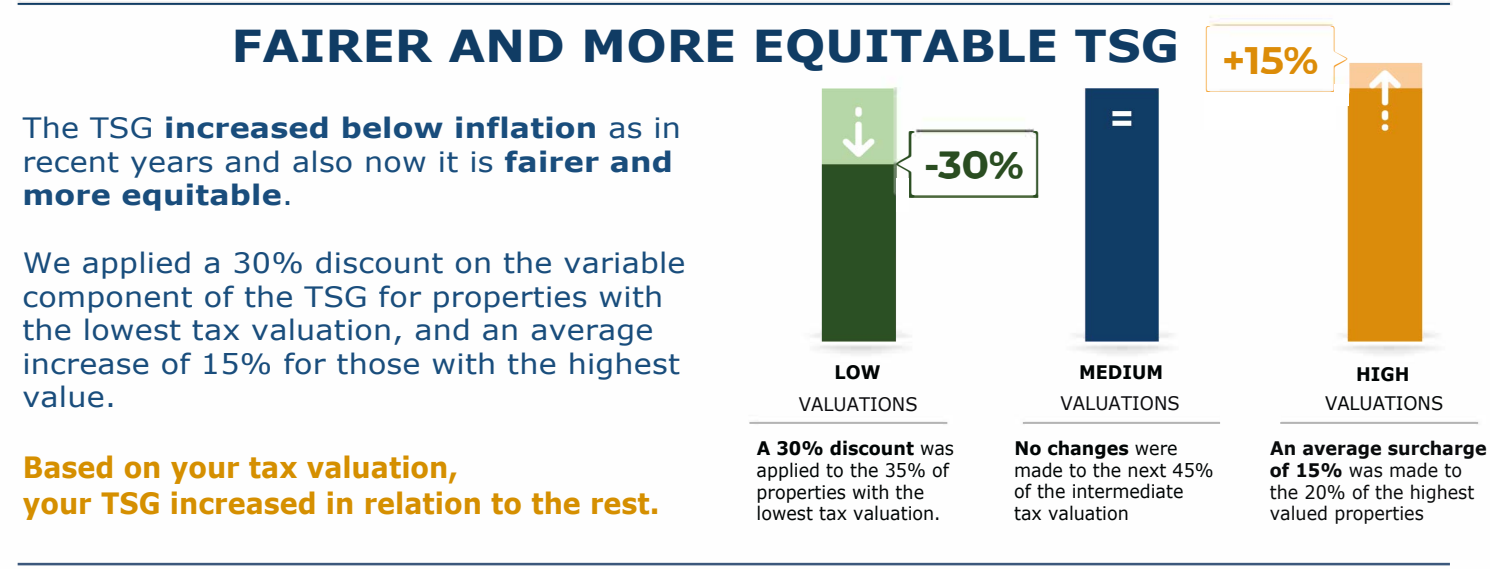
Note: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained in the second page of the mailer.

Figure I.2: Sample of Control and Treatment Messages: Rich Households

(a) Control Letter Message



(b) Treatment Letter Message



Note: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained in the second page of the mailer.

I.2 Sample Letters: 2024 Field Experiment Replication (in English)

This section provides an English translation of the sample letters used in the 2024 field experiment, which closely resembled those from 2023 with only minor refinements. The treatment letters for both years were substantively identical, emphasizing the progressive nature of the tax reform and its broader redistributive goals. These letters detailed not only the recipient's tax changes but also the reform's impact on other taxpayer groups, accompanied by graphical elements that visually represented the adjustments across property valuation brackets.

However, the control letter differed between 2023 and 2024. In 2023, the control letter informed recipients whether their tax rate had decreased, remained unchanged, or increased, but without specifying the magnitude of the change. In 2024, the control letter provided additional detail by explicitly stating the exact percentage change in the recipient's tax rate. Additionally, the 2024 control letter included a small graphical representation illustrating how the reform affected tax rates for different taxpayer brackets (poor, middle, and rich properties).

This modification of the 2024 control letter provided an opportunity to examine whether detailed quantitative information and visual representations influenced taxpayer behavior differently compared to the categorical information provided in 2023. By maintaining identical treatment letters while enhancing the control letter's content, the 2024 experiment enabled us to isolate the effects of different presentations of the progressive reform on tax compliance through social preference channels.

Municipality of
Tres de Febrero

Tasa por Servicios Generales

ACCOUNT:

MR/MRS:

IN CHARGE:

LOCATION:

ADDRESS:

CADASTRAL NOMENCLATURE

Cir: Sec: Cod Mz: Mz: Pc: UF:

CATEGORY:

MODULE VALUE: **RATE:**

VALUATION:

Monthly calculation

MONTHLY TOTAL: $

Amount owed to date

PREVIOUS PERIODS:
CURRENT *MORATORIAS*:

TOTAL: $

The debt mentioned is for informational purposes. Go to **tresdefebrero.gov.ar/tsg** to find out the payment facilities available.

Payment methods

Pago Fácil · mercado pago · LINK · PagoMisCuentas Banelco · rapipago · Banco Nación · Banco Provincia

Or come from 9 a.m. to 3 p.m. at:
- **Municipality** (CAV hall): Alberdi 4840 - Caseros
- **Ciudadela Municipal Headquarters** : Av. Rivadavia y San Martín
- **Neighbor care center**

Remember that you can also pay at **tasas.tresdefebrero.gov.ar** and join the automatic debit through **mi3f.tresdefebrero.gov.ar**

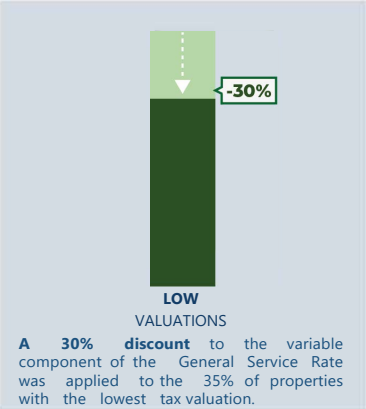
Due Date AMOUNT $

Municipality of
Tres de Febrero

TSG EVEN FAIRER AND EQUITABLE

From January 2024, the General Services Rate will be even more fair and equitable compared to last year.

Due to your tax valuation, your TSG decreased in relation to the rest.



A 30% discount to the variable component of the General Service Rate was applied to the 35% of properties with the lowest tax valuation.

SIMPLER FOR YOU

All your experience with the Municipality in one place

With your user you can:

- **Pay your municipal taxes**
- **Carry out online procedures and registrations**
- **Load urban reports from your neighborhood**

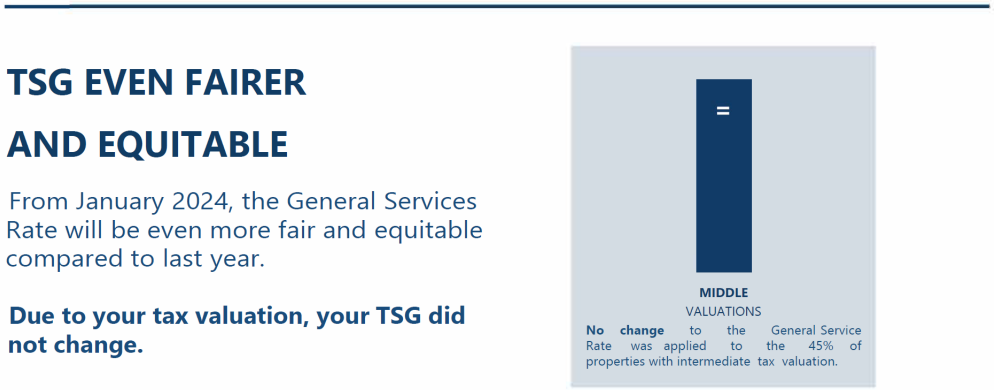
Scan the **QR** to link your **TSG** account.

Remember that you can view all your **invoices** and join the **automatic debit** through **mi3f.tresdefebrero.gov.ar**

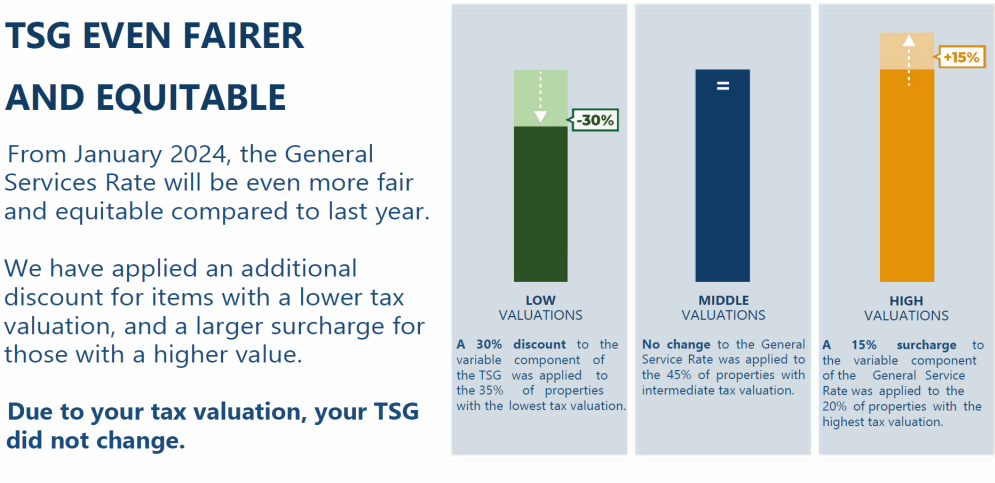


Figure I.3: Sample of Control and Treatment Messages: Middle Households, 2024 Experimental Design

(a) Control Letter Message



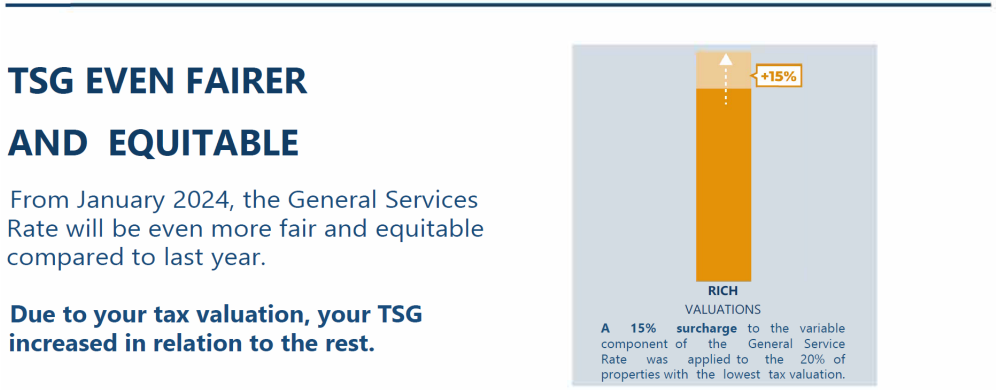
(b) Treatment Letter Message



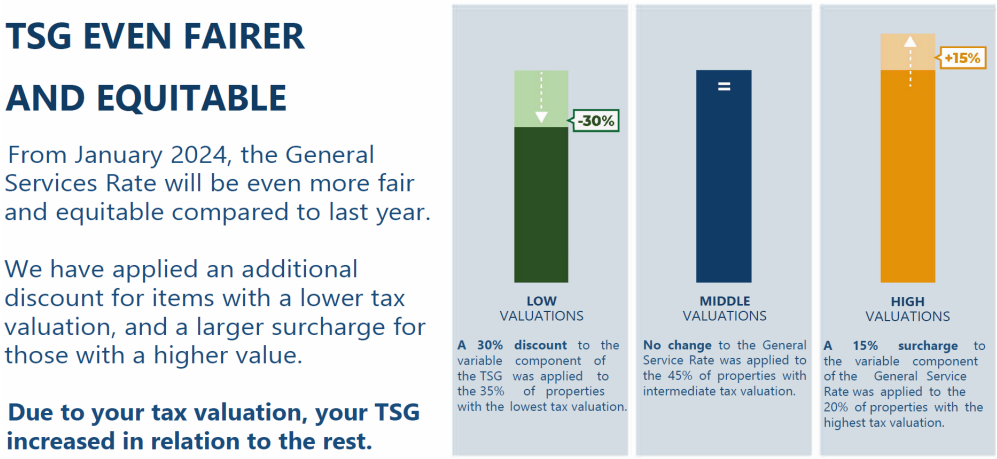
Note: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained on the second page of the mailer.

Figure I.4: Sample of Control and Treatment Messages: Rich Households, 2024 Experimental Design

(a) Control Letter Message



(b) Treatment Letter Message



Note: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained on the second page of the mailer.

J Survey Questionnaires

J.1 Email Invitation to Taxpayer Survey (English Translation)



Gmail

What do you think about rates and taxes in Tres de Febrero?
1 message

Tres de Febrero <ANONYMIZED> To:
ANONYMIZED

MUNICIPALIDAD

JOIN THE MUNICIPALITY WHATSAPP

Hello ANONYMIZED!

In Tres de Febrero, we are working to make taxes and rates fairer and more equitable. That's why we want to hear your opinion on the measures we have implemented in the municipality.

We only ask for three minutes to complete a short survey and share your thoughts.

CLICK HERE TO COMPLETE

Remember, you can pay your taxes, submit reports, or complete procedures online by registering on Mi3F.

Municipality of Tres de Febrero

LESS TAXES, MORE WORK

At Tres de Febrero we are on the side of those who produce and generate employment, that is why we take measures that benefit those who invest.

FREE ABILITATIONS
For any sector or size.

Shops | SMEs and industries | Commercial vehicles

NO OBSTRUCTIONS OR BUREAUCRACY
We eliminated more than 400 fees and simplified 30 thousand procedures per year.

WE HELP THOSE WHO GIVE WORK
Credits and tax benefits for those who hire Tres de Febrero workers.

FAIRER AND MORE EQUITABLE TSG

The TSG **increased below inflation** as in recent years and also now it is **fairer and more equitable**.

We applied a 30% discount on the variable component of the TSG for properties with the lowest tax valuation, and an average increase of 15% for those with the highest value.

Based on your tax valuation, your TSG increased in relation to the rest.

CLICK HERE AND SHARE THE INFORMATION

Unsubscribe Unsubscribe link

J.2 Complementary Taxpayer Survey (English Translation)



Welcome!

Thank you very much for participating in this brief survey, which will take less than 3 minutes and will help us make 3F the place we all want.

We want to know your opinion on the administration of municipal rates and taxes and about some measures applied during the last year.

Additional Information - Control Poor

FAIRER AND MORE EQUITABLE TSG

The TSG **increased below inflation** as in recent years and is also now **fairer and more equitable**.

Based on your tax valuation, your TSG decreased in relation to the rest.

Additional Information - Control Middle

TSG MÁS JUSTA Y EQUITATIVA

The TSG **increased below inflation** as in recent years and is also now **fairer and more equitable**.

Based on your tax valuation, your TSG accompanied the general increase.

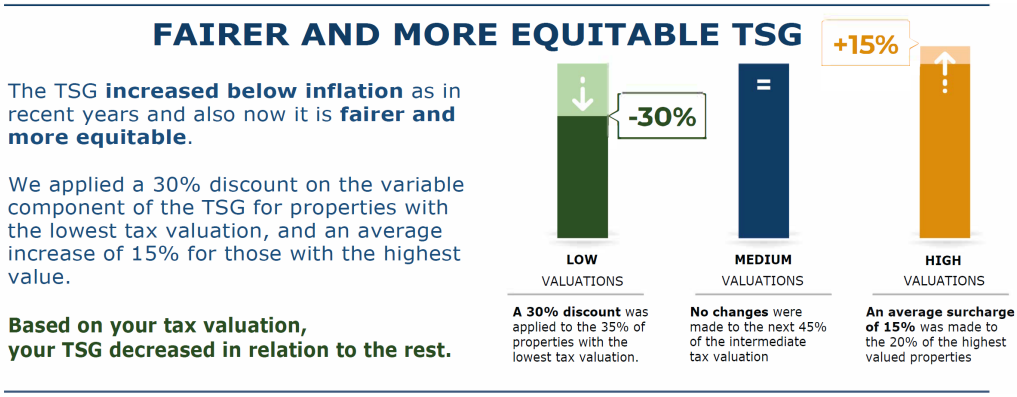
Additional Information- Control Rich

FAIRER AND MORE EQUITABLE TSG

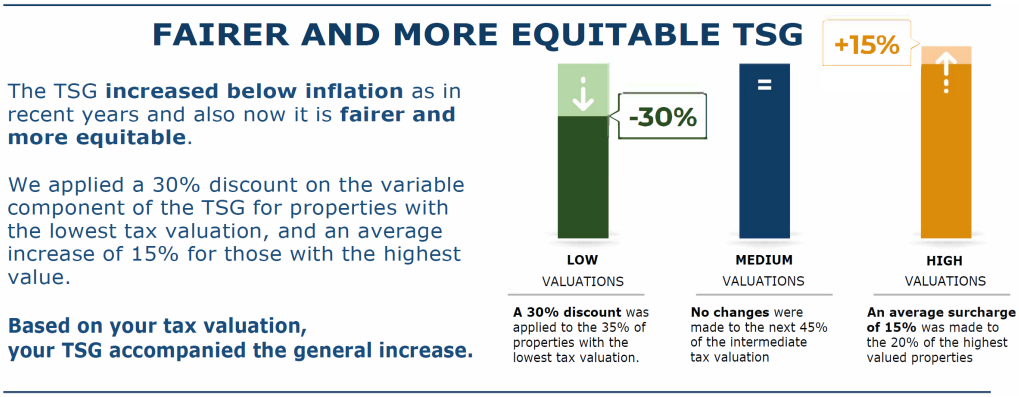
The TSG **increased below inflation** as in recent years and is also now **fairer and more equitable**.

Based on your tax valuation, your TSG increased in relation to the rest.

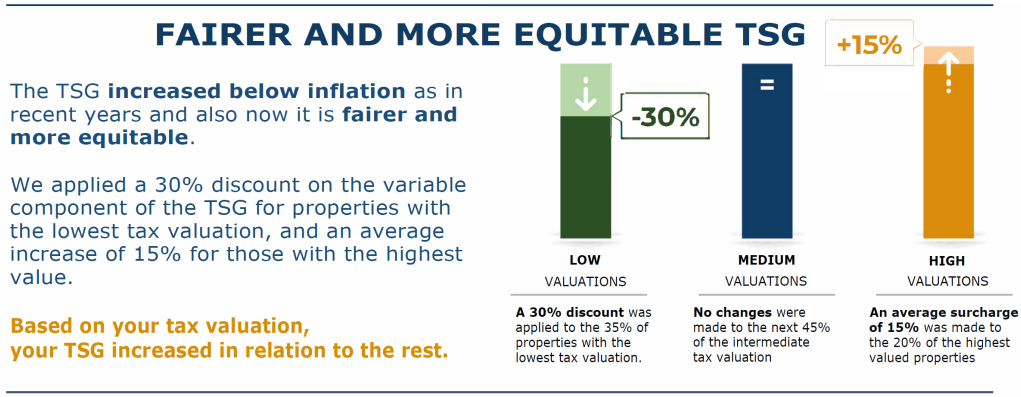
Additional Information - Treatment Poor



Additional Information - Treatment Middle



Additional Information - Treatment Rich





Do you live in Tres de Febrero?

- ◯ Yes
- ◯ No

Did you know about any of the following measures recently taken in Tres de Febrero?

- ☐ Free and express commercial permits
- ☐ Credits to businesses for hiring employees and making improvements
- ☐ Elimination of certain rates, taxes, and administrative procedures
- ☐ TSG: discounts for low-valuation properties and additional charges for high-valuation properties
- ☐ No, I haven't heard of any

Were you benefited by any of these policies?

- ☐ Yes, my household has benefited
- ☐ Yes, my business/industry has benefited
- ☐ No, I have not seen any benefit from these measures



Are you satisfied with the payment options available for municipal rates and taxes?

- ◯ Yes
- ◯ No, I would like other options

What other payment options would you like for your municipal taxes?

Some people think the government at its various levels (national, provincial, municipal) should not be concerned with the differences between the rich and the poor. Others think the government should do everything it can to reduce inequality. What do you think about this issue?

It should not be concerned about inequality										It should do everything to resolve inequality
0	1	2	3	4	5	6	7	8	9	10
◯	◯	◯	◯	◯	◯	◯	◯	◯	◯	◯



How fair do you think the current distribution of municipal taxes is between wealthier and poorer residents, that is, how much each group pays?

Very unfair — Very fair

0 1 2 3 4 5 6 7 8 9 10

Please tell us how much you agree or disagree with the following statements about the General Services Tax (TSG):

Lower the TSG for the poor (the 35% of households with the lowest valuations). There would be less money to provide services to the community.

Strongly disagree — Strongly agree

0 1 2 3 4 5 6 7 8 9 10



Raise the TSG for the rich (the 20% of households with the highest valuations). There would be more money to provide services to the community.

Strongly disagree — Strongly agree

0 1 2 3 4 5 6 7 8 9 10

Lower the TSG for the poor (the 35% of households with the lowest valuations) and at the same time raise the TSG for the rich (the 20% of households with the highest valuations). The money used to provide services to the community will remain constant.

Strongly disagree — Strongly agree

0 1 2 3 4 5 6 7 8 9 10

Are you generally satisfied with the services provided by the municipality?

Very dissatisfied — Very satisfied

0 1 2 3 4 5 6 7 8 9 10



What could the municipality improve regarding the collection of rates and taxes?

What is your overall opinion of the municipal administration and what aspects do you think should be improved?

J.3 Expert Forecast Survey



This survey involves no more than minimal risk to participants (i.e., the level of risk encountered in daily life). Participation typically takes between 5 and 10 minutes and is strictly confidential. Many individuals find participation in this survey enjoyable, and no adverse reactions have been reported thus far. Participation is voluntary, and participants may withdraw from the survey at any time.

I Agree

→

Introduction:

We conducted a field experiment about property taxes in a municipality from Buenos Aires, Argentina. Property taxes fund various services such as hospitals, parks, and roads. The taxpayers' residential properties are appraised each year, and the property taxes are computed as a function of the home's appraised value. For the sake of brevity, we assign each household to one of three groups:

- Low-income households: those with home values in the bottom 35% of the distribution.
- Middle-income households: those with home values in the middle 43% of the distribution.
- High-income households: those with home values in the top 22% of the distribution.

← →

The local government implemented a progressive tax reform in the property tax. Our study seeks to measure the effects of the reform on tax compliance. The tax rate is comprised of a fixed component and a component that is proportional to the assessed home value.

The tax reform affected households in the following way:

- Low-income households: the proportional component of their tax rate was reduced by 30%.
- Middle-income households: the proportional component of their tax rate did not change.
- High-income households: the proportional component of their tax rate increased by 15%.

← →

Subject Pool:

Households in the municipality receive a property tax bill by mail outlining their monthly obligations at the beginning of the year. In January 2023, we included a message in the paper bill mailed to the universe of about 100,000 residential households. We randomized some of the information contained in this message.

Outcome of Interest:

Property taxes are paid on a monthly basis and are due by the 15th of the corresponding billing period. Our main outcome of interest is whether the household paid at least one bill in the 3-month period after our intervention (i.e., between January and March 2023).

← →

Subject Pool:

Households in the municipality receive a property tax bill by mail outlining their monthly obligations at the beginning of the year. In January 2023, we included a message in the paper bill mailed to the universe of about 100,000 residential households. We randomized some of the information contained in this message.

Outcome of Interest:

Property taxes are paid on a monthly basis and are due by the 15th of the corresponding billing period. Our main outcome of interest is whether the household paid at least one bill in the 3-month period after our intervention (i.e., between January and March 2023).

← →

Experimental Design

We included a message printed on the flip side of the bill, in the area depicted by the yellow rectangle below:

Each household is randomized with equal chance to one of the following two messages:

(1) Control message: contains summary information about the change (or lack thereof) in the recipient's own tax rates (i.e., reduced, no change or increased).

(2) Treatment message: in addition to the basic information about the recipient's own taxes, the message included an infographic with detailed information about the progressive nature of the reform – i.e., on how it affected the taxes of the three separate groups (low, middle and high property valuations).

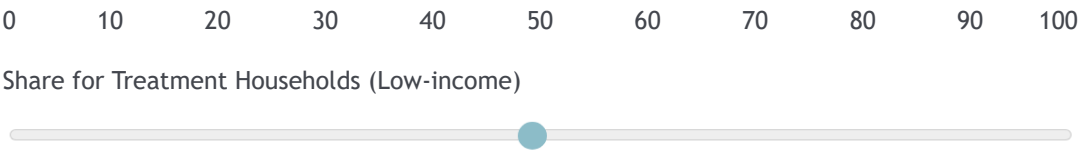


Prediction 1. Low-income group.

Low-income households were randomized to one of the two following messages:

(1) Control message: due to the tax reform, the recipient's own tax rate decreased.
(2) Treatment message: due to the tax reform, the recipient's own tax rates decreased (as well as the tax rates of all low-income households). Additionally, tax rates increased for high-income households and remained unchanged for middle-income households (information conveyed by means of an infographic).

In the 3-months after the intervention, 44% of the low-income households ***with the control message*** paid at least one bill. What do you think is the corresponding share for low-income households ***with the treatment message***?



How sure are you in this prediction?

- Not confident at all
- Slightly confident
- Somewhat confident
- Very confident
- Extremely confident

Could you please provide an explanation to justify your prediction? (optional)

Prediction 2. Middle-income group.

Middle-income households were randomized to one of the two following messages:

(1) Control message: due to the tax reform, the recipient's own tax rate did not change.

(2) Treatment message: due to the tax reform, the recipient's own tax rates did not change. Additionally, tax rates increased for high-income households, remained unchanged for middle-income households, and decreased for low-income households (information conveyed by means of an infographic).

In the 3-months after the intervention, 46.5% of the middle-income households ***with the control message*** paid at least one bill. What do you think is the corresponding share for middle-income households ***with the treatment message***?

0 10 20 30 40 50 60 70 80 90 100

Share for Treatment Households (Middle-income)



How sure are you in this prediction?

- Not confident at all
- Slightly confident
- Somewhat confident
- Very confident
- Extremely confident



Could you please provide an explanation to justify your prediction? (optional)



Prediction 3. High-income group.

High-income households were randomized to one of the two following messages:

(1) Control message: due to the tax reform, the recipient's own tax rate increased.

(2) Treatment message: due to the tax reform, the recipient's own tax rates increased (as well as the tax rates of all high-income households). Additionally, tax rates remained unchanged for middle-income households, and decreased for low-income households (information conveyed by means of an infographic).

In the 3-months after the intervention, 48.5% of the high-income households ***with the control message*** paid at least one bill. What do you think is the corresponding share for high-income households ***with the treatment message***?

0 10 20 30 40 50 60 70 80 90 100

Share for Treatment Households (High-income)

How sure are you in this prediction?

- Not confident at all
- Slightly confident
- Somewhat confident
- Very confident
- Extremely confident

Could you please provide an explanation to justify your prediction? (optional)

← →

This is the last section of the survey. We would appreciate if you could share some information about yourself.

← →

Are you currently one of the following: faculty, graduate student (either Master level or PhD level), post-doc, researcher in an international organization or non-academic researcher?

Yes

No

← →

The University of Nottingham

Which of the following describes your current position?

- Professor
- Assistant Professor
- Post-doc
- Researcher
- PhD Student
- Master Student

← →

The University of Nottingham

Please select your discipline

- Economics
- Business (management, accounting, finance, etc.)
- Political Science
- Psychology
- Sociology
- Other

← →

Have you ever conducted research on preferences for redistribution?

Yes

No

Have you ever conducted research on taxation?

Yes

No

← →

This is the end of the survey.

If you click next, you will submit your responses. We thank you for taking the time to provide your forecasts!

← Submit