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 perceived fairness of the tax system and their tax compliance. When rich households learn about the tax cuts for the poor, their perceived fairness also increases significantly, but their compliance, if anything, goes down. Leveraging a further reform (and another 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 its perception of 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, real-world evidence on the factors behind the support for tax progressivity is scarce. This study addresses this gap by analyzing how taxpayers respond in practice to a real-world 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, holding constant its own tax rate, a taxpayer's compliance may also depend on what the household perceives 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 2.7% in 2022.[2] The municipality uses tax revenues to fund basic services such as street lighting and urban sanitation. The municipality implemented a progressive tax reform in January 2023. Properties at the bottom 37% of the value distribution received a tax cut. Hereinafter, we refer to these as house-poor households, or *poor households* for short.[3] Tax rates for properties in the middle part of the distribution (*middle-valuation households*) remained unchanged. Properties in the top 27% (*rich households*) faced a significant tax increase.

The setting in which this reform occurred has two advantages in addressing the research question at hand. First, there is substantial wealth inequality, and thus there is ample scope for redistribution through taxation. 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). This inequality is also reflected in home values. For example, in Tres de Febrero, a property in the 90th percentile of assessed value is worth 6.8 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 the *own-rate* effects of the reform 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]This seemingly high nominal rate corresponds to a chronic and homogeneous under-valuation of properties in the cadaster with respect to market values. See the next section for more details.

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

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 0.256%, implying an elasticity of ε^{own}_{poor}=0.256. Conversely, a 1% increase in the tax rate for the rich reduces their compliance by 0.488%, corresponding to an elasticity of ε^{own}_{rich}=0.488.[4] While we do not have data to disentangle the two, our preferred interpretation is that the own-rate effects 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" 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 makes them less affordable, and thus they become less likely to pay. However, the effects of the own rate could also be due, at least in part, to tax morale. 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 ones 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 nearly the universe of taxpayers (around 110,000 letters). 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 either a (control) letter detailing the tax cut for poor households or a (treatment) letter that also highlighted the broader reform, that is, the tax hike for rich households. Both groups received information about the amounts deducted from or added to their bills. This setup ensured that information about changes

[4]Since their tax rate was not affected by the reform, we cannot estimate an *own-rate* effect for middle-valuation households.

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 15% response rate with 1,909 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 the treatment actually had a significant effect on awareness of the progressive reform. Finally, 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 first discuss the cross-rate effects for poor households. Using survey data, we show that the treatment had a significant effect on awareness of the reform. Only a minority of the control group is aware of it, and awareness is about 50% higher households in the treatment group. Using administrative records, we also show that the treatment had a positive and significant effect on tax compliance. That is, poor households' tax compliance increases when they learn about the progressive nature of the reform. Compared to poor households who received the control letter, those treated with information about the progressivity of the reform were 0.808 percentage points (p-value = 0.006) more likely to pay their due taxes during the following three months. 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 to 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%. While 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.[5] 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.

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

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 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-valuation 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 perceived 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 conducted 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 treatment effects. 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 close to our experimental estimates.[6]

Our findings have potential implications for the design of progressive tax reforms. In a nutshell, the government must factor in the tax compliance responses to forecast the true effects of a tax policy (Saez and Zucman, 2023). 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 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 percentage points. 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

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

the four behavioral elasticities estimated with the quasi-experimental and experimental approaches. Relative to the counterfactual with no 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.

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, contrasting sharply 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 to income inequality (Piketty and Saez, 2007; Bachas et al., 2024), while theoretical work has examined optimal progressivity levels balancing 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 preferences for redistribution. 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 relying 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.[7] 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 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 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 suggestive 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

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

et al. (2022) shows that tax compliance increases when taxpayers believe they benefit from government spending.[8] 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. While our sample includes some commercial properties, the vast majority (84%) are residential, so we refer to taxpayers as "households" for brevity.

The tax revenues are used for the provision of 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.[9] For our analysis, we have access to monthly administrative data from the municipality's tax records.[10]

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 $29,406 per year in current Argentine pesos, equivalent to 478 U.S. dollars.[11] Throughout the remainder of the document, unless explicitly stated otherwise, all monetary amounts are expressed in current Argentine pesos. On average, households paid an annual tax rate of 2.69% of the property's assessed value in 2022. The provincial tax authority's cadastre determines property assessments, but these values systematically under-estimate 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

[8] 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).

[9] 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).

[10] We exclude from the analysis 8,546 properties without property value assessments and 148 households with incomplete tax compliance records.

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

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).[12]

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 flat fee, earmarked for specific services such as security and health.[13] For reference, for a household with a median-value home, about 61% of the total tax burden comes from the variable component, while the remaining 39% is attributed to the flat fee. While the variable part is progressive, the flat charge makes the overall tax schedule somewhat regressive. For instance, a property in the 25th percentile of the assessed value distribution faced an effective tax rate of 2.67%, compared to 1.78% 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 developed-country municipality, only 0.42% of households had delinquent property taxes; in contrast, studies from Argentina, Brazil, Haiti, and Malawi report that over 50% of households failed to 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 instance, in the United States, while the 40 largest U.S. cities collect nearly 95% of property taxes on time (Chirico et al., 2016), compliance of income taxes can be substantially lower: the IRS estimates that individuals underreport 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.[14] Our primary outcome of interest is a binary variable that indicates whether a household made a tax payment within three months of the due

[12] Using propietary 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.

[13] 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.

[14] 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. While it is technically possible to request the removal of this charge from the electricity bill, very few households do so. In 2023, 98% of households paid at least part of their property tax through this mechanism, highlighting its high compliance rate.

date. This definition of compliance aligns with related studies (Del Carpio, 2014; Castro and Scartascini, 2015; Carrillo et al., 2021; Cruces et al., 2024; Flores et al., 2025), though 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 across 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 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. The government implemented this reform swiftly, limiting advertising beyond the experiment. For instance, the city hall 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.[15]

The reform established four taxpayer groups based on sharp, binding thresholds of assessed property values: properties valued at or below \$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 \$750K and \$1.5M (the middle 36% of the distribution) were not affected by the reform. Properties valued above \$1.5M (the top 27% of the distribution) experienced a tax hike. More precisely, properties assessed between \$1.5M and \$3M faced a 10% surcharge in the variable component, while properties assessed above \$3M experienced a 20% increase in the variable component. We should note that when estimating own-rate effects, given the small share of properties above the \$3M mark (around 6%), we lack sufficient statistical power to analyze the discontinuity at that threshold, so we focus on the \$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.[16] 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

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

[16] 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.

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 $750K received a reduction in the fixed component of approximately 16%. Properties assessed between $1.5M and $3M faced a surcharge in the fixed component of approximately 39%, while properties valued above $3M experienced a surcharge of approximately 64%. And, like in the 2023 reform, properties valued between $750K and $1.5M (the middle 36% of the distribution) were not affected by the 2024 reform.

3 Own-Rate Effects

3.1 Research Design

We identify the *own-rate* effects using two key identification strategies. First, we take advantage of the sharp discontinuities in tax rates induced by the reform. Second, we exploit the timing of the reform by comparing the outcomes before and after its implementation. Since it leverages threshold discontinuity as well as temporal changes around the implementation date, our research design resembles a difference-in-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. 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.[17,18] We estimate the RDD parameters using non-parametric 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 the 2024 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 that uses a fake

[17] For instance, if an individual 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.

[18] 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 value was set to zero. This correction is made to avoid pre-treatment outcomes to be contaminated by post-treatment choices. In any case, this is a minor detail, as the results remain almost identical without this correction.

implementation date.

3.2 Main Results

Figure 1 summarizes our main *own-rate* results. Panels (a) and (b) present the first-stage results, that is, the effects of the reform on tax rates of poor and rich households, respectively. In each panel, the running variable is the assessed value of each property. As expected, both panels depict visible, substantial and statistically significant effects on the tax rates. The progressive reform reduced poor households' tax rates by approximately 0.39 percentage points compared to middle-valuation households near the threshold. Conversely, rich households' tax rates at the threshold increased by about 0.15 percentage points relative to middle-valuation households near the threshold.

Panels (c) and (d) of Figure 1 present the reduced-form results, that is, the effects of the reform on compliance rates. Panels (c) and (d) are equivalent to panels (a) and (b), but using a different outcome variable. We find significant and meaningful effects on tax compliance for both poor and rich households. Poor households responded to a tax rate reduction by increasing their compliance, whereas rich households, facing a tax rate increase, reduced their compliance. The impact on poor households is large, with a 2.72 percentage point increase in the proportion of bills paid from one semester to the next. The corresponding effect for rich households is also large, as they experienced a 2.25 percentage point reduction in compliance.

Our RDD results are robust to a series of alternative estimation choices and tests. Figure 2 replicates the analysis using a hypothetical, non-existent reform in mid-2023 (a placebo date robustness test). Figure 3 performs an additional test by using placebo discontinuities in the running variable ($500K and $2M instead of $750K and $1.5M) for both the first stage and reduced form. Reassuringly, the results in both figures confirm the robustness of our main estimates to these placebos. Additionally, we conducted manipulation tests around the cutoffs and found no evidence of relevant discontinuities in the density of the running variable (see Figure B.1).

Finally, we conducted several robustness checks to validate our preferred specification. We observed consistent patterns when including additional pre-reform controls, using an alternative bandwidth, or redefining the outcome variable (for instance, including the possibility of paying 6 months after the due date instead of only 3).[19]

[19] See Table B.1 for more details.

3.3 Own-Rate Elasticities

To assess the magnitude of the *own* effects, we express the results in terms of behavioral elasticities. Let β^{own} represents the reduced form effect from the RDD, α denotes the first stage effect, C^{own} represents the compliance of the middle group (just above or below the cutoff), and τ represents the tax rate of the middle group (just above or below the cutoff). Both elasticities are expressed in terms of six-month to six-month changes, 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 2023 levels for the middle group, focusing on middle-value properties situated near the thresholds – above for poor households and below for rich households. The results are presented below, with 90% confidence intervals in brackets:[20]

$$\varepsilon^{own}_{poor} = \frac{\frac{\beta^{own}_{poor}}{C^{own}_{poor}}}{\frac{\alpha_{poor}}{\tau_{poor}}} = \frac{\frac{-2.718}{60.1}}{\frac{0.392}{2.221}} = \underset{[-0.386;-0.099]}{\text{-0.256}} \tag{1}$$

$$\varepsilon^{own}_{rich} = \frac{\frac{\beta^{own}_{rich}}{C^{own}_{rich}}}{\frac{\alpha_{rich}}{\tau_{rich}}} = \frac{\frac{-2.248}{58.3}}{\frac{0.146}{1.837}} = \underset{[-0.901;-0.022]}{\text{-0.488}} \tag{2}$$

We estimate a significant *own-rate* elasticity for poor households of -0.256, indicating that for every 1% decrease in their tax rate, compliance increases by 0.256%. Similarly, the *own-rate* elasticity for rich households is -0.488, implying that for every 1% increase in their tax rate, compliance decreases by 0.488%. The point estimate suggests that the effects are stronger for the rich (-0.488) than for the poor (-0.256), suggesting that individuals 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 = 0.403).[21]

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. While we have so far concentrated on the direct effects of each group's own rate changes on their compliance, in the following section we establish whether there is an indirect effect of changes in other groups' tax rates on the own group compliance.

[20] We calculated confidence intervals using 2,500 bootstrap iterations.

[21] To test whether the difference in elasticities between the two groups is statistically significant, we conducted 2,500 bootstrap iterations to calculate the p-value of the difference.

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 *cross-rate* effects from the 2024 reform are presented in Appendix E.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, while rich households respond to the tax increase by reducing their compliance. The *own-rate* elasticities are nearly identical between the two years: for poor households the estimates are -0.256 for 2023 and -0.288 for 2024, while for rich households the estimates are -0.488 for 2023 and -0.471 for 2024.

4 Cross-Rate Effects

4.1 Experimental Design

In this section, we seek to estimate how the changes to the tax rates of others (in the context of a progressive tax reform) affect one's own perceptions of fairness and tax compliance. With this goal, we conducted a natural field experiment.[22]

In January 2023, the municipality sent a personalized letter to around 92,000 taxpayers.[23] These letters included personalized information to each account holder, such as the property's tax valuation, the payment structure, a monthly breakdown of the tax amount due, their expiration dates, and the payment options. This level of detail ensured that recipients had a clear understanding of their financial obligations. Section H.1 shows an example of a letter sent to a household.

All participating households received their usual tax bill, but the information provision experiment was embedded in the second page of the letter. Importantly, the control letters specified whether the recipient's *own* tax rate was reduced, increased, or not impacted by the reform. The control letters mentioned the implementation of a tax reform in broad terms, emphasizing its aim to enhance the tax system's equity but without specifying how the reform affected the tax rates of other groups.

[22] Balance Tables can be found in Appendix C, where we show that all our treatment arms are balanced.
[23] This was the only bill of 2023 delivered in paper format. The municipality sent additional communications during that year, but only digitally.

The key aspect of our research design is that it experimentally varied the awareness about the tax reform's progressivity by providing a detailed description of the reform to a randomly selected subset of taxpayers. The treatment letters contained exactly the same baseline information as in control group letters, but they also included a key element designed to convey the progressive nature of the reform's–informing not only how it affected the recipient's tax rate, but also how it affected that of other groups. More precisely, the treatment letters featured an infographic that highlighted the reform's progressive nature along with an accompanying text. Policy communication professionals collaborated to create an engaging and informative visual, depicting how the reform affected tax rates across property valuation brackets (poor, middle, and rich groups) and the recipient's own rate. This design ensured clarity for a non-specialized audience.

We provide a series of illustrations of our treatment and control messages. Panel (a) of Figure 4 presents an example of the message received by poor households in the control group (similar infographics were included in the letters for rich and middle taxpayers).[24] This message was carefully calibrated to acknowledge the reform's intent to change the tax burden for the taxpayer without emphasizing its progressive character nor the changes in other groups. The treatment infographic was more explicit and detailed in depicting the progressivity of the reform (panel (b) of Figure 4 presents the message included in the letter for the poor group). It clearly outlined the tax implications for different property valuation groups, indicating which brackets would experience an increase, a fall, or no change in their tax rates, and the magnitude of these changes in the rates. To ensure that individuals in both the treatment and control groups would be aware of the amount that was deducted from or added to their bills, the front page of the letter included a line that explicitly contained this information in pesos, as can be seen in the example letter of Appendix H.1.

Since it was an official government communication (as opposed to a letter from researchers), the municipality had full discretion over how to communicate the reform. The municipality wanted to prioritize that the message would be easy to understand, while being 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 message does not fully capture the complexity of the reform in two ways. First, the figures communicated by the municipality correspond to the changes to the variable component of the tax. Due to the existence of a fixed component, when considering the effects on total tax liabilities, the actual discount and surcharges were somewhat smaller than the ones described in the message.[25] Second, the 15% surcharge for rich households mentioned

[24]The control and treatment messages for middle and high property valuation groups are presented in Figures H.1 and H.2.

[25]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

in the letter was calculated as an unweighted average between properties valued between \$1.5M and \$3M (facing a 10% surcharge) and properties valued above \$3M (facing a 20% surcharge).

Since the reform was not widely discussed nor advertised, and it was approved and implemented in a relatively short period of time, most taxpayers were probably not aware of it or 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 the progressive reform. We expect our information treatment (through this visual representation) to have induced not only increased awareness about the reform but also genuine learning about it among a relatively disinformed public. At the very least, our treatment should have boosted the reform's salience among those who already knew about it, although this group seems to be only a minority of taxpayers. In any case, because of increased awareness, learning, or salience, we expected that this detailed explanation of the reform's progressive nature would play a significant role in shaping households' perceptions and attitudes towards the reform, and potentially induce changing patterns in tax compliance and perceptions of tax fairness with respect to the control group.

Because subjects were impacted differently by the reform, we stratified our experiment across the three property valuation groups (the poor, middle, and rich groups, as defined above). In addition, a small group of taxpayers (5,913) was randomly selected to receive no letters. As shown in prior research (Cruces et al., 2024), 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.

4.2 Main Results

We first document the effect of our intervention on the awareness of citizens of the reform – we asked respondents in our survey if they knew about the reform or not.[26] Only about 15.2% of 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, as depicted in panel (a) of Figure 5. This aligns with our prior expectation that knowledge about the reform was low among taxpayers given the relatively low level of discussion about it in the public sphere. Panel (b) shows a consistent and significant treatment effect across all valuation brackets, with blue indicating the effect on poor households, green on medium households, and red on rich households, while the gray

surcharge of approximately 9.9% – see Appendix A.1 for more details.

[26] Because survey respondents received additional information and questions related to the reform, we exclude them from the main experimental sample for tax compliance outcomes. Including this group yields qualitatively similar results.

color represents the average effect. Our information treatment significantly increased awareness of the reform in the three groups by about 7.2 percentage points 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 paperwork simplification for new businesses. We also asked respondents about their awareness of these policies in the survey. Unlike the information about the progressive nature of the reform, which was only present in treatment letters, both types of letter included information about these two policies.[27] We used the question about awareness of business loans as a falsification test of our information treatment (the results are very similar to awareness about the paperwork simplification policy). Panel (c) of Figure 5 indicates a relatively low and evenly distributed level of knowledge about this policy among taxpayers, with only 13.1% stating that they knew about it. 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 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 perceptions can be directly attributed to our targeted intervention, rather than to a general increase in policy awareness or other spurious effects.

The results from the survey allow us to study the impact of our intervention on our first outcome of interest, taxpayers' fairness perceptions about the reform. We asked respondents whether they thought the local tax system was fair– more specifically, we asked how fair they considered the distribution of the municipal tax rates between rich and poor households, on a scale from 0 to 10 (in the context of a survey about the tax). Panel (a) of Figure 6 shows relatively high perceived levels of fairness in the control group, with an average perception of about 5.5 and relatively evenly distributed between the three groups. Interestingly, our information provision treatment increased this perception across the board, as depicted in panel (b) of Figure 6. There is a positive and significant treatment effect on fairness perceptions across all valuation brackets of about 0.54 points on the 0-10 scale, with fairly similar effects for the poor, middle, and rich groups.[28] However, priming or social desirability bias may explain these effects, with respondents receiving information about a progressive reform simply offering more positive responses. We added a

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

[28] 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)

further question in the survey as a falsification test. Specifically, we included the General Social Survey on agreement with the role of government in reducing the gap between the rich and the poor, which allegedly represents deeply rooted general preferences and not something about a specific policy. Reassuringly, the results in Panel (d) of Figure 6 show that our information treatment did not influence respondents' redistribution preferences. This null effect on preferences supports our interpretation that our information treatment truly increased the perception of fairness of the tax system and does not represent some spurious result.

We now turn to the other core finding of our experimental setup: the *cross-rate* effects of the reform on actual taxpayer behavior, as measured with administrative data. Panel (a) of Figure 7 presents an event-study analysis at the most granular level, the monthly level. That is, the outcome of interest is the the probability of paying a tax bill no later than three months after the due date (see Section 2.2 for details). We estimate the difference in this outcome between treatment and control taxpayers, and repeat this separately for the poor, middle and rich groups.[29] The x-axis represents months before and after the intervention, while the y-axis measures treatment effects, in percentage points (pp). The pre-reform levels and trends reflect the random assignment of the treatment within each of the three groups: compliance is balanced between treatment and controls, with small and mostly non-significant differences between the two for any of the valuation brackets. Interestingly, there is a clear positive and significant effect of the treatment on compliance for the poor group, as witnessed by the six monthly coefficients. For the middle group, the differences are smaller and not statistically significant at conventional levels. Finally, we also document a slightly negative effect on rich households in some months.[30] To provide a 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 'After Letter' coefficients of these pooled results, which remain consistent with our previous observations, and the 'Before Letter' coefficients, which show no difference between groups. The blue coefficients represent 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). By contrast, the green coefficient represents the effect for middle-value individuals, revealing no statistically significant change in payment behavior. Rich individuals, displayed by a red coefficient, show a negative pooled estimate of -0.313, though this is not significant at

[29] 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 value was set to zero. Results remain almost identical without this correction.

[30] The patterns seen in Figure 7 align with existing literature on correspondence studies and suggest that salience may play a role in the observed outcomes. As the message recedes from taxpayers' immediate memory, its impact appears to wane, a phenomenon consistent with the patterns discussed by Bergolo et al. (2023) in an information provision tax compliance experiment.

conventional levels. The raw differences before and after the intervention between treatment and controls for the three groups confirm the pattern in Panel (a) of Figure 7 (see Figure D.1 for details). Pre-intervention, treated and control taxpayers reported similar compliance levels. Post-intervention, poor households in the treatment group displayed a noticeable increase in their probability of making a tax payment, while rich households showed a slight decline compared to their control counterparts.

We conducted several robustness checks. For example, we find similar patterns when we exclude from the regression control variables not related to pre-treatment payments. We also find similar results when using an alternative sample of subjects (adding the survey sample), when using an alternative definition of the outcome (allowing payments up to 6 months after the due date instead of only 3), or when excluding outliers in property valuations (below the 5th and above the 95th valuation percentiles). Table B.2 provides more details about these tests.

These results are more notable when considering that the actual treatment was the inclusion of an infographic and some text at the back of a local tax bill. A potential concern could arise if the letter open rate (or reading rate) was too low, which would bias our results downwards. If taxpayers had not opened the letters at all, however, they would not have been exposed to our information treatment, and our results could be spurious. To address this issue, we included an additional simple experiment comparing taxpayers who received a control letter and those who received no letter at all (we assigned a small group of taxpayers to this no-letter condition). Payment patterns remained similar between groups that received a letter and those that did not before the intervention. However, after the letters were delivered, taxpayers in the control group who received a letter consistently and substantially increased their payments when compared to those who received no letter. This pattern appeared similar across poor, middle, and rich households (see Figures D.2 and D.3). The average difference of about 5 percentage points matches a recent systematic review of simple experiments of this type (Antinyan and Asatryan, 2024).[31] Overall, the results from this auxiliary experiment suggest that probably a significant share of taxpayers opened the letters we sent.

However, opening the letters does not necessarily mean reading them carefully. Our treatment was intentionally subtle, with the message appearing only on the back page of the tax bill letter. The effect of sending the information provision letters (0.808 pp for the poor group, for instance) should be interpreted as an intention-to-treat (ITT) coefficient. We can view the Average Treatment Effect on the Treated (ATET) as a factor of the ATE,

[31] It should be noted that experiments of this type find large effects because they compare the receipt of a letter to no letter at all, which includes a large reminder effect over and above the content of the actual letter. Our information provision experiment's results are considerably smaller because they rely on the comparison of subtle variations between two types of letters, which nets out the basic reminder effect.

influenced by the non-compliance rate. For a taxpayer to be actually treated, the letters needed to arrive on time, be opened, read front and back, and the recipient must process and understand the message. The recent literature on similar experiments estimates these effects for the United States, and finds non-compliance adjustment factors between 6.5 and 7.5 (Perez-Truglia and Cruces, 2017; Bottan and Perez-Truglia, 2020; Gerber et al., 2020; Nathan et al., 2020). In our context, we can use the results from Figure 5 (Panels (a) and (b)) to estimate the treatment effect on the probability of knowing the reform's progressive nature as a proxy for compliance with the treatment. If 18.8% 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 81.2 pp due to our treatment (from 18.8% to 100%). However, we identify an effect of approximately 9.5 pp (Panel (b)), implying a reading rate of 11.7% ($\frac{9.5}{81.2}$), or a non-compliance adjustment factor of around 8.5. This adjustment factor suggests that the treatment ATET on the poor was 7 pp, which we consider an upper bound and interpret cautiously. 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. On the other hand, Perez-Truglia and Cruces (2017) noted that, according to the Environmental Protection Agency, nearly 50% of unsolicited mail is discarded unopened, determining a non-compliance adjustment factor of 2, which we will use as our conservative lower bound. Overall, we estimate the effect ranging between 2 pp and 8 pp, depending on the non-compliance adjustment factor used.[32]

The information provision results on tax compliance, which represent actual behavior, contrast with those on perceptions documented in panel (b) of Figure 6. First, as panel (a) of the same figure shows using data from the control group, citizens across valuation brackets rate the TSG as somewhat fair (measured from 0 to 10). Second, as panel (b) shows, using survey self-reported data on perceived tax fairness, our analysis reveals a significant and large positive treatment effect on fairness perception (of 0.54 points, on a scale from 0 to 10, with a baseline around 5.5). This effect is fairly similar across valuation brackets, which suggests that poor and middle but also rich households increased their perception of fairness of the tax system when exposed to our treatment. In addition, regardless of their property value, most of the taxpayers surveyed expressed satisfaction

[32]Non-compliance is not exclusive of the *cross-rate* effects. The *own-rate* effects (Section 3) could also be plausibly affected by attenuation bias, because of non-compliance. This is because not every taxpayer was necessarily aware of how her own tax changed. However, in the case of *own-rate* estimates, estimating the ATET is considerably more challenging and implies more assumptions. In particular, own-rate effects are potentially explained by a mix of a "price" effect (which are not affected by attenuation bias) and tax morale (which are), and thus estimating the scaling-up factor requires an assumption about the relative weight of each of the two effects. Therefore, we prefer not to report scaled-up (ATET) results for the own-rate effects.

with a reform that lowers the tax for the poor and raises it for the rich (see Figure D.4).

Based on these stated perceptions, we would expect a similar behavioral effect in terms of actual tax payments across the three valuation brackets. However, the treatment effect on actual tax-paying behavior appears to be mediated by how the reform affects individuals' own tax rates: poor households respond by further increasing their tax compliance (over and above the positive *own-rate* effect), while rich households show no significant behavioral response in this dimension (if anything, they slightly reduce their compliance). However, this reduction is difficult to detect with our sample size, as the sample of rich households is less than half as large as the sample of poor households. Panel (b) of Figure 7 illustrates these results.

4.3 Cross-Rate Elasticities

We assess the magnitude of the *cross-rate* effects by expressing them in terms of behavioral elasticities. β^{cross} represents the effect obtained through the experimental design for each property valuation group, C^{cross} denotes the compliance of each group in 2022, 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 the poor group and a 15% increase for the rich group, matching the information conveyed (regarding the variable component of the tax) in the letters sent.[33] 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 2023 compliance rates of the control group for each valuation bracket. The results are presented below, with 90% confidence intervals in brackets:[34]

$$\varepsilon^{cross}_{poor} = \frac{\frac{\beta^{cross}_{poor}}{C^{cross}_{poor}}}{\frac{\Delta\tau_{rich}}{\tau_{rich}}} = \frac{\frac{0.808}{55.2}}{0.15} = \underset{[0.038;0.156]}{0.098} \tag{3}$$

$$\varepsilon^{cross}_{rich} = \frac{\frac{\beta^{cross}_{rich}}{C^{cross}_{rich}}}{\frac{\Delta\tau_{poor}}{\tau_{poor}}} = \frac{\frac{-0.313}{54.8}}{\text{-0.3}} = \underset{[-0.019;0.057]}{0.019} \tag{4}$$

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. We also estimate a *cross-rate* elasticity for rich households of 0.019, although this

[33] Our cross-rate elasticities are computed based on changes in the variable tax rate rather than total tax liability. Implicitly, we assume that taxpayers interpreted the messages as referring to changes in the total tax rate, given that the letters did not explicitly state otherwise.

[34] We calculated confidence intervals using 2,500 bootstrap iterations.

is not statistically significant at conventional levels. These results are not scaled up for non-compliance and thus should be interpreted as ITT elasticities. Furthermore, using 2,500 bootstrap iterations, we show the difference in elasticities between poor and rich households is statistically significant at the 10% significance level (p-value = 0.059).

To put the magnitude of the cross-rate effect 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% ($\varepsilon^{own}_{poor} \cdot 0.38\% = 0.098\%$).

4.4 Replication: The 2024 Reform

Taking advantage of the new progressive reform conducted in 2024, we conducted a second large-scale field experiment to assess whether the results from the 2023 experiment replicate. On top of the changes to the variable components from the 2023 reform, the 2024 reform made the fixed component more progressive. Despite this difference in the details of the components of the tax affected by the reform, the Municipality preferred to convey treatment messages similar to those of 2023, focusing the attention on the fact 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.[35]

Besides 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 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 the treatment and control messages. Contraty 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 all 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), whereas 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 H.2.

While the reform and letters were similar in 2023 and 2024, the broader environment

[35]For details on the 2024 experimental design see Appendix E.1.

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. Additionally, the similarity between the reforms may have led some individuals in 2024 to misinterpret our communications as referring to the previous years reform, potentially weakening their response.

We replicate our *cross-rate* analysis of the 2023 reform using data from the 2024 experiment. The full set of results is presented in Appendices E.3 and E.4 and 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 subjects opened and paid attention to the letters. Most importantly, the difference in compliance between the treatment and control letters in 2024 mirrors the results from the 2023 experiment. 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 effect but statistically insignificant effect on rich households, and an insignificant effect on middle households. The consistency of the findings between the two experiments, despite the differences in economic and political environment, underscores the robustness of our results.

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 for properties in the poor group by some proportion $\%\Delta\tau_{poor}$ and increases the tax rate on properties in the rich group 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 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.[36]

In this hypothetical reform, we assume that all households within each property value bracket are affected equally, allowing us to focus on average effects within each group.[37] 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 *counter-factual* 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 for poor households before the reform. Let $C_j^s \in (0, 1)$ denote the average tax compliance. For instance, C_{poor}^0 is the average probability that a poor household pays its taxes on 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.[38]

The progressive reform affects taxes paid via 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 increase 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., holding constant the new tax liabilities (L_j^s), the amount of taxes paid (T_j^s) varies because of changes in tax compliance (C_j^s). The compliance effects can be summarized as a function of the four behavioral elasticities estimated above (ε_{poor}^{own}, ε_{rich}^{own}, $\varepsilon_{poor}^{cross}$ and $\varepsilon_{rich}^{cross}$):

$$C_{poor}^A = C_{poor}^0 \cdot \left(1 + \varepsilon_{poor}^{own} \cdot \%\Delta\tau_{poor} + \varepsilon_{poor}^{cross} \cdot \%\Delta\tau_{rich}\right) \tag{5}$$

$$C_{rich}^A = C_{rich}^0 \cdot \left(1 + \varepsilon_{rich}^{own} \cdot \%\Delta\tau_{rich} + \varepsilon_{rich}^{cross} \cdot \%\Delta\tau_{poor}\right) \tag{6}$$

Following the conservative estimates discussed in Section 4 above, we adjust the raw

[36] 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, in contrast, 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.

[37] 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.

[38] 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 where taxpayers either pay the full amount or nothing at all.

cross-rate elasticities by a factor of two, in order to scale up the intention-to-treat effects into treatment effects on the treated. With these compliance 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_j^A 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_j^A following equations (5) and (6). We then use the distribution of outcomes across bootstrap samples to compute 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 no behavioral responses from households in the middle property valuation group.[39] 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.[40]

5.2 Results

The key results are summarized in Figure 8.[41] Panel (a) shows the results corresponding to the first outcome of interest: the effective tax progressivity. 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 households), 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 taxpayers in the rich group faced on average an effective tax rate that was 0.31 pp *lower* than that of taxpayers in the poor group–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

[39] 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 the middle group were small and not statistically significant, and thus we assume a nil behavioral response through this channel.

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

[41] For more detailed results, see Appendix F.2.

behavioral responses the reform would have made taxes significantly more progressive, with taxpayers in the rich group paying an effective tax rate 0.5 pp *higher* than that of the poor group. 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 from taxpayers in the poor group only.[42] Since taxpayers in this group households 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 from those in the poor group *reduce* the effective tax progressivity by 0.113 pp when compared to the situation without this group's behavioral responses. 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 those in the rich group only. Since taxpayers in this group actually reduce their compliance, 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=0.042). 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<0.001). In other words, due to behavioral responses, 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 $12,843. The second bar, corresponding to post-reform without behavioral responses ($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 taxpayers in the poor group only. Since poor households increase compliance, this channel generates $201 in additional per capita revenue, an effect that is highly statistically significant (p-value < 0.001). Conversely, the fourth bar incorporates the behavioral responses from taxpayers in the rich group only. Since rich taxpayers lower compliance, their responses reduce tax revenue by $763 (p-value $= 0.042$). The fifth and final bar shows the results when allowing for behavioral responses from taxpayers in both groups. Since the two behavioral responses go in opposite directions, the net impact

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

hinges on which of these two offsetting effects predominates. 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 = 0.134).

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 F.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 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 perceptions of distributional equity within the tax system. In contrast, while middle- and high-valuation households also report improved perceptions of fairness under the progressive scheme, their compliance rates show, if anything, a negative response.

Our findings have important 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 these findings to other contexts, one must consider institutional and cultural differences. In low-enforcement settings, tax reforms have 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), leav-

ing 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 noncompliance 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. Additionally, cultural or political factors may shape these behavioral responses. A progressive reform could have stronger or weaker effects depending on a societys prevailing ideological stance or 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 fees, 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 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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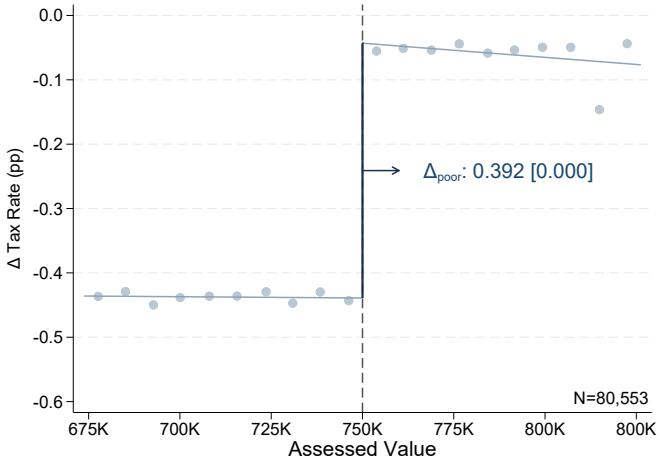
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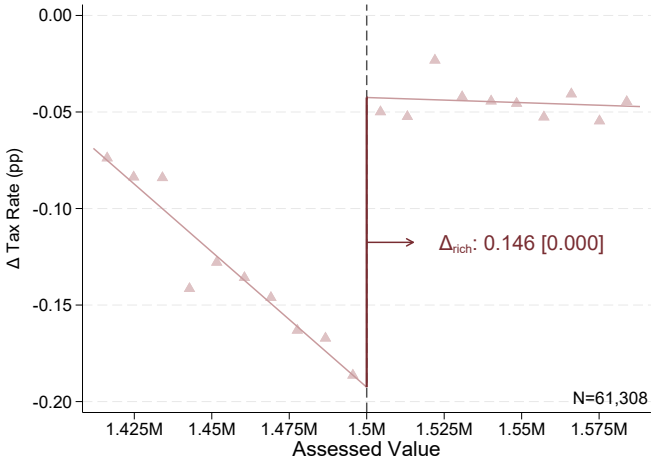
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Figure 1: Own-Rate RDD Effects on Tax Compliance for 2023

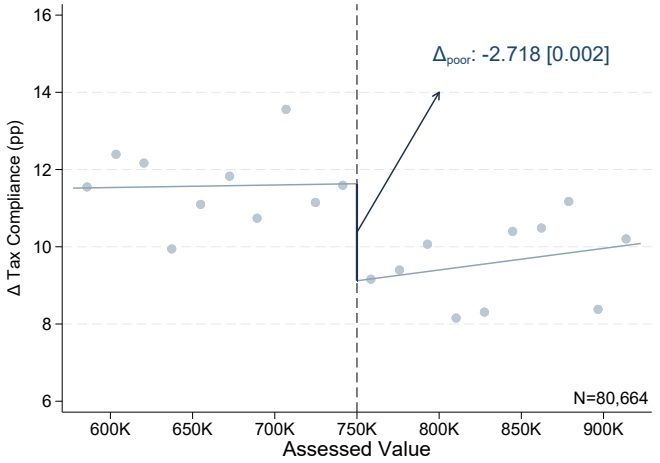
(a) First Stage: Poor Households



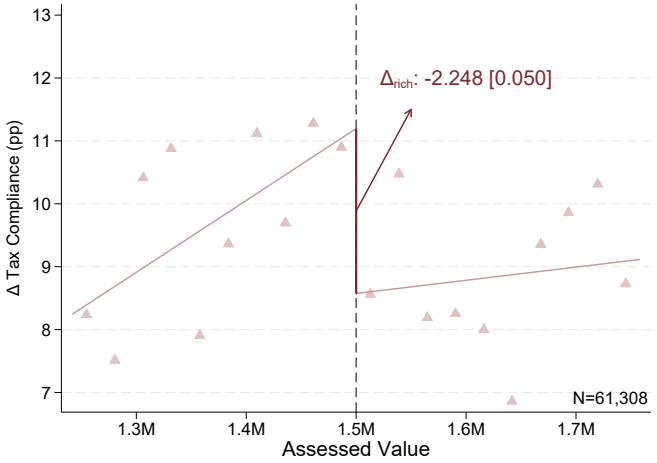
(b) First Stage: Rich Households



(c) Reduced Form: Poor Households



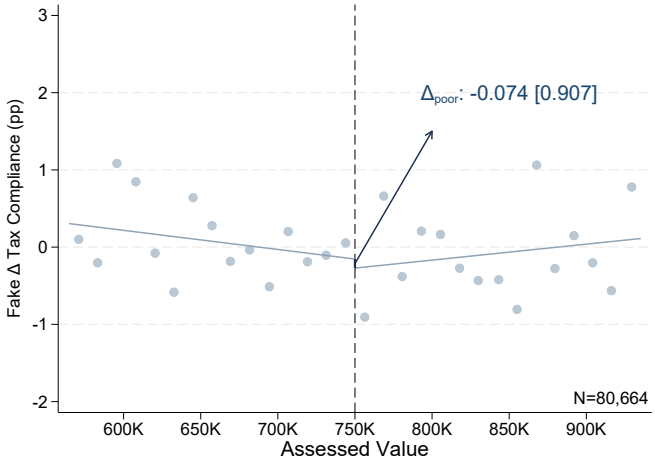
(d) Reduced Form: Rich Households



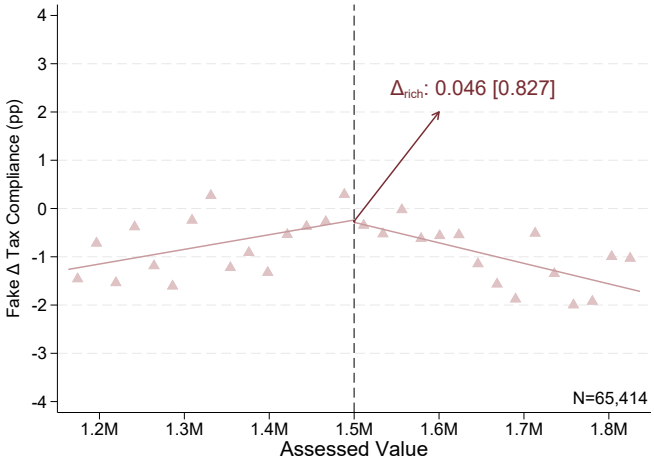
Notes: This figure calculates the *own-rate* effects of the progressive property tax reform in 2023, using a two-cutoff RDD. The left figures (in blue) show the results for the poor at the cutoff of \$750K. The right figures (in red) show the results for the rich at the cutoff of \$1.5M. The running 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 parentheses. 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



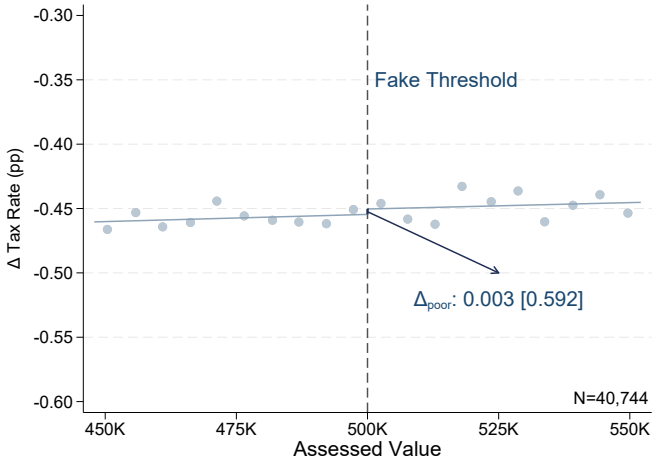
(b) Reduced Form: Rich Households



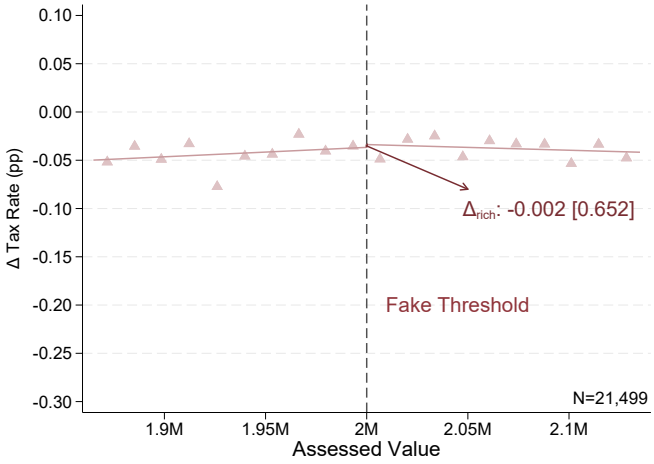
Notes: This figure calculates the *own-rate* effects using a placebo reform date in the middle of 2023. Panels (a) and (b) represent the reduced form RDD for poor and rich, in which we evaluate the change in tax compliance for both groups for placebo dates between the first semester of 2023 and the last 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 Fake Thresholds for 2023

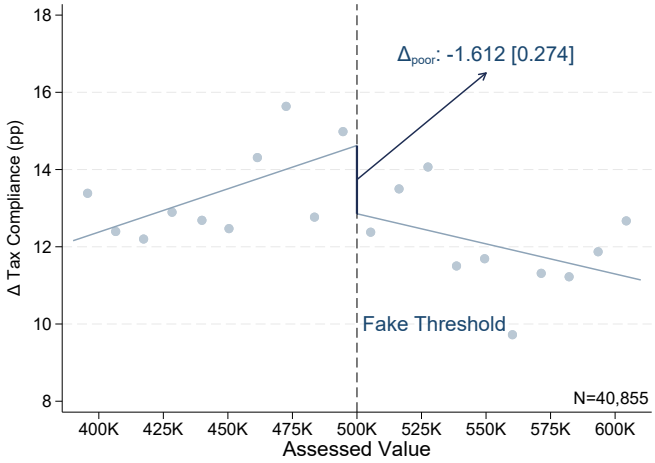
(a) First Stage: Poor Households



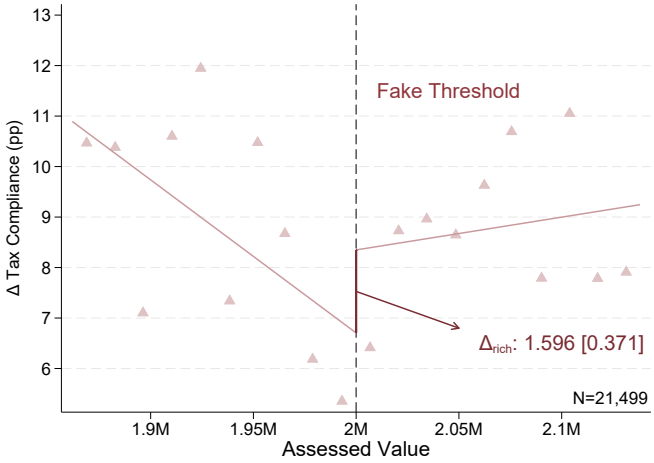
(b) First Stage: Rich Households



(c) Reduced Form: Poor Households



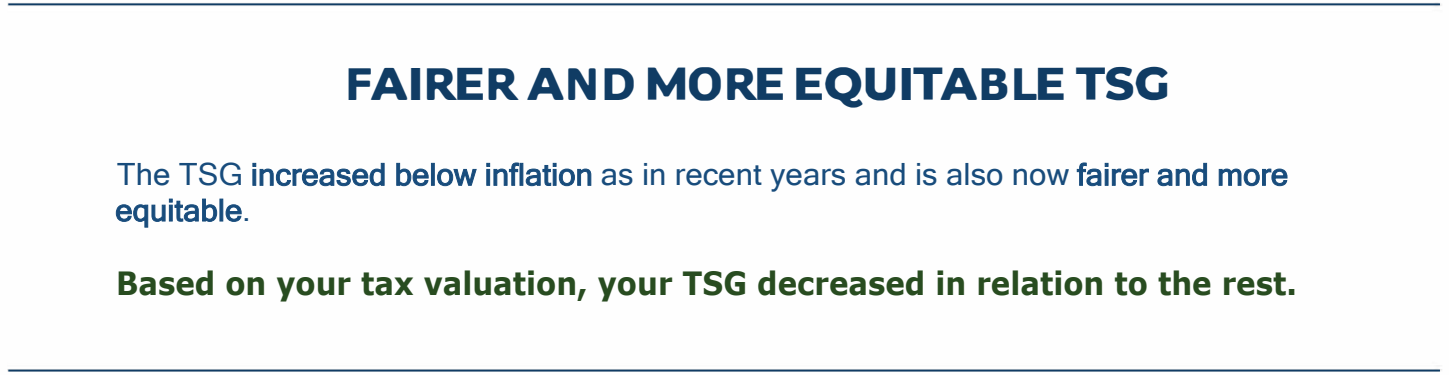
(d) Reduced Form: Rich Households



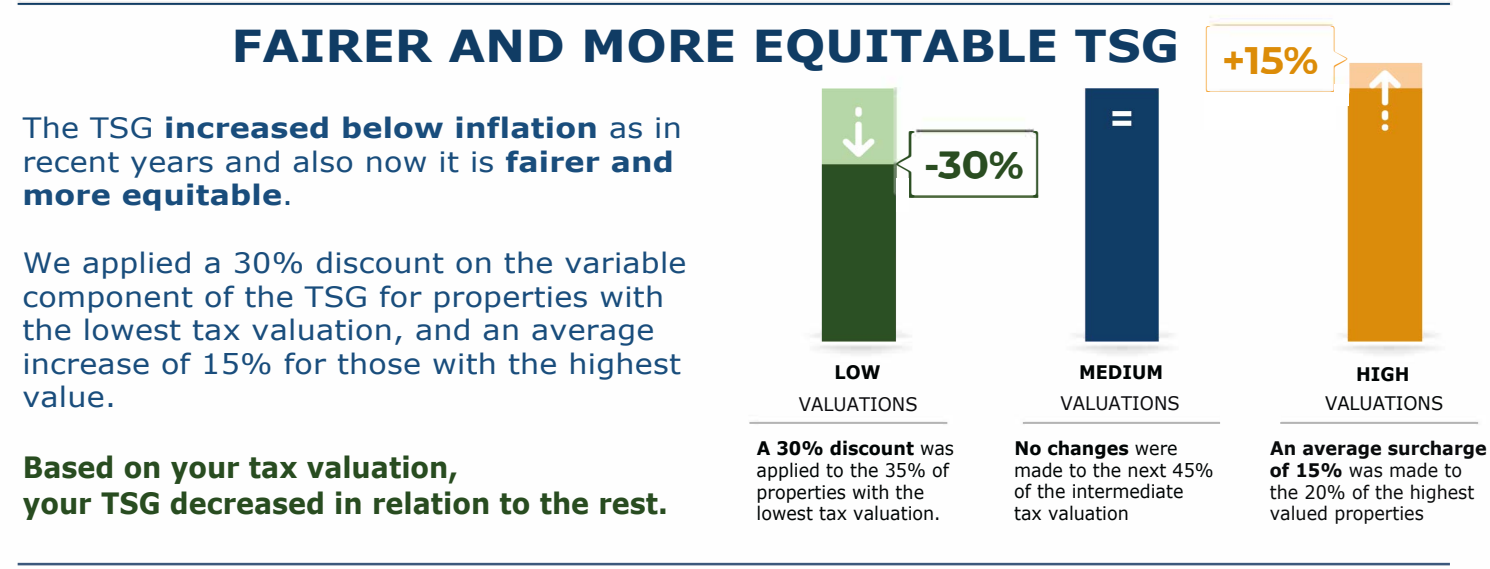
Notes: This figure calculates the *own-rate* effects using fake tax thresholds at \$500K and \$2M. Panels (a) and (b) represent the first stage RDD for poor and rich, respectively. These two figures evaluate the increase in the amounts owed between the last semester of 2022 and the first semester of 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



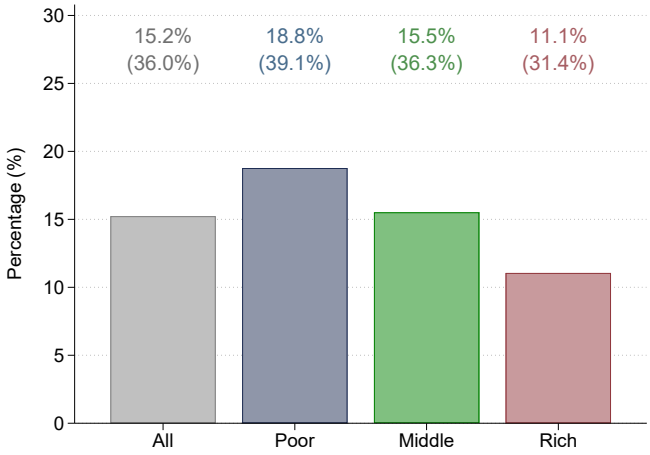
(b) Treatment Letter Message



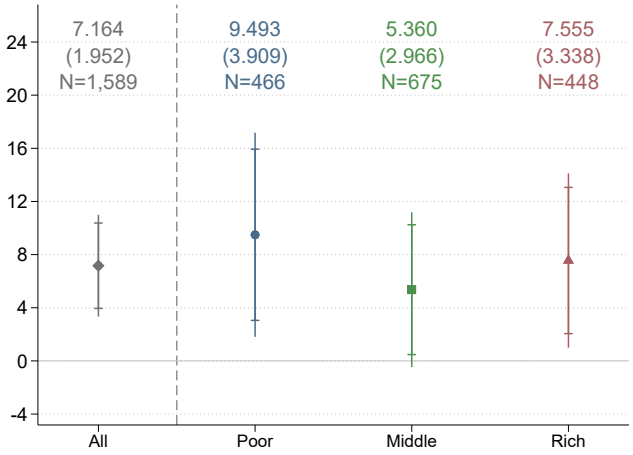
Notes: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained in the second page of the mailer (for a full sample of the mailer, see Appendix H.1).

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

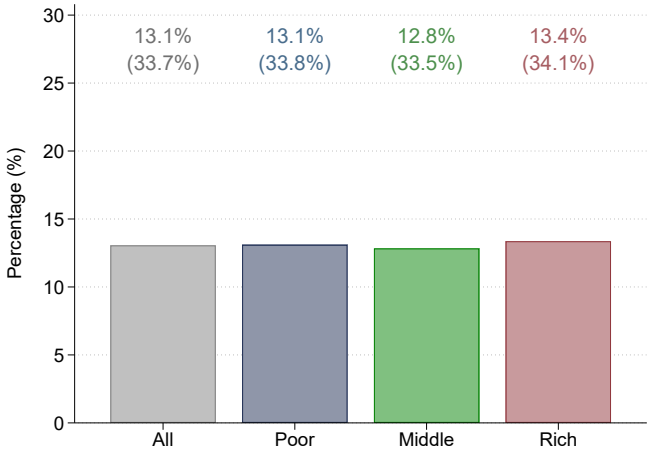
(a) Control: Awareness of TSG Reform



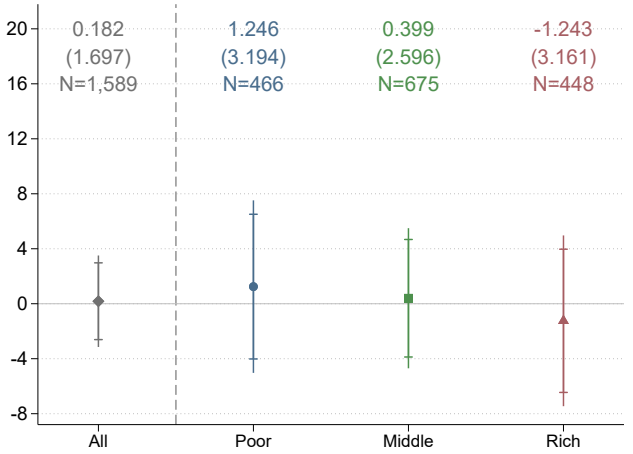
(b) ATE: Awareness of TSG Reform



(c) Control: Awareness of Business Loans



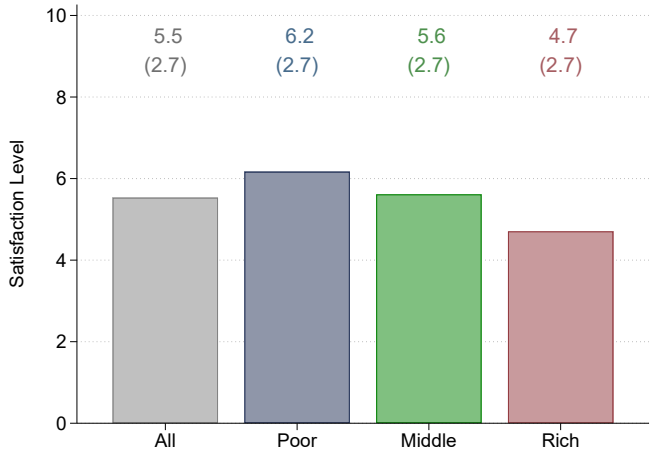
(d) ATE: Awareness of Business Loans



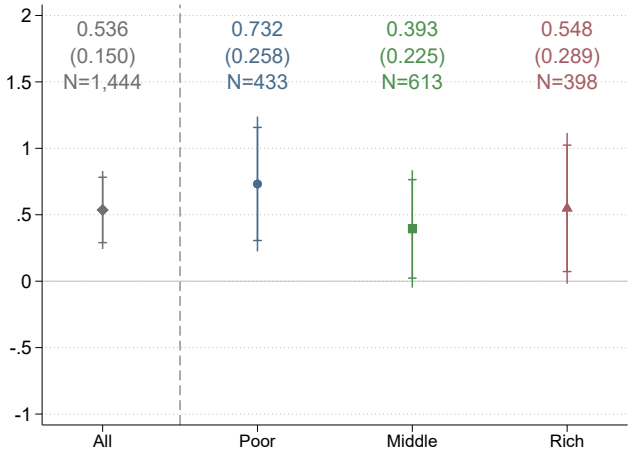
Notes: This figure uses data from our short online survey on taxpayers to assess awareness of the progressive tax reform. We invited a small subsample of subjects with email addresses on file to participate. The respondents received a list of recent policies and were asked to indicate which ones they 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. While in the control group a minority of households are aware of the reform, the awareness is about 50% higher in treatment households. Control mean and treatment effects are displayed, while we also include in parenthesis the standard deviation and the standard errors.

Figure 6: The Effect on Fairness Perceptions: Survey Experiment

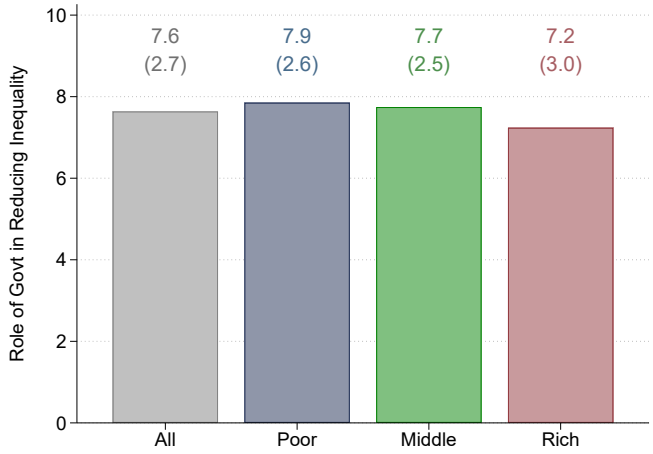
(a) Control - Fairness of Actual TSG



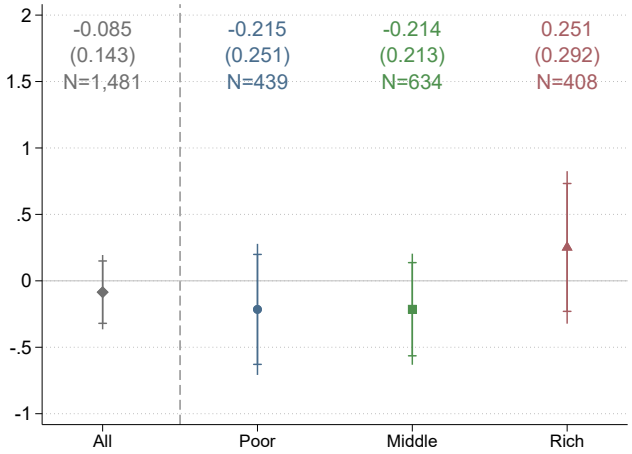
(b) ATE - Fairness of Actual TSG



(c) Control - Role of Govt in Reducing Inequality



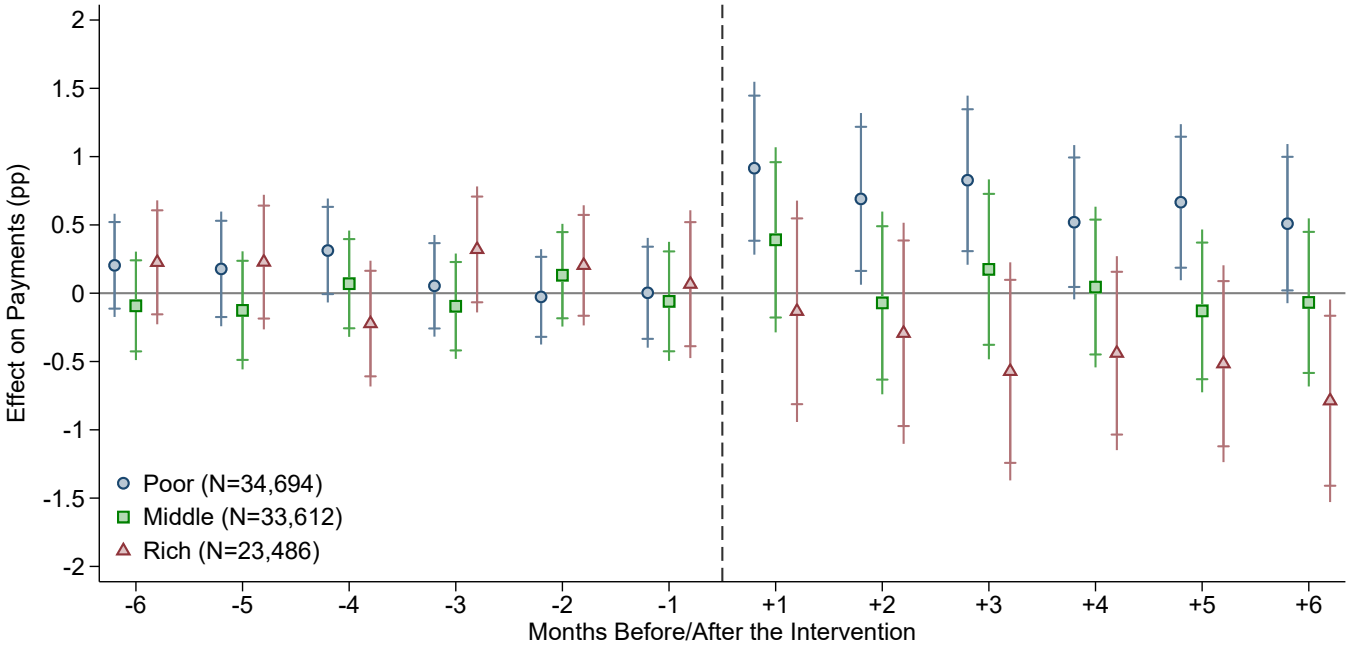
(d) ATE - Role of Govt in Reducing Inequality



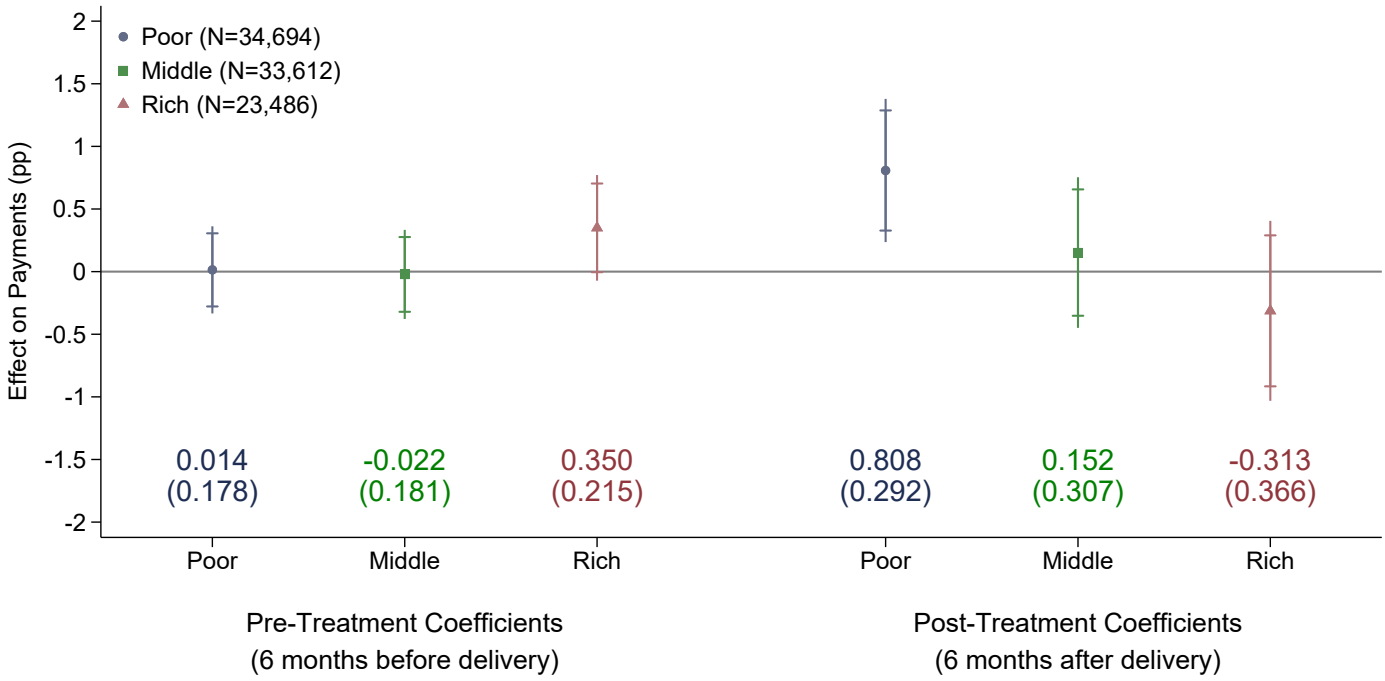
Notes: This figure uses data from our short online survey on taxpayers to estimate the impact of our progressivity information experiment on taxpayers' fairness perceptions. We asked respondents how fair they considered the distribution of the municipal tax rates between rich and poor households, on a scale from 0 to 10 (top panels) and their perception regarding the government's role in addressing inequality (bottom panels). In both cases, a higher value implies a more progressive view of the world. The left panels show the average levels for households receiving the control letter. The right panels show the average effects of the treatment letter. The vertical spikes denote 90% and 95% confidence intervals. Panel (a) indicates relatively high perceived levels of fairness. Panel (b) shows that our information provision treatment increased this perception across the board by about 0.54 points in the 0-10 scale. Reassuringly, Panel (d) suggests that our information treatment did not affect respondents' redistribution preferences. Control mean and treatment effects are displayed, while we also include in parenthesis the standard deviation and the standard errors.

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

(a) Event-Study Analysis (Monthly Level)



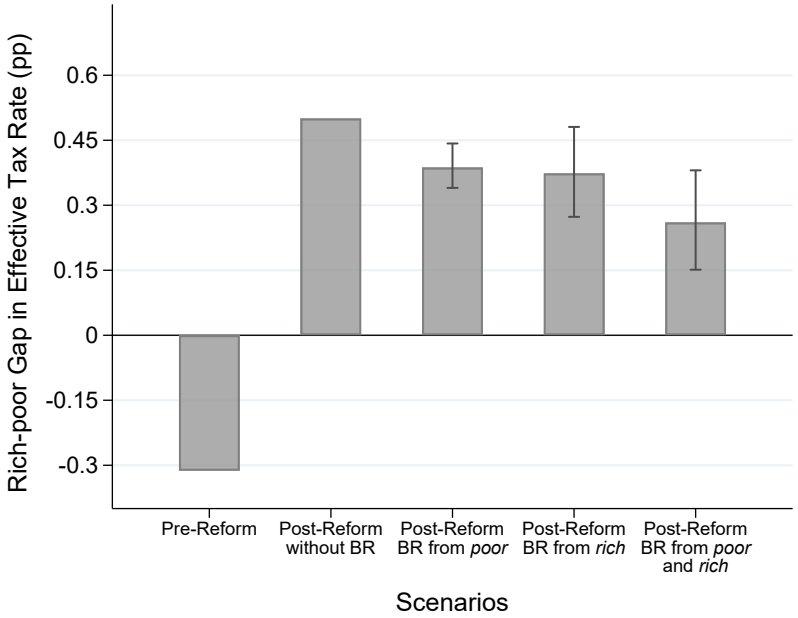
(b) Pre- and Post-Treatment Effects (Semester-level)



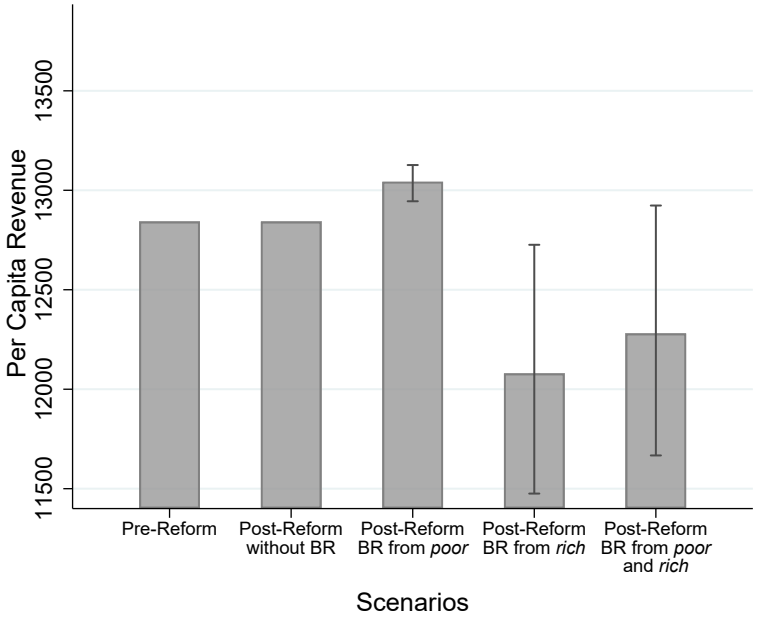
Notes: This figure compares the likelihood of paying the monthly property tax bill between treatment and control. The control group received a letter with information solely about how the reform changed the household's own tax rate. The treatment group letter included additional information about how 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 months pooled effects of the treatment (pre and post treatment). 'Poor' (in blue) denotes households with properties valued at \$750K or less, 'Middle' (in green) denotes properties valued between \$750K and \$1.5M, and 'Rich' (in red) denotes properties valued more than \$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 residences and shops 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. 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)



Notes: 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 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 2,500 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 11, 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.[43] 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.[44] 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.[45] 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 variable rates that generally exceed those applied to residential properties. Property assessments are based on the cadastre maintained by the Revenue

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

[44] 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.

[45] 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 $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.[46]

Figure A.1 illustrates that for properties in the bottom decile, fixed charges comprise 70% of the tax bill and variable charges 30%. In contrast, for the top decile, fixed charges account for only 15% of the bill, with variable charges making up 85%. These fixed charges make the overall tax schedule somewhat regressive, as shown in Figure A.2.

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.[47]

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 $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 $750K or below, kept it unchanged for properties between $750K and $1.5M, retained the 10% surcharge for properties valued between 1.5M and $3M (now based on an updated cadastre), and imposed a 20% surcharge for properties above $3M. For a *residential* taxpayer i in cadastral value bracket b, the monthly tax became:

[46] 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.

[47] 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:

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

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 \$750K; no change for properties between \$750K and \$1.5M; approximately a 39% increase for properties between \$1.5M and \$3M; and approximately a 64% increase for properties above \$3M. Thus, for a residential taxpayer i in cadastral value bracket b, the monthly tax is given by:

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

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:

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

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

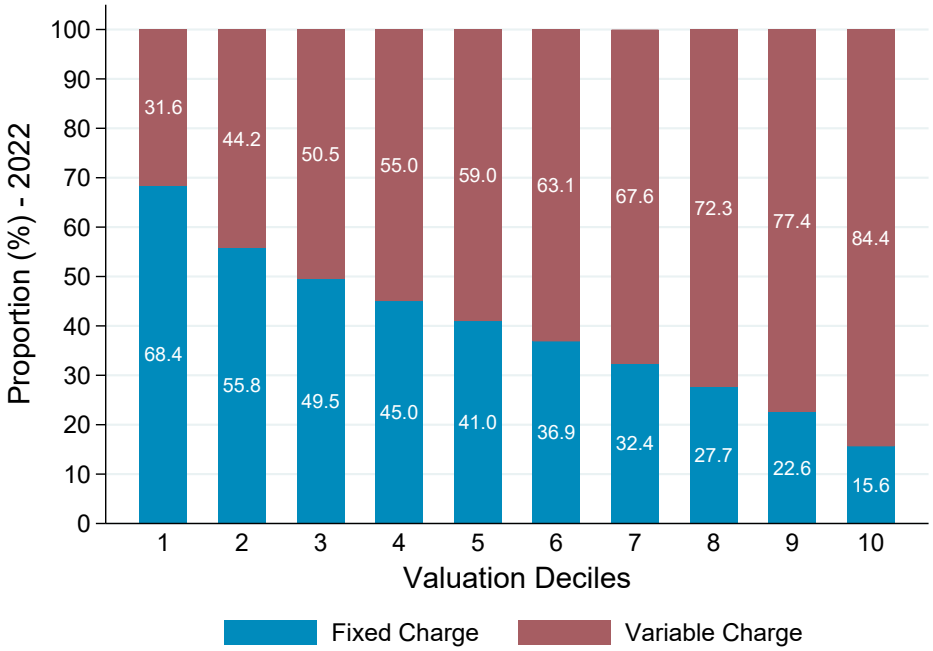
Note: This table reports statutory tax rates by category of property as defined in the tax code (*Ordenanza Impositiva*) every year. The first two columns display the number of properties in 2018. The tax base used to calculate the monthly fee corresponds to the assessed value in 2018 provided by the 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			
			$\leq$ \$750K	\$750K-\$1.5M	\$1.5M-\$3M	$>$ \$3M
Unit modules (\$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

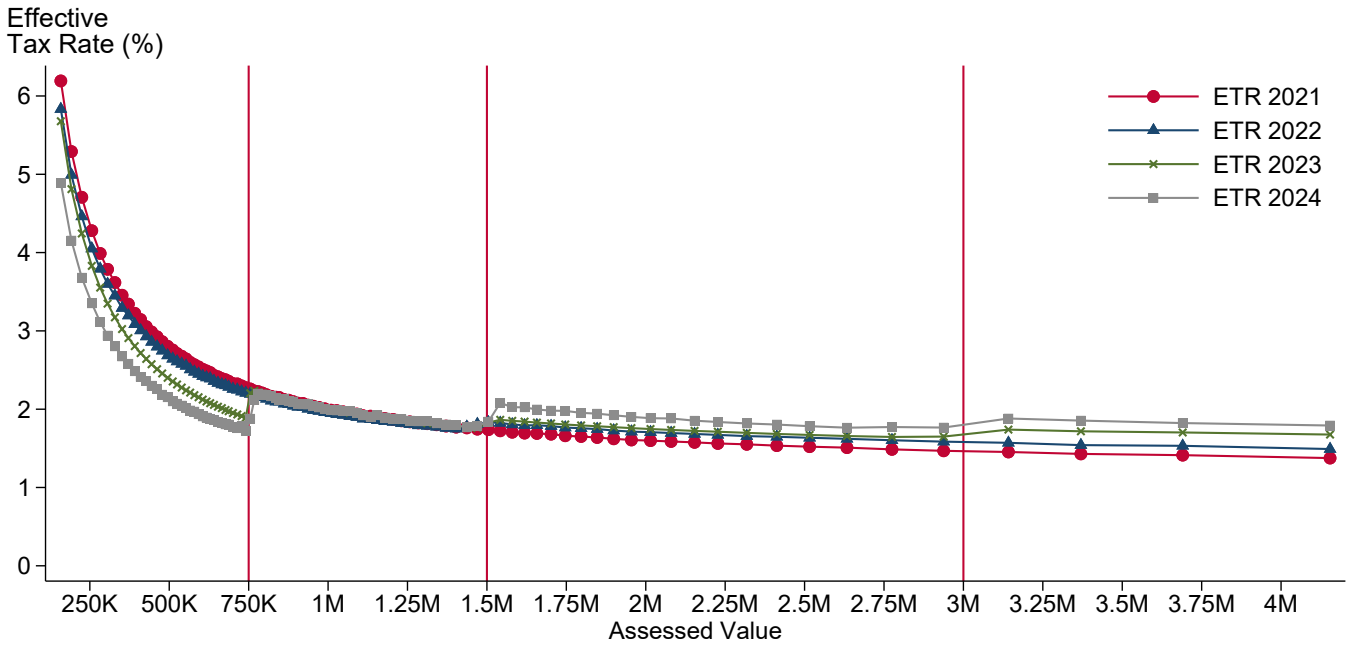
Note: The municipality adjusts the tax bill annually for inflation. Since 2024, they have adjusted the bill every other month to keep up with the rising inflation. The module factor is 3.6 in January, 5.1 in March, 7 in May, 8.4 in July, and 9 in September.

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



Notes: 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.

Figure A.2: Effective Tax Rate for Households by Percentiles of Assessed Property Values



Notes: This figure shows the effective tax rates for households (ETR) for 2021-2024, by percentiles of cadastral values. The red vertical lines denote the thresholds used to make the property tax progressive in 2023 and 2024. For comparison purposes, the ETRs are calculated by first adjusting each year's tax liability to 2022 pesos, using the middle percentile group as the reference. Properties below the 2.5th percentile and above the 97.5th percentile were excluded to enhance the clarity of the figure.

B Robustness Checks

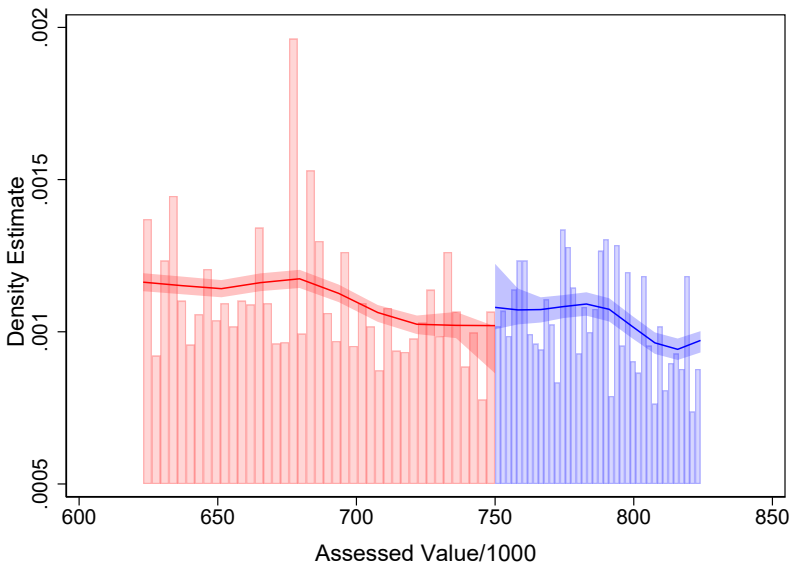
B.1 Manipulation Test

We conduct the standard McCrary (2008) manipulation test to assess the continuity of the running variable's density function. Intuitively, taxpayers might attempt to influence their own-rate treatment status by adjusting their property's assessed values to fall just above the \$750K threshold or below the \$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).

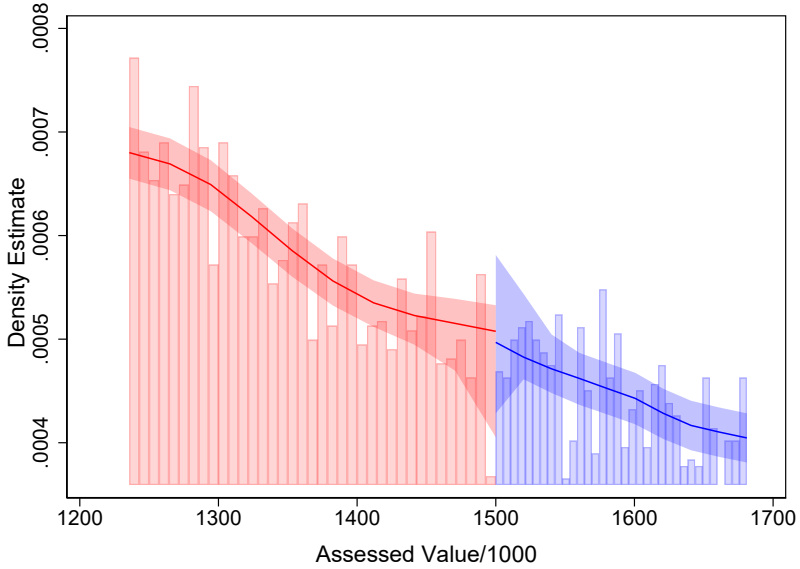
The McCrary density test and accompanying figures reveal no evidence of unusual bunching around the thresholds that determine treatment eligibility. While we detect a slight discontinuity for low-value properties, the institutional context suggests this is unlikely to reflect deliberate manipulation. 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



Notes: Panels (a) and (b) show the McCrary (2008) manipulation test of the property assessed values at the low-value threshold (poor) and the high-value threshold (rich) in 2023.

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, and (iii) alternative definitions of the outcome variable. 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) using an alternative definition of the outcome, and (iv) 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)
Effect of Reform on Tax Compliance:						
Low-Valuation HHs	−2.627***	−1.531**	−3.194***	−2.514***	−3.931***	−1.358
	[0.002]	[0.025]	[0.001]	[0.004]	[0.001]	[0.240]
					[pval diff.=0.142]	
High-Valuation HHs	−1.977*	−1.307	−2.308**	−2.146**	−1.208	−3.302*
	[0.062]	[0.114]	[0.038]	[0.040]	[0.680]	[0.065]
					[pval diff.=0.304]	
Effect of Reform on Tax Rates:						
Low-Valuation HHs	0.615***	0.615***	0.626***	0.615***	0.607***	0.615***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
					[pval diff.=0.233]	
High-Valuation HHs	0.253***	0.266***	0.251***	0.253***	0.232***	0.258***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
					[pval diff.=0.706]	
Specification						
Additional Controls		✓				
Uniform Kernel			✓			
Alternative outcome (6 months of late payment)				✓		
Just Control Sample					✓	
Just Treatment Sample						✓
Observations (Compliance)						
Low-Valuation HHs	30,589	31,332	20,756	35,344	32,806	32,615
High-Valuation HHs	69,386	59,735	69,386	69,386	15,009	16,243
Observations (Rates)						
Low-Valuation HHs	10,563	9,080	11,501	10,563	32,806	32,615
High-Valuation HHs	69,386	59,735	69,386	69,386	6,660	5,936
Average Tax Compliance (Baseline)						
Low-Valuation HHs	59.8%	59.7%	60.5%	59.9%	57.3%	56.8%
High-Valuation HHs	56.6%	56.7%	56.3%	56.7%	60.5%	60%
Average Tax Rate (Baseline)						
Low-Valuation HHs	2.3%	2.3%	2.3%	2.3%	2.2%	2.3%
High-Valuation HHs	2.3%	2.3%	2.3%	2.3%	2.2%	2.2%

Notes: The 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) uses a alternative outcome that includes the possibility of paying 6 months after the due date instead of only 3 months later. Finally, columns (5) and (6) 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 2,500 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)
Effect of Reform on Tax Compliance:					
Low-Valuation HHs	0.808***	0.795***	0.698**	0.570**	0.806**
	(0.292)	(0.295)	(0.278)	(0.290)	(0.320)
Medium-Valuation HHs	0.152	0.170	0.132	0.166	0.152
	(0.307)	(0.309)	(0.286)	(0.304)	(0.307)
High-Valuation HHs	−0.313	−0.421	−0.245	−0.254	−0.446
	(0.366)	(0.369)	(0.341)	(0.367)	(0.405)
Specification					
Just pre-treat payment controls		✓			
Email Sample Included			✓		
Alternative outcome (6 months of late payment)				✓	
No valuation outliers					✓
Observations					
Low-Valuation HHs	34,694	34,694	38,678	34,694	29,747
Medium-Valuation HHs	33,612	33,612	38,325	33,612	33,612
High-Valuation HHs	23,486	23,486	26,716	23,486	19,323
Average Tax Compliance (Baseline)					
Low-Valuation HHs	54.5%	54.5%	57.3%	54.5%	55.5%
Medium-Valuation HHs	56.0%	56.0%	59.1%	56.0%	56.0%
High-Valuation HHs	56.1%	56.1%	59.0%	56.1%	55.8%

Notes: The 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 sample, column (4) uses an alternative outcome that includes the possibility of paying 6 months after the due date instead of only 3 months later, and column (5) excludes observations with valuations below and above, respectively, the 5th percentile and the 95th percentile. Clustered standard errors 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.

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, confirming no significant differences in baseline characteristics.

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	0.292	42.4	41.9	0.306	44.7	45.0	0.531	46.1	45.1	0.114
	(45.8)	(45.8)	(45.8)		(45.4)	(45.5)		(45.7)	(45.8)		(46.4)	(46.1)	
Share of Payments (2022) (%)	43.5	43.7	43.3	0.270	41.6	41.2	0.344	44.2	44.4	0.697	46.0	45.1	0.132
	(45.2)	(45.1)	(45.2)		(44.7)	(44.7)		(45.1)	(45.2)		(45.6)	(45.6)	
2021 Assessed Value (million $s)	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)	(1,431.9)	
2022 Tax Rate (%)	2.000	1.997	2.003	0.593	2.425	2.434	0.779	1.652	1.651	0.838	1.862	1.870	0.436
	(1.873)	(1.786)	(1.957)		(2.716)	(3.021)		(0.588)	(0.495)		(0.805)	(0.821)	
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	

Notes: 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) (%)	43.1	43.3	42.9	0.671	44.5	43.1	0.415	38.4	39.2	0.624	45.2	44.7	0.673
	(45.6)	(45.8)	(45.5)		(45.6)	(45.1)		(45.2)	(45.1)		(45.9)	(45.7)	
2021 Assessed Value (million $s)	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)	(1,027.1)		(18.5)	(18.7)		(21.7)	(21.4)		(1,163.1)	(1,429.1)	
2022 Tax Rate (%)	3.154	3.151	3.157	0.855	3.727	3.677	0.615	2.810	2.852	0.400	3.044	3.056	0.649
	(1.609)	(1.867)	(1.300)		(3.304)	(1.431)		(0.912)	(1.620)		(0.985)	(0.930)	
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	

Notes: 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) (%)	50.2	50.2	50.1	0.894	50.0	49.9	0.806	50.4	50.4	0.989	50.2	50.2	0.948
	(46.1)	(46.1)	(46.2)		(46.4)	(46.4)		(45.9)	(46.0)		(46.0)	(46.1)	
2021 Assessed Value (million $s)	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 (%)	2.357	2.356	2.358	0.837	2.021	2.037	0.293	2.282	2.298	0.050	2.978	2.943	0.076
	(1.263)	(1.232)	(1.293)		(1.328)	(1.491)		(0.687)	(0.731)		(1.433)	(1.387)	
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	

Notes: 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) (%)	50.1	49.0	50.2	0.092	49.0	50.0	0.396	49.9	50.4	0.660	47.7	50.2	0.073
	(46.1)	(46.2)	(46.1)		(46.4)	(46.4)		(46.0)	(45.9)		(46.3)	(46.0)	
2021 Assessed Value (million $s)	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 (%)	2.361	2.402	2.357	0.025	2.052	2.021	0.358	2.305	2.283	0.212	3.053	2.982	0.126
	(1.265)	(1.390)	(1.251)		(1.578)	(1.328)		(0.738)	(0.689)		(1.561)	(1.495)	
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	

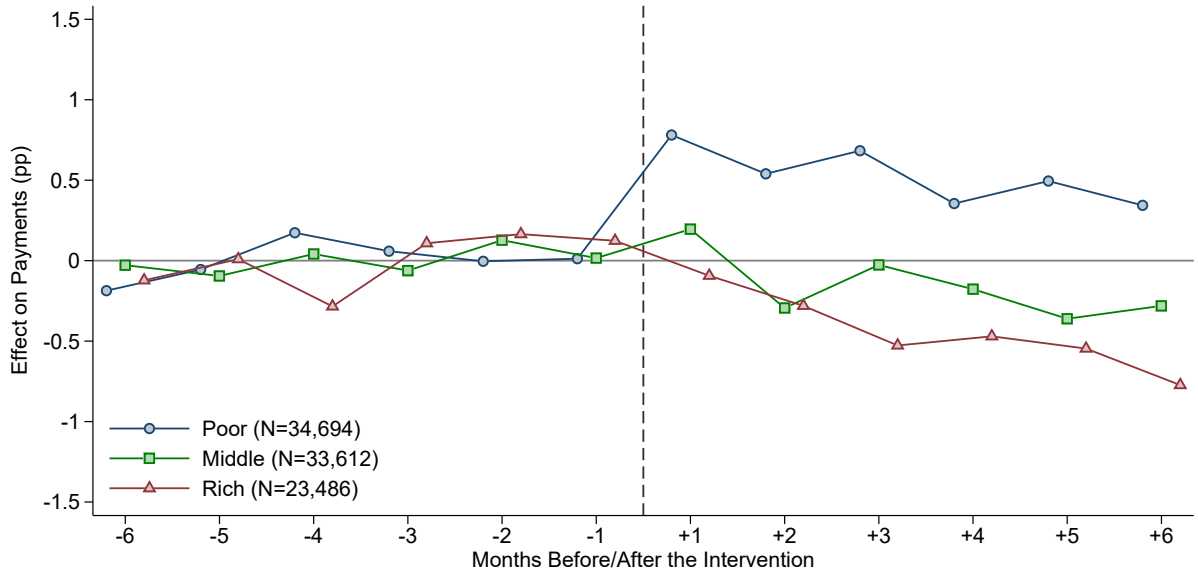
Notes: This balance table presents the difference between control and no letter groups across different categories (All, Poor, Middle, and Rich). Standard deviation in parenthesis. The p-value columns reflect the results of difference tests between control and treatment groups. Results are omitted in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

D Additional Experimental Results

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



Notes: This figure compares the likelihood of paying the monthly bill between treatment and control. The blue line denotes the poor group with assessed values below $750K. The green line denotes the middle group with assessed values between $750K and $1.5M. The red line denotes the rich group with assessed values above $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 residences and shops and excludes units that made payments for 2023 before we sent the letters.

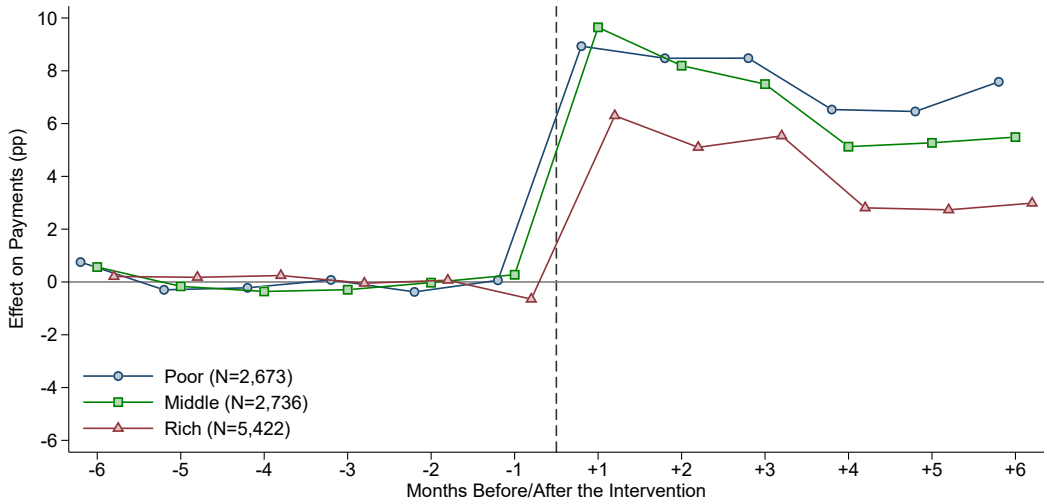
D.2 Control Letter vs No Letter

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.

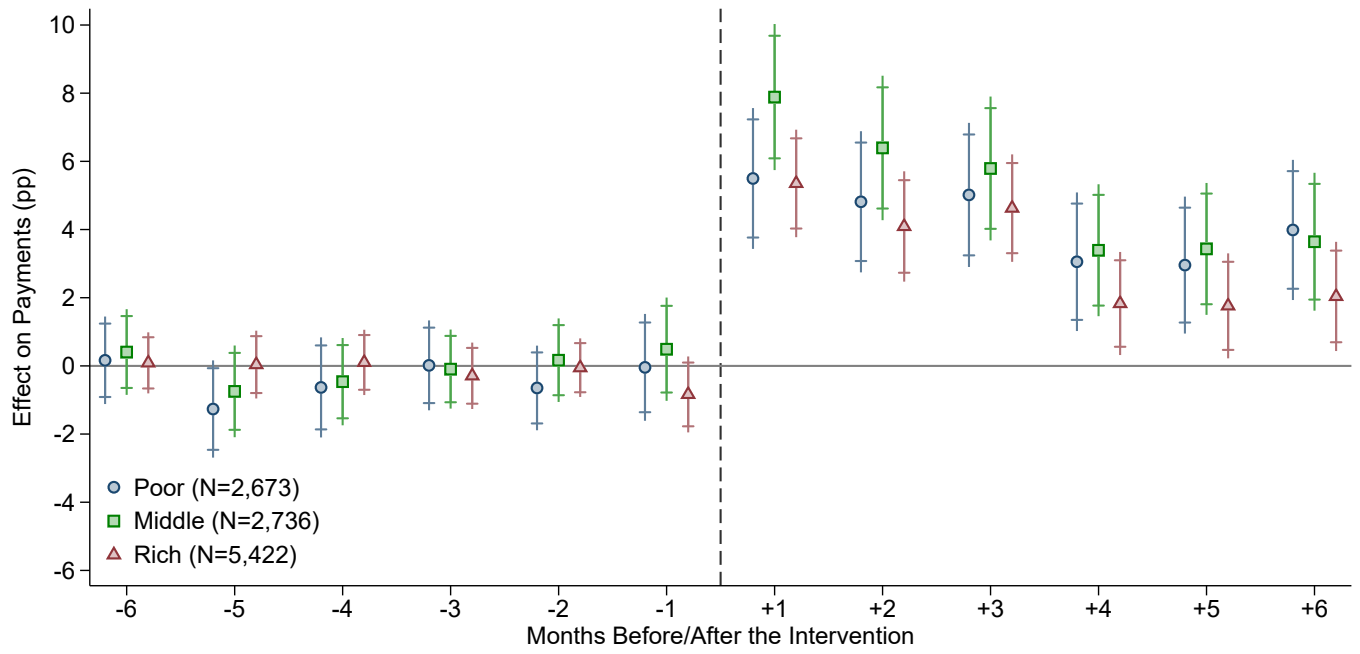
Figure D.2: Raw Difference in Payment Rates between Control and No-Letter Groups in 2023



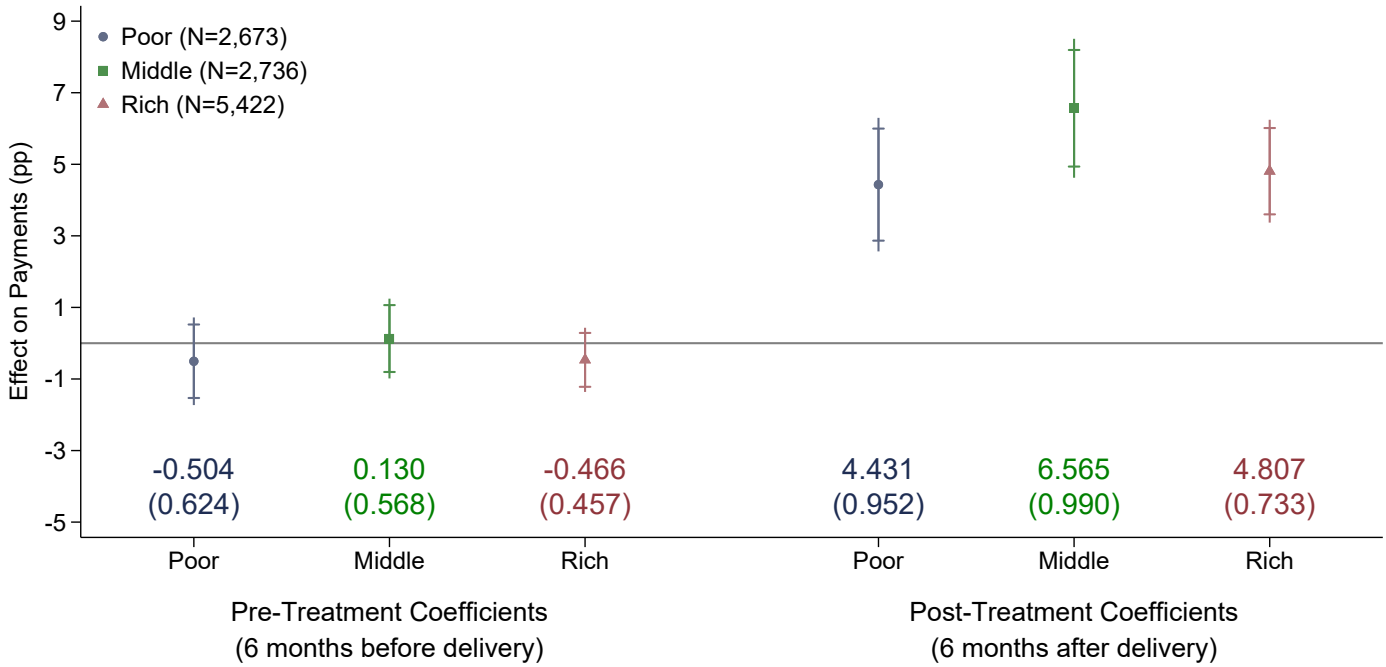
Notes: This figure compares the likelihood of paying the monthly bill between treatment and control, normalized by the average pre-treatment difference. We estimate the coefficient for each monthly bill in separate regressions. The sample includes residences and shops and excludes units that made payments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

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

(a) Dynamic Effect of the Letters



(b) Two Months Pooled Data

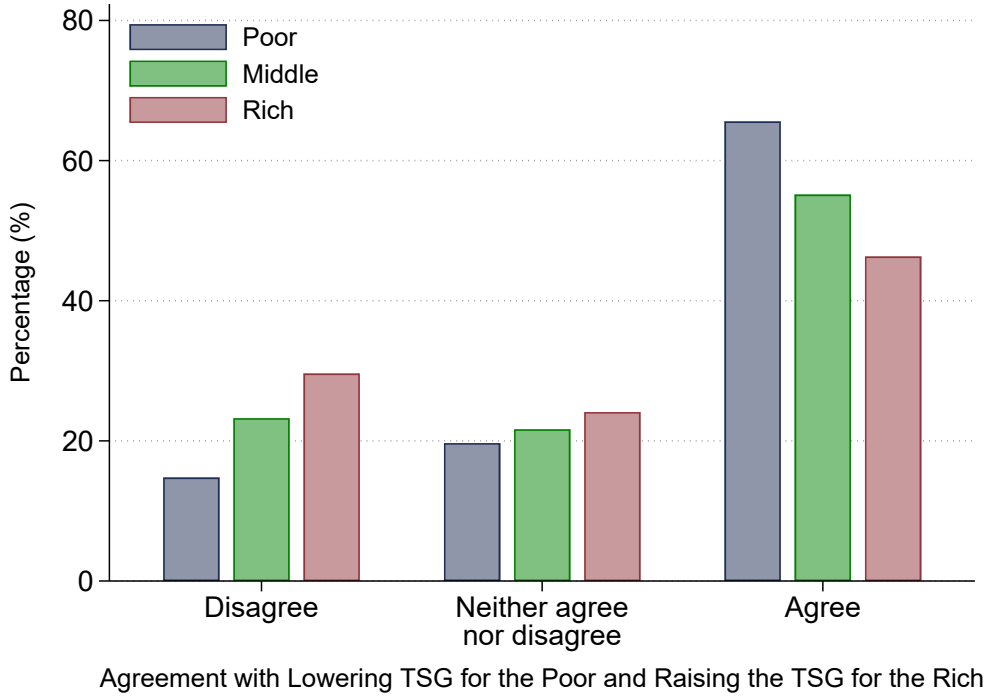


Notes: This figure compares the likelihood of paying the monthly property tax bill between the control and the no-letter groups. The control group received a letter with information solely about how the reform changed the household's own tax rate, while the no-letter group received none. Panel (a) shows 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 $750K or less, 'Middle' (in green) denotes properties valued between $750K and $1.5M, and 'Rich' (in red) denotes properties valued more than $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. Standard errors clustered at the individual taxpayer level.

D.3 Support for Redistribution

Figure D.4 presents survey findings on taxpayers' preferences for redistribution. Respondents rated their agreement, on a scale from 0 to 10, with the idea of lowering property tax rates for the poor while increasing them for the rich. The results show that the majority of taxpayers support making the tax system progressive. However, taxpayers owning high-value properties differ from the poor group in their behavioral response; despite their stated support for redistribution, they do not translate this into increased tax compliance. At best, their payment behavior remains unchanged after learning about the system's progressivity (see Figure 7).

Figure D.4: Preferences for Redistribution by Group



Notes: Variable constructed based on the justice perception of the TSG responses from the 2023 survey experiment. The initial variable took integer values from 0 to 10. Those who rated between 0 and 4 were included in the "Disagree" group, those who rated 5 were included in the "Neither agree nor disagree" group, and those who rated between 6 and 10 inclusive were included in the "Agree" group.

E Replication of the 2023 Findings in 2024

E.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.[48] 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 E.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 4.4 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.

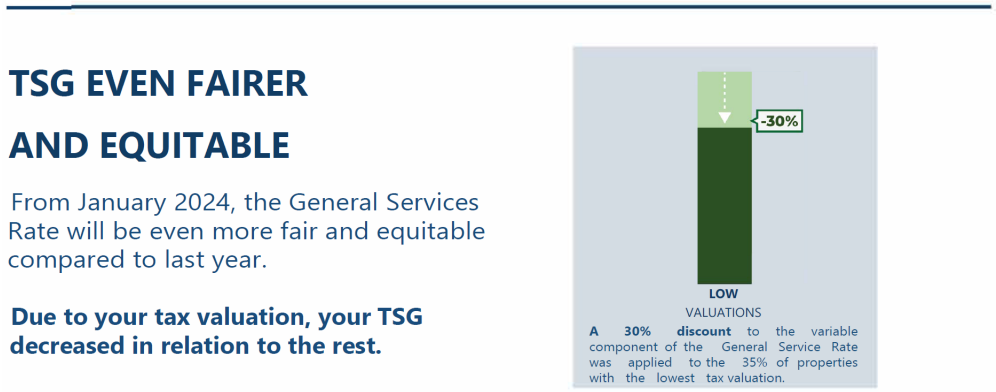
An example of the treatment and control infographic for poor households is shown in Figure E.1, while Appendix H.2 provides a full sample letter along with the treatment and control infographics for middle and rich households. One important detail is the information about the discounts and surcharges. 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

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

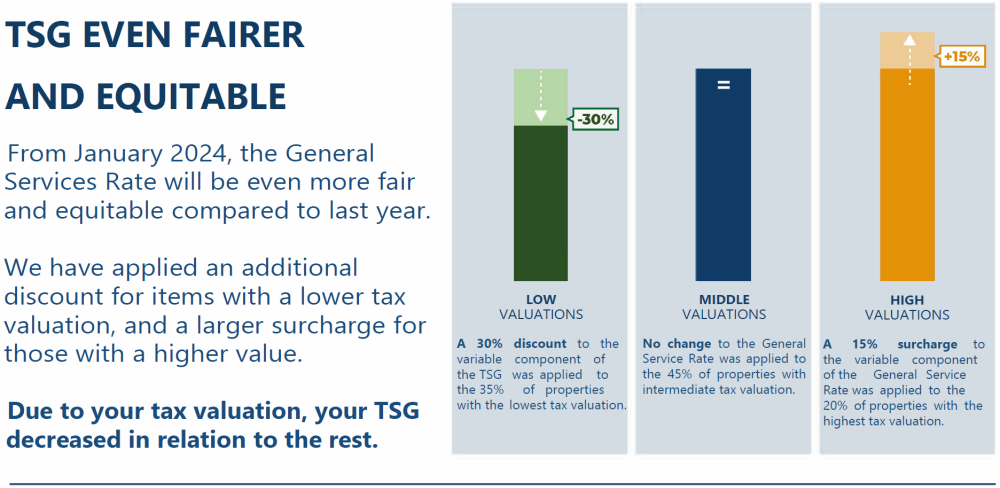
fixed component adjustment introduced in 2024. The justification for this approach was that when considering the total tax liability from 2022 to 2024, the cumulative effect of the reforms translated into an approximate 22.3% reduction for poor households and a 17.7% increase for rich households. Since 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 E.1: Sample of Control and Treatment Messages: Poor Households – 2024 Experimental Design

(a) Control Letter Message



(b) Treatment Letter Message



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

E.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 E.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 E.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 E.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 the middle group (just above or below the cutoff), and τ represents the tax rate of the middle group (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 the middle group, focusing on middle-value properties situated near the thresholds – above for poor households and below for rich households. The computed elasticities are as follows:[49]

$$\varepsilon^{own}_{poor} = \frac{\frac{\beta^{own}_{poor}}{C^{own}_{poor}}}{\frac{\alpha_{poor}}{\tau_{poor}}} = \frac{\frac{-2.559}{35.3}}{\frac{0.567}{2.252}} = \underset{[-0.523;-0.083]}{\text{-0.288}} \tag{E.1}$$

$$\varepsilon^{own}_{rich} = \frac{\frac{\beta^{own}_{rich}}{C^{own}_{rich}}}{\frac{\alpha_{rich}}{\tau_{rich}}} = \frac{\frac{-2.605}{35.6}}{\frac{0.294}{1.894}} = \underset{[-1.100;-0.037]}{\text{-0.471}} \tag{E.2}$$

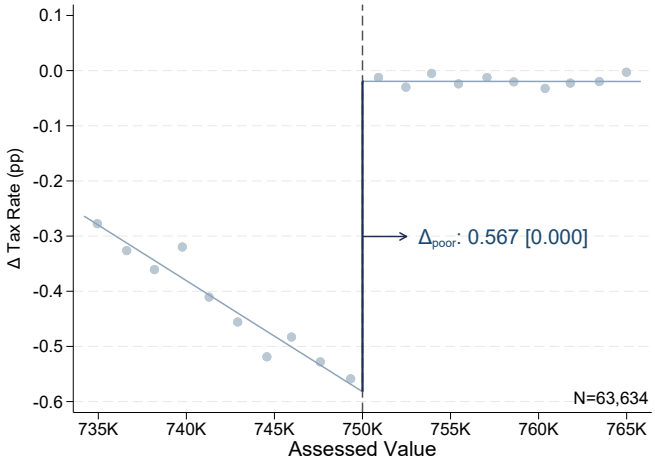
We estimate a significant *own-rate* elasticity for poor households of -0.288, indicating that for every 1% decrease in their tax rate, compliance increases by 0.288%. Similarly, the *own-rate* elasticity for rich households is -0.471, implying that for every 1% increase

[49] We also calculated confidence intervals at 90% significance using 2,500 bootstrap iterations for these elasticities.

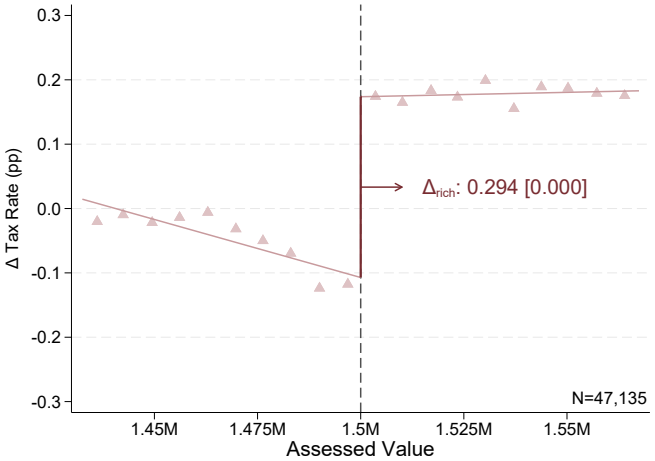
in their tax rate, compliance decreases by 0.471%. To test whether the difference in elasticities between the two groups is statistically significant, we conducted 2,500 bootstrap iterations, which revealed no significant difference (p-value = 0.684).

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

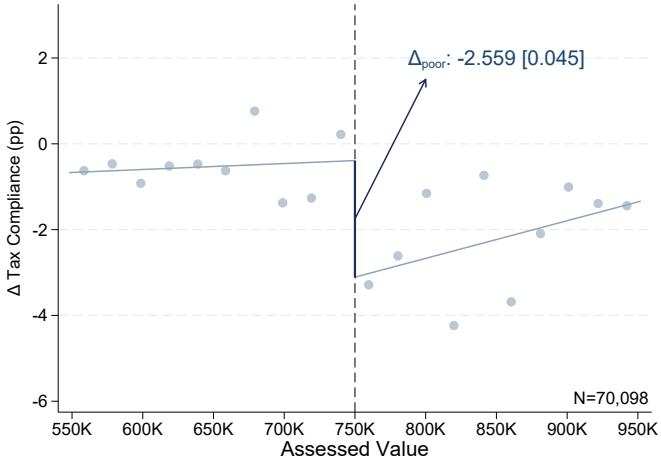
(a) First Stage: Poor Households



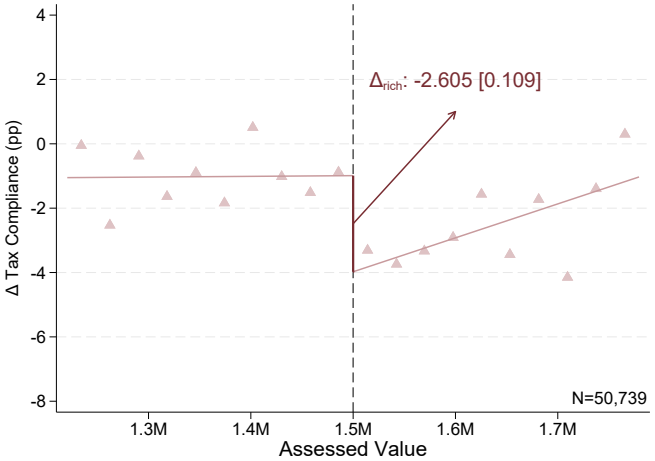
(b) First Stage: Rich Households



(c) Reduced Form: Poor Households



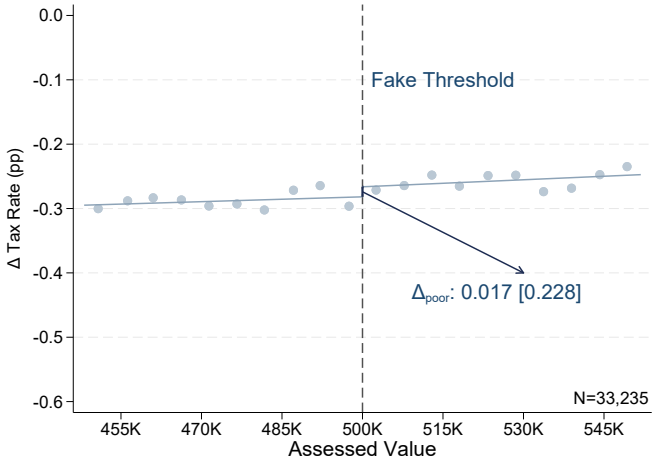
(d) Reduced Form: Rich Households



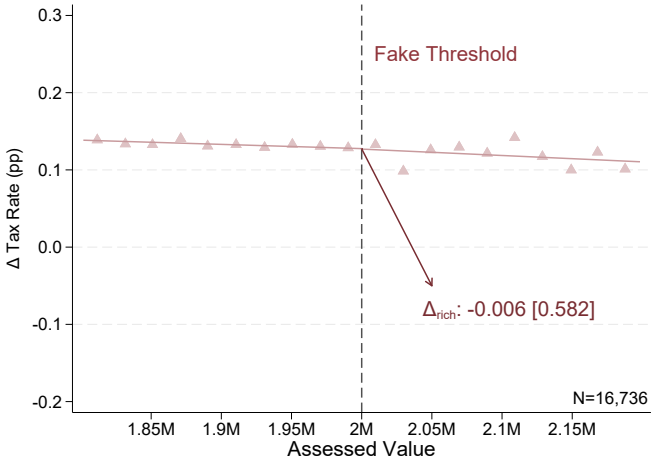
Notes: Panels (a) and (b) represent the first stage for poor and rich, in which we evaluate the increase in the amounts owed between the last bimester of 2023 and the fist bimester of 2024, in relation to the value of the property, in the cut-off of \$750K and \$1.5M, respectively. Panels (c) and (d) 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 E.3: 2024 Falsification Test: Fake Thresholds in RDD Analysis

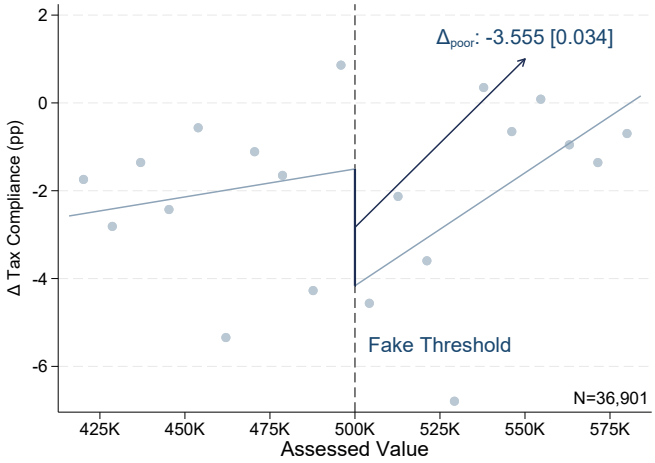
(a) First Stage: Poor Households



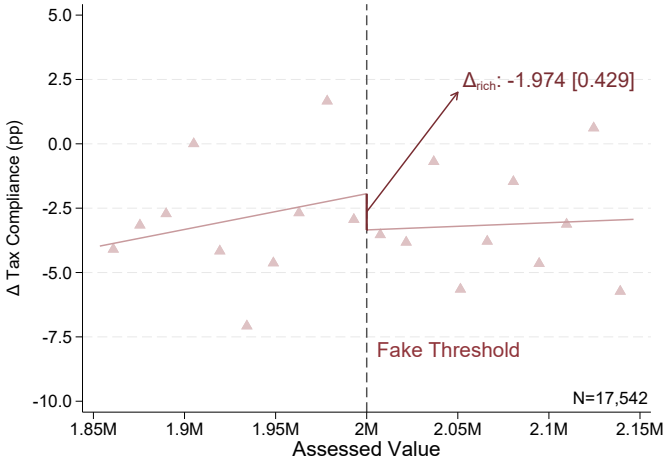
(b) First Stage: Rich Households



(c) Reduced Form: Poor Households



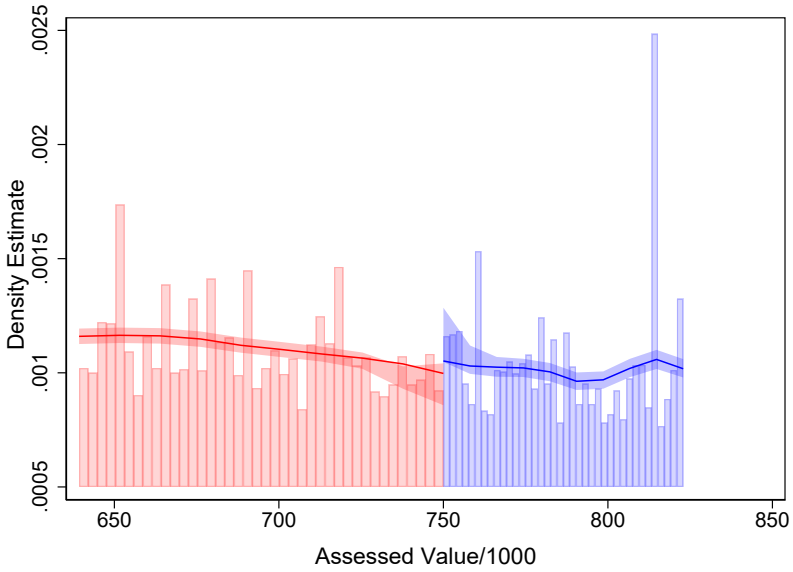
(d) Reduced Form: Rich Households



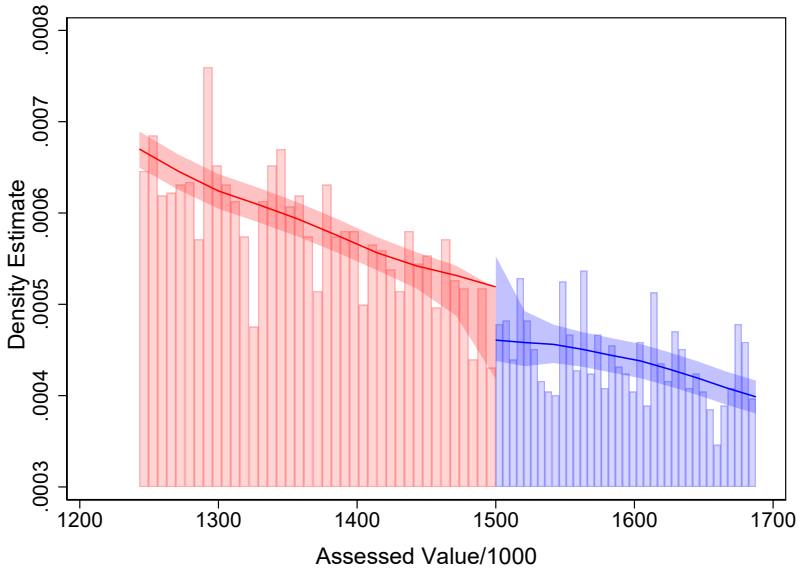
Notes: Panels (a) and (b) represent the first stage RDD for poor and rich, in which we evaluate the increase in the amounts owed between the last bimester of 2023 and the fist bimester of 2024, in relation to the value of the property, in the cut-off of \$500K and \$2M, respectively. Panels (c) and (d) 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 E.4: Manipulation Test of Assessed Values around the Discontinuities: 2024

(a) Poor Cutoff - 2024



(b) Rich Cutoff - 2024



Notes: Panels (a) and (b) show the manipulation tests for poor and rich in 2024.

E.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 E.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-value and middle-value properties in the treatment group showed no statistically significant changes.

Figure E.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, lower-value properties exhibit increased compliance in the two months following the intervention, significant at the 10% level, while high-value 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 high-value 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 the poor group and a 15% increase for the rich group, 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:[50]

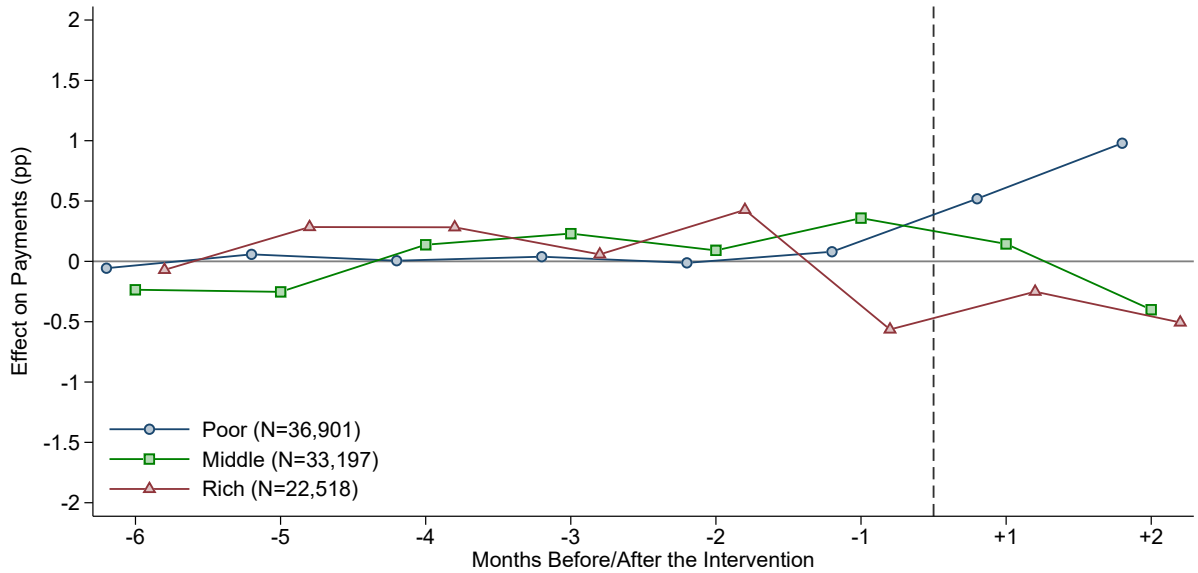
$$\varepsilon^{cross}_{poor} = \frac{\frac{\beta^{cross}_{poor}}{C^{cross}_{poor}}}{\frac{\Delta\tau_{rich}}{\tau_{rich}}} = \frac{\frac{0.721}{38.4}}{0.15} = \underset{[0.023;0.230]}{0.125} \tag{E.3}$$

[50] We also calculated confidence intervals at 90% significance using 2,500 bootstrap iterations for these elasticities.

$$\varepsilon_{rich}^{cross} = \frac{\frac{\beta_{rich}^{cross}}{C_{rich}^{cross}}}{\frac{\Delta\tau_{poor}}{\tau_{poor}}} = \frac{\frac{-0.409}{32.1}}{\text{-0.3}} = \underset{[-0.024;0.108]}{0.042} \tag{E.4}$$

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-compliance 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=0.261).

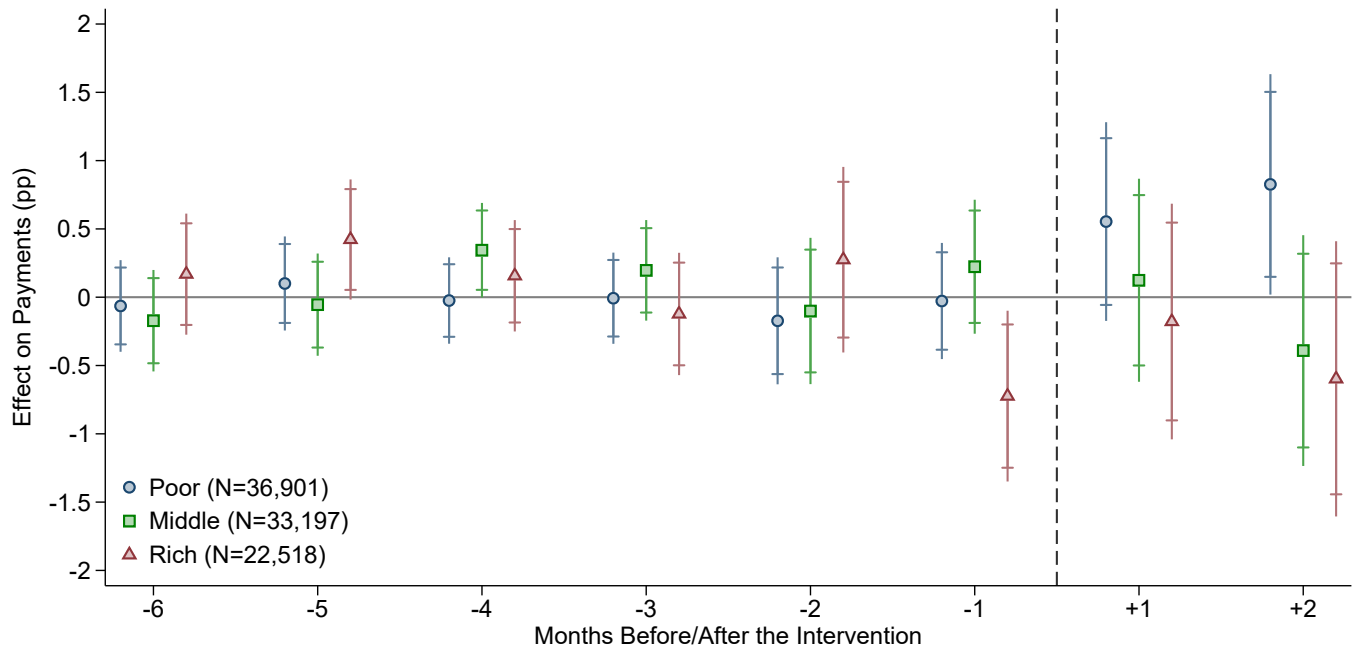
Figure E.5: Raw Difference in Payment Rates between Control and Treatment Letters: 2024



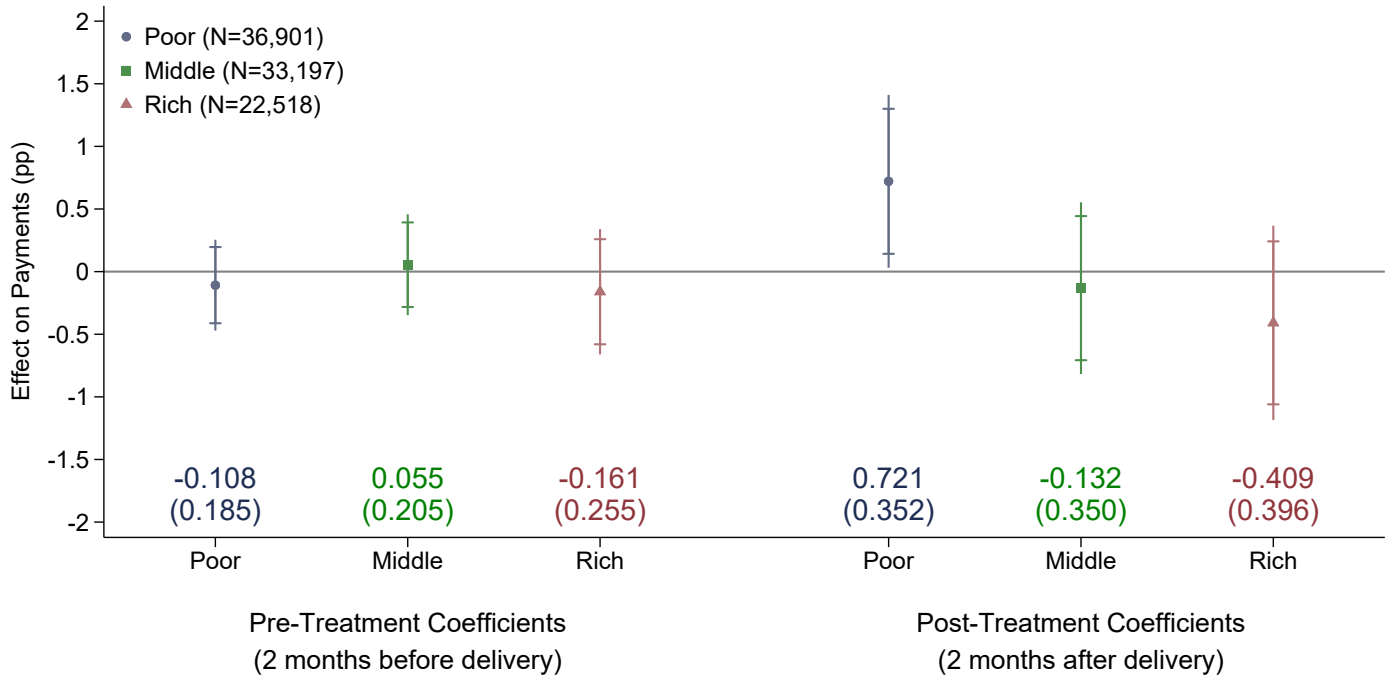
Notes: This figure compares the likelihood of paying the monthly bill between treatment and control, normalized by the average pre-treatment difference. We estimate the coefficient for each monthly bill in separate regressions. The sample includes residences and shops and excludes units that made payments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

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

(a) Dynamic Effect of the Treatment



(b) Two Months Pooled Data



Notes: 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 E.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 $750K or less, 'Middle' (in green) denotes properties valued between $750K and $1.5M, and 'Rich' (in red) denotes properties valued more than $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 residences and shops 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. Standard errors clustered at the individual taxpayer level.

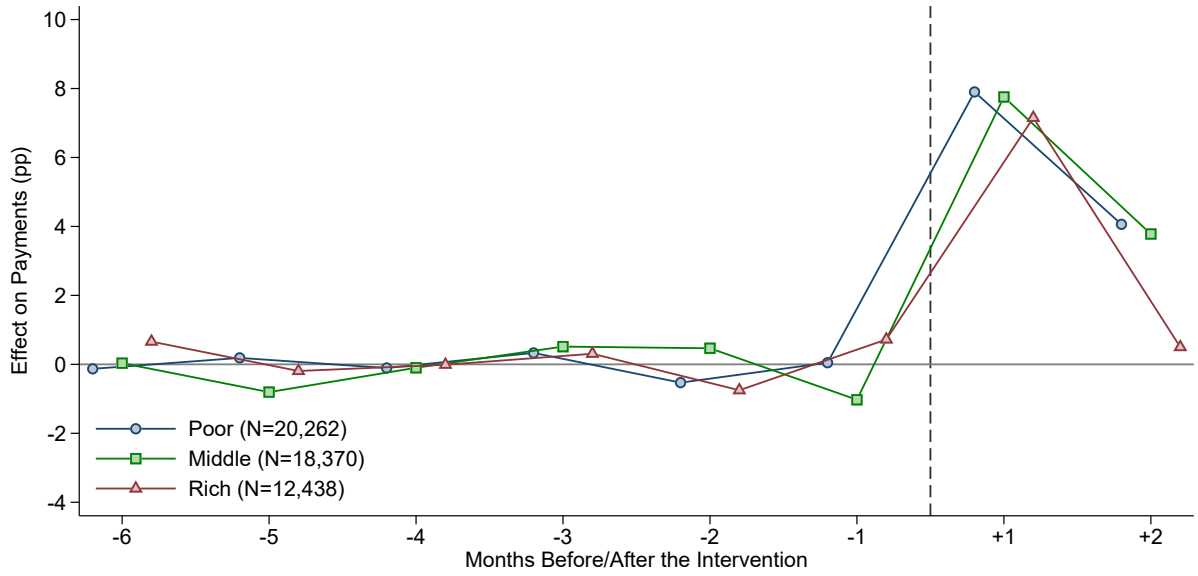
E.4 Control Letter vs No Letter

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 E.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 E.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 E.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.

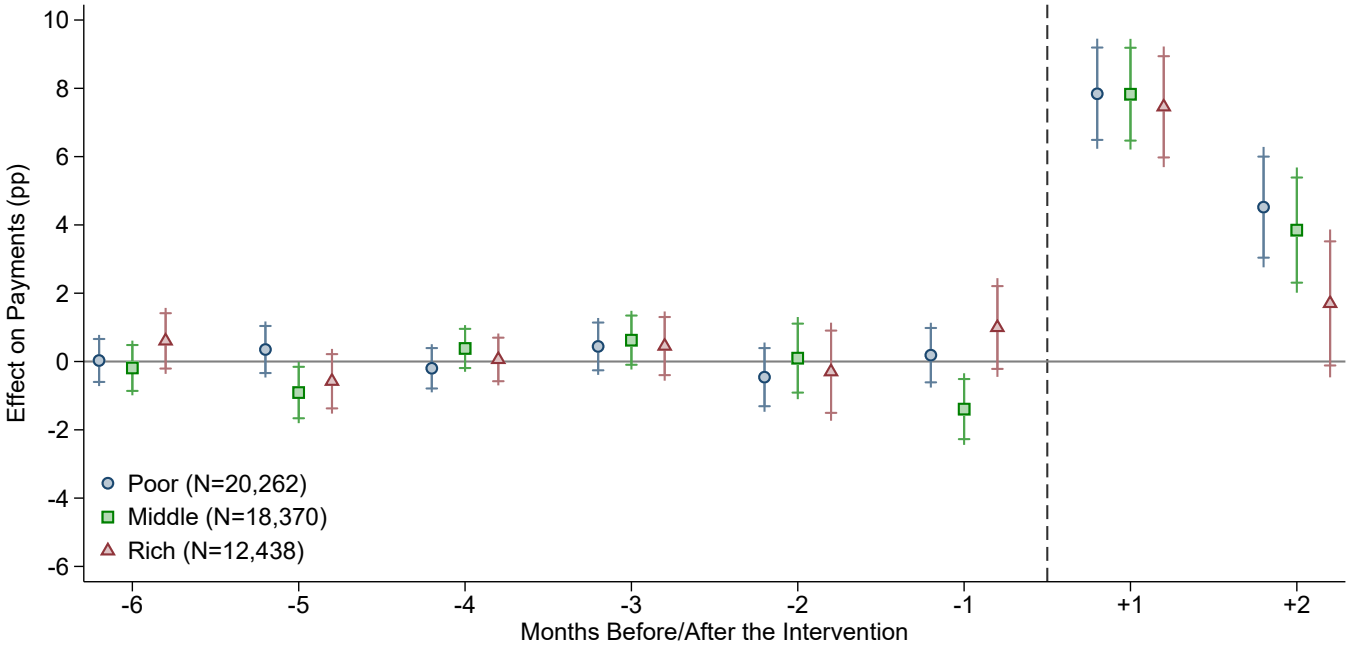
Figure E.7: Raw Difference in Payment Rates between Control and No-Letter: 2024



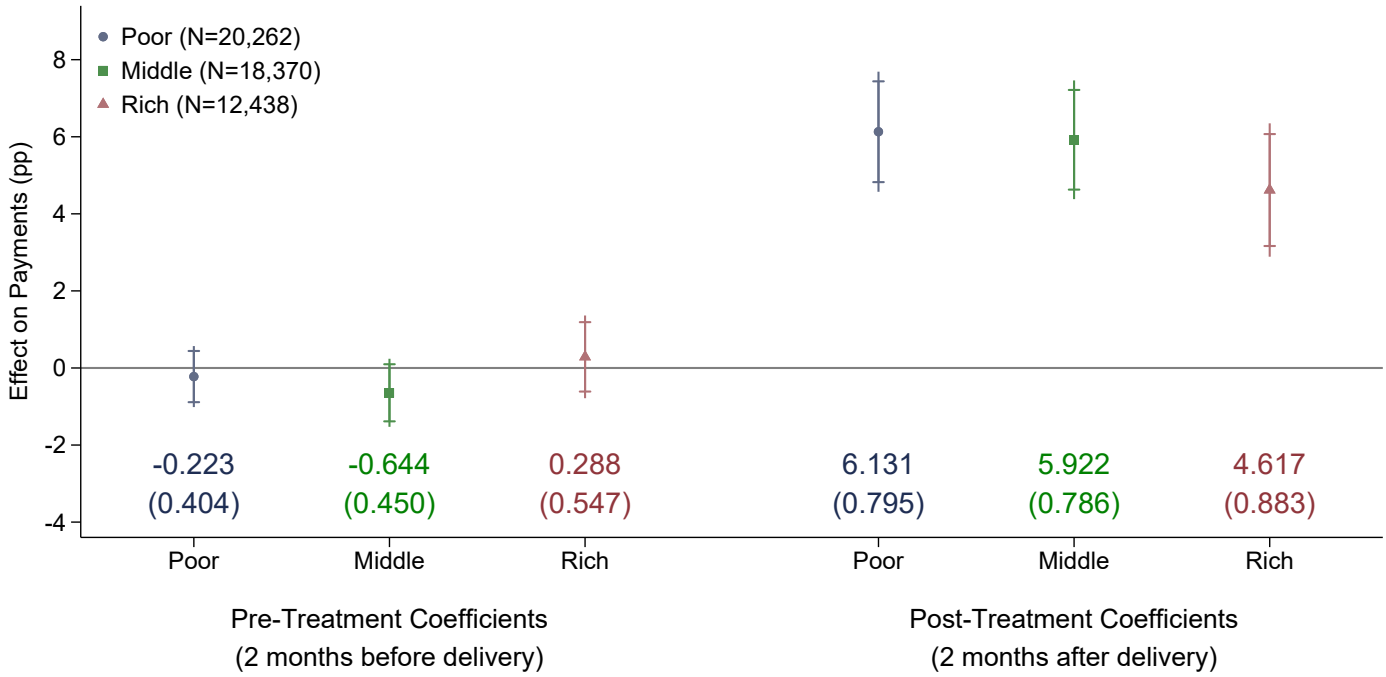
Notes: This figure compares the likelihood of paying the monthly bill between treatment and control, normalized by the average pre-treatment difference. We estimate the coefficient for each monthly bill in separate regressions. The sample includes residences and shops and excludes units that made payments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

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

(a) Dynamic Effect of the Letters



(b) Two Months Pooled Data



Notes: 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 $750K or less, 'Middle' (in green) denotes properties valued between $750K and $1.5M, and 'Rich' (in red) denotes properties valued more than $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 residences and shops 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. Standard errors clustered at the individual taxpayer level.

F Counterfactual Analysis

F.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.

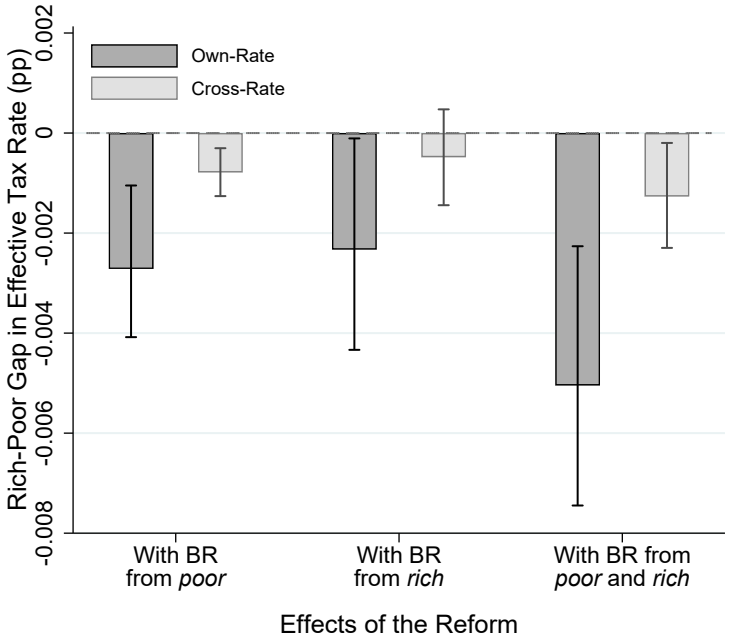
We now present the methodology used to separate the effects of changes in taxpayers' own rates (*own-rate* effects) from changes in others' rates (*cross-rate* effects) and their contribution to compliance and revenue outcomes.

Figure F.1 illustrates how these effects drive divergences in these outcomes. Panel (a) of Figure F.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.

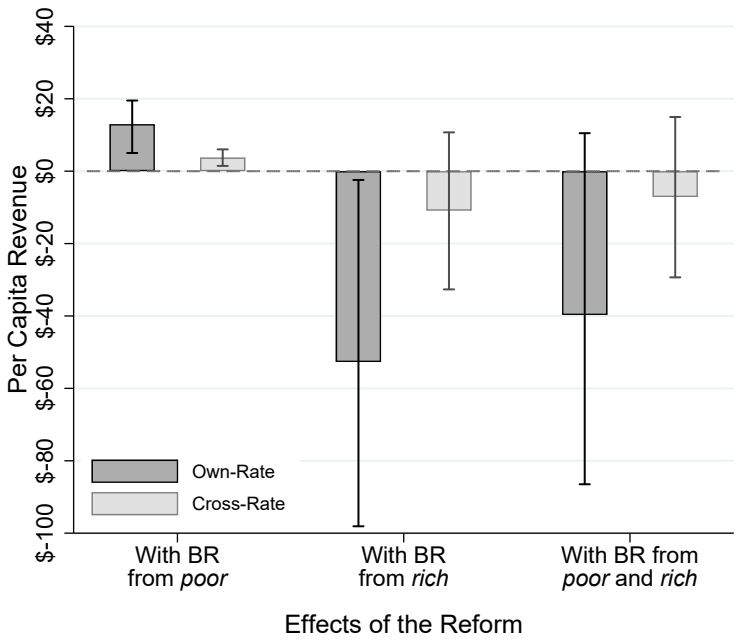
In contrast, panel (b) of Figure F.1 highlights that revenue increases for the poor group through both the *own-rate* and *cross-rate* channels. Conversely, revenue declines for the rich group through these same channels, though the *cross-rate* effect for high-value properties is not statistically significant. 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 F.1: Own-Rate vs Cross-Rate: Rich-Poor Gap and Per Capita Revenue

(a) Rich-Poor Gap: Decomposition



(b) Per Capita Revenue: Decomposition



Notes: 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 2,500 repetitions bootstrap.

F.2 Additional Details

This section explains the structure and interpretation of the detailed results presented in Table F.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., [50.1%; 56.5%]), derived from 2,500 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 F.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%	0.500
	L_j^{AP}	C_j^{AP}	T_j^{AP}
Panel (c): Post-Reform, BR from *poor*			
$j = \text{Poor}^{(i)}$	$\$7,718$	53.6%	$\$4,137$
	–	[50.1%; 56.5%]	[$\$3,870$; $\$4,361$]
$j = \text{Middle}^{(ii)}$	$\$22,159$	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%; 52.2%]	[$\$12,945$; $\$13,127$]
Rich-Poor $\text{Gap}^{[(iii)-(i)]}$	$\$48,679$	-3.3%	0.387
	–	[-6.3%; 0.1%]	[0.340; 0.443]
	L_j^{AR}	C_j^{AR}	T_j^{AR}
Panel (c): 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%	$\$25,473$
	–	[41.1%; 49.5%]	[$\$23,204$; $\$27,901$]
Weighted $\text{Sum}^{[(i)+(ii)+(iii)]}$	$\$25,915$	47.2%	$\$12,079$
	–	[46.1%; 48.3%]	[$\$11,475$; $\$12,726$]
Rich-Poor $\text{Gap}^{[(iii)-(i)]}$	$\$48,679$	-1.4%	0.373
	–	[-5.4%; 2.9%]	[0.273; 0.481]
	L_j^A	C_j^A	T_j^A
Panel (c): Post-Reform, BR from *poor* and *rich*			
$j = \text{Poor}^{(i)}$	$\$7,718$	53.6%	$\$4,137$
	–	[50.1%; 56.5%]	[$\$3,870$; $\$4,361$]
$j = \text{Middle}^{(ii)}$	$\$22,159$	49.3%	$\$10,929$
	–	–	–
$j = \text{Rich}^{(iii)}$	$\$56,397$	45.2%	$\$25,473$
	–	[41.1%; 49.5%]	[$\$23,204$; $\$27,901$]
Weighted $\text{Sum}^{[(i)+(ii)+(iii)]}$	$\$25,915$	49.8%	$\$12,280$
	–	[48.1%; 51.4%]	[$\$11,667$; $\$12,923$]
Rich-Poor $\text{Gap}^{[(iii)-(i)]}$	$\$48,679$	-8.4%	0.260
	–	[-13.2%; -2.9%]	[0.151; 0.381]

Notes: 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 individuals only. Panel (d) shows the post-reform estimates with behavioral responses for rich individuals 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 2,500 repetitions bootstrap.

G 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 Section I.2).

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 G.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)).

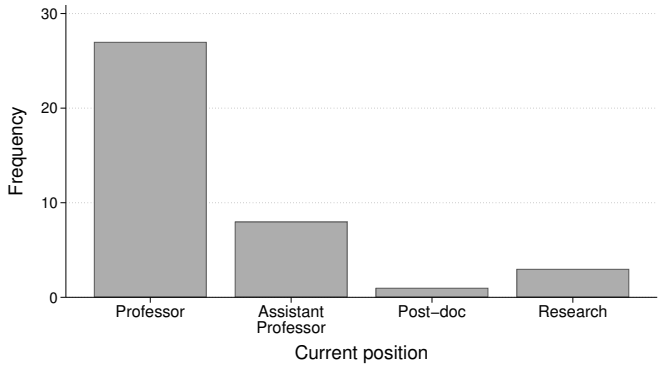
Figure G.2 shows that experts' predictions revealed significant variability across valuation groups. For lower-value properties, the majority expected a positive compliance response, attributing this to increased perceptions of fairness arising from the reform. However, for middle-value properties, predictions were more mixed, with some experts expecting no change and others anticipating increases in compliance. For higher-value 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.

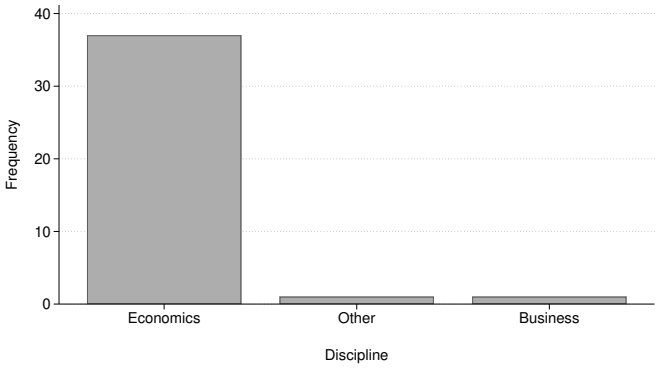
Overall, the survey results underscore the distinctive nature of the experimental findings. Although experts accurately anticipated positive compliance effects among poor properties, they significantly overestimated the strength of the treatment's impact. In contrast, the responses for medium- and high-value 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 G.1: Descriptive Statistics: Forecast Survey

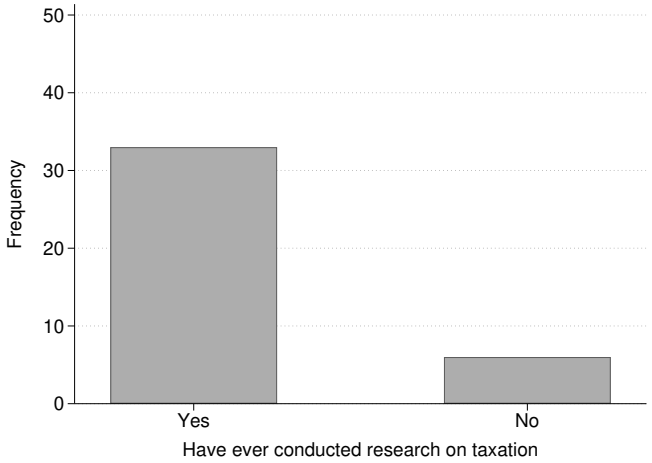
(a) Current Positions of Participants



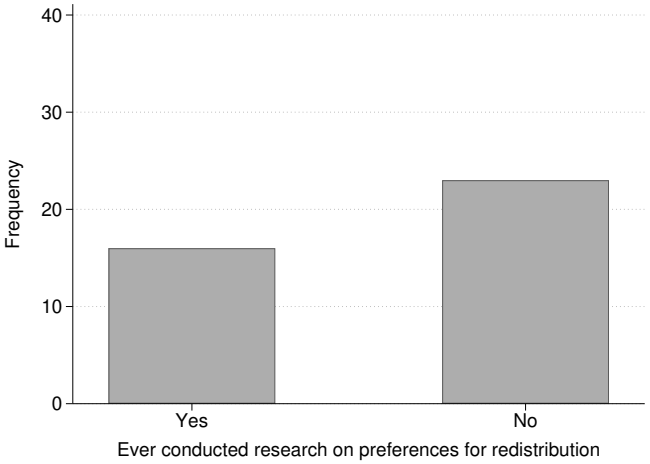
(b) Disciplines of Participants



(c) Research on Taxation



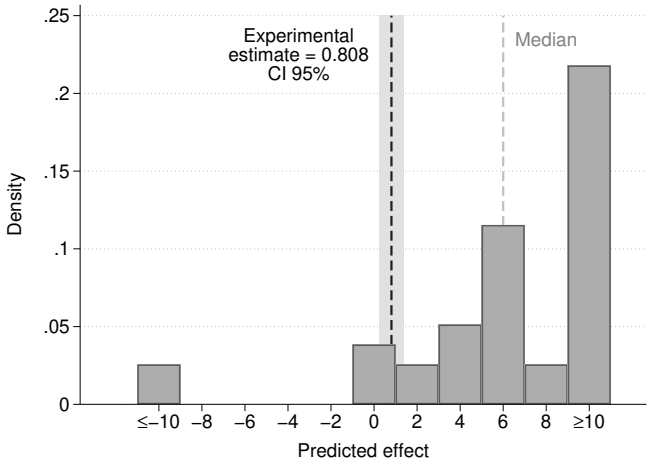
(d) Research on Redistribution



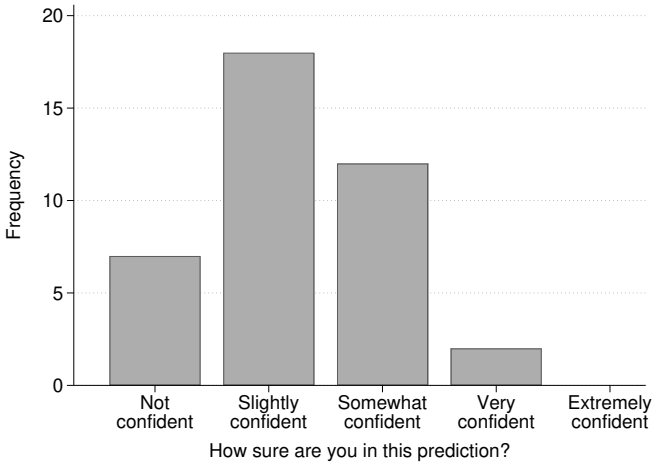
Notes: These figures summarize the descriptive statistics of the expert sample from the forecast survey. Panel (a) shows the distribution of participants' current positions, while panel (b) presents the disciplines they specialize in. Panels (c) and (d) display the proportion of participants who have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

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

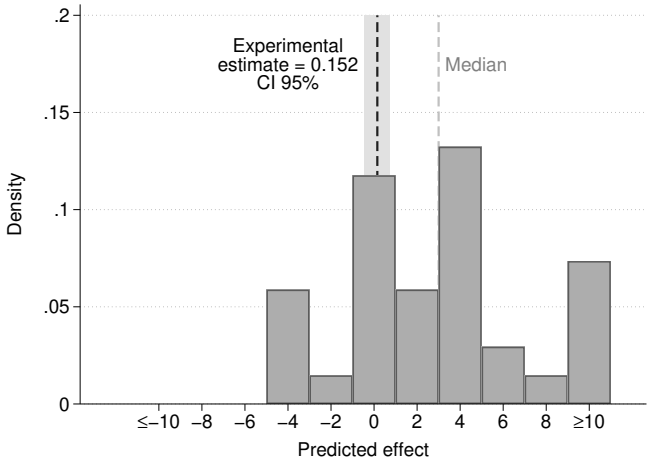
(a) Predictions - Low-Group



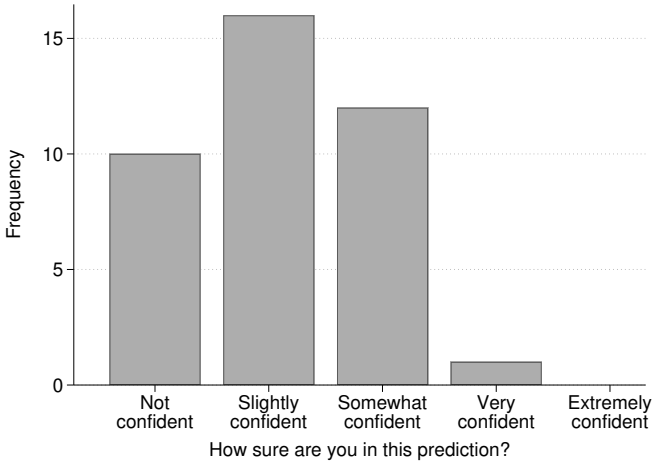
(b) Confidence - Low-Group



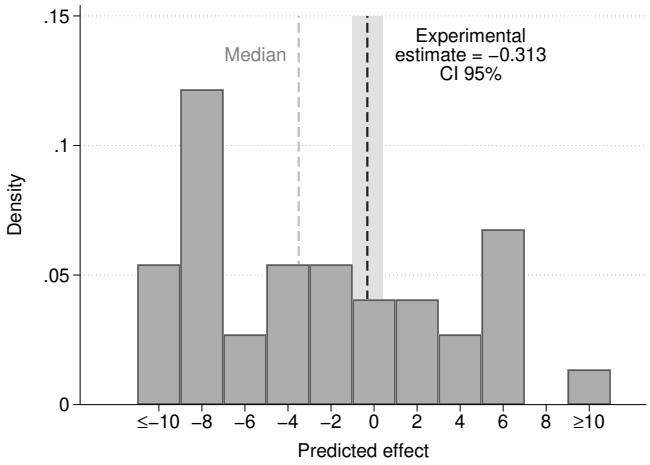
(c) Predictions - Middle-Group



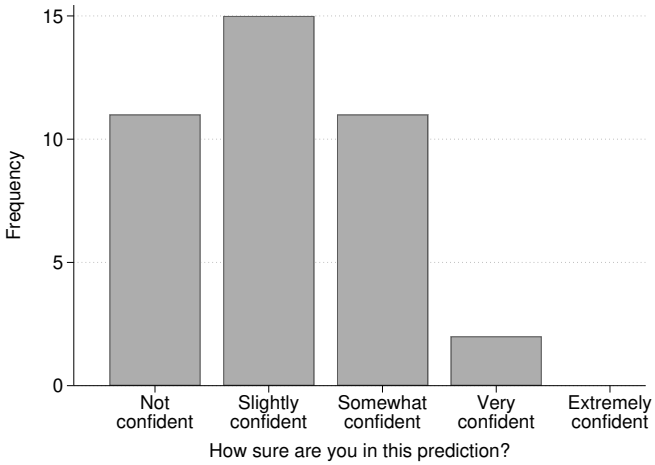
(d) Confidence - Middle-Group



(e) Predictions - High-Group



(f) Confidence - High-Group



Notes: Panels (a), (c), and (e) show the distribution of predictions for the low-, middle-, and high-valuation groups, 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.

H Sample Letters

H.1 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.

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

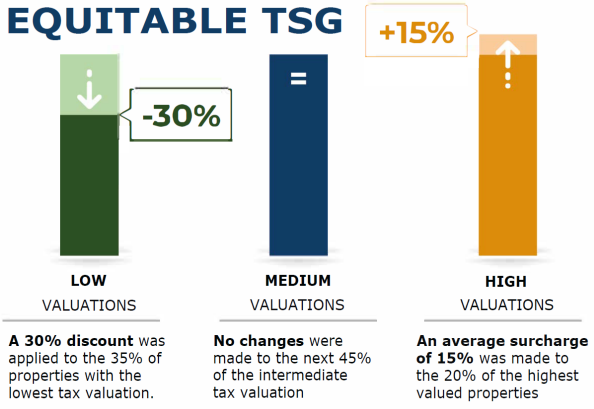
Register for one time only at **mi3f.tresdefebrero.gov.ar**

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.

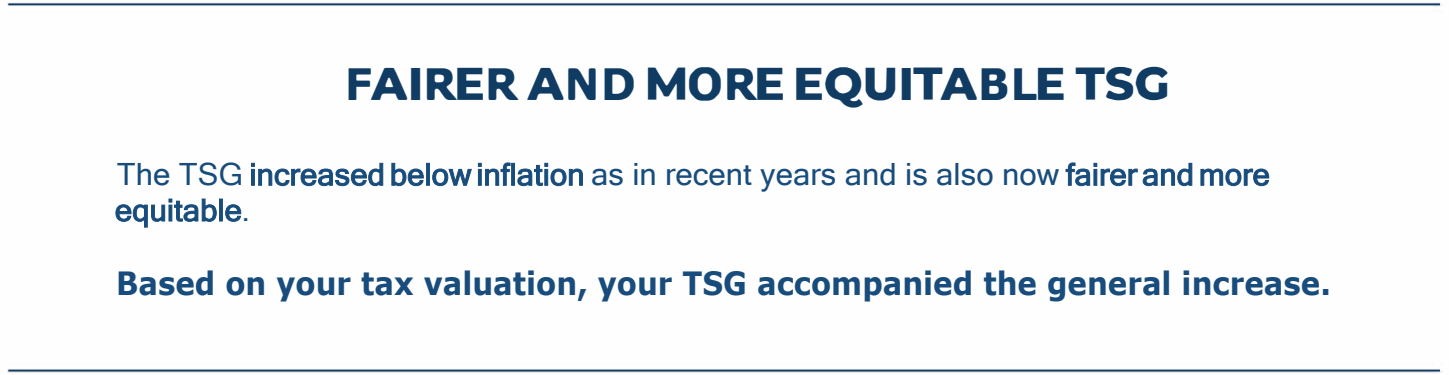


TELL US WHAT YOU THINK ABOUT OUR TAX POLICY

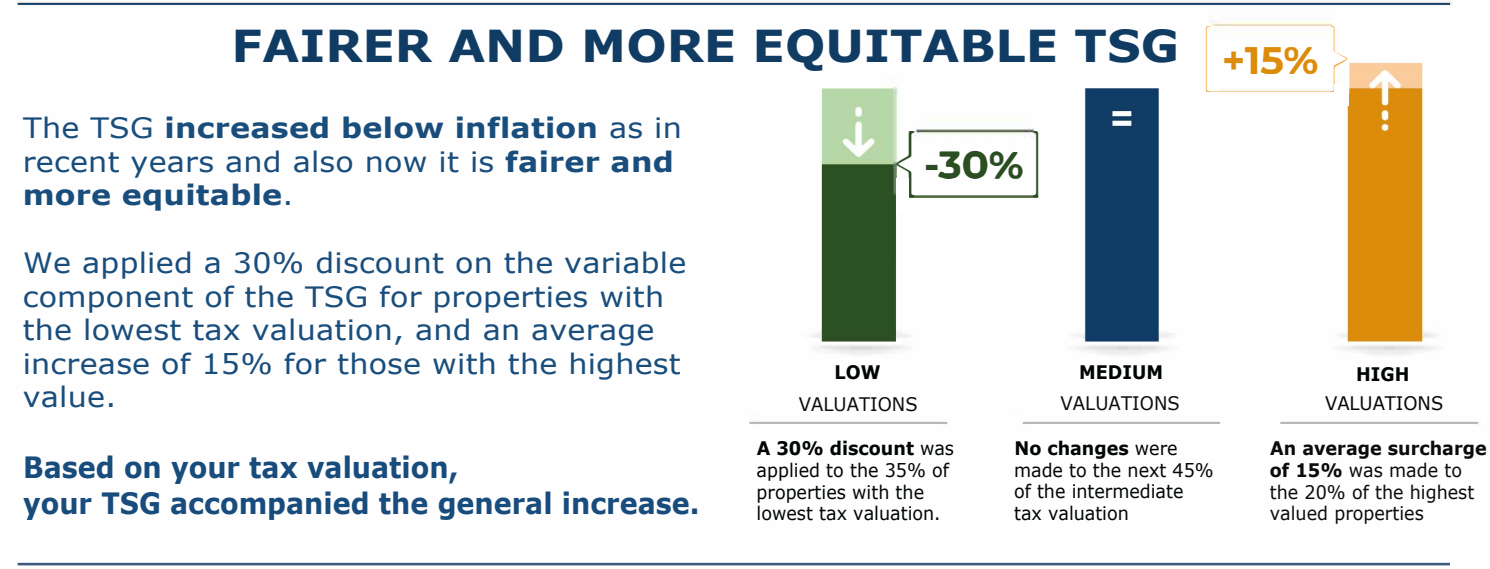
Knowing what you thought helps us improve.
Scan the QR or go to opinion.tresdefebrero.gov.ar/tasas2023

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

(a) Control Letter Message



(b) Treatment Letter Message



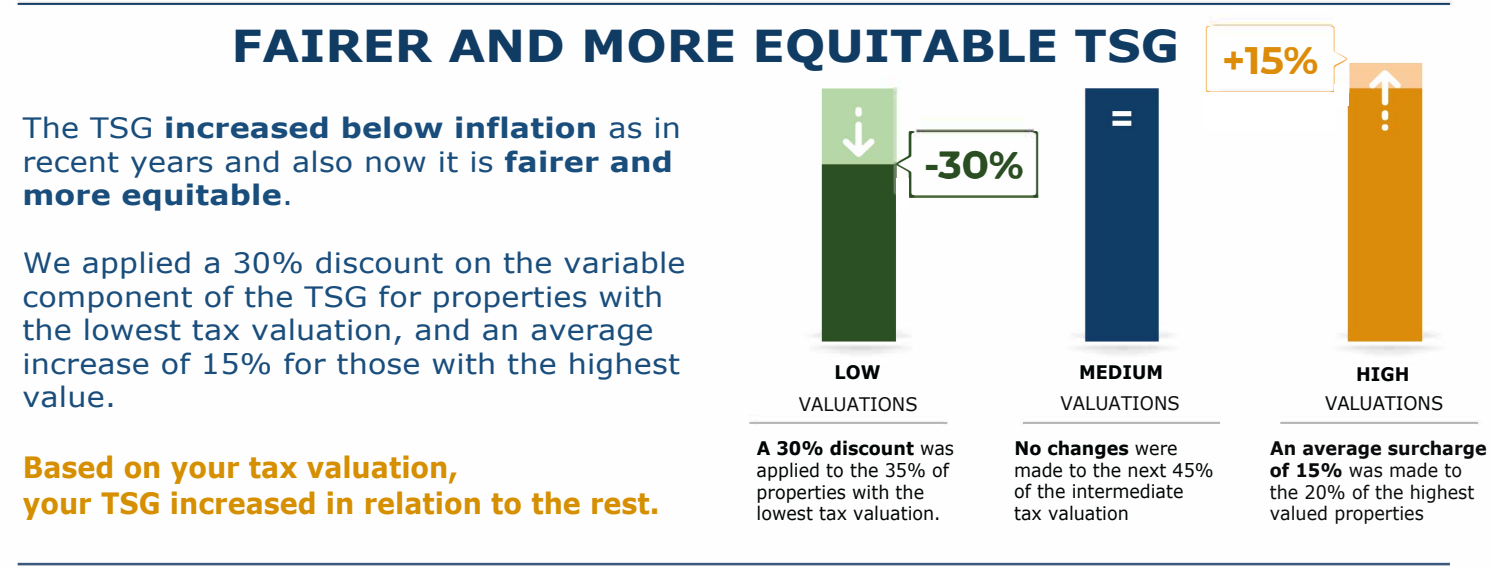
Notes: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained in the second page of the mailer.

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

(a) Control Letter Message



(b) Treatment Letter Message



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

H.2 2024 Field Experiment (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 (lower-value, middle-value, and higher-value 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**

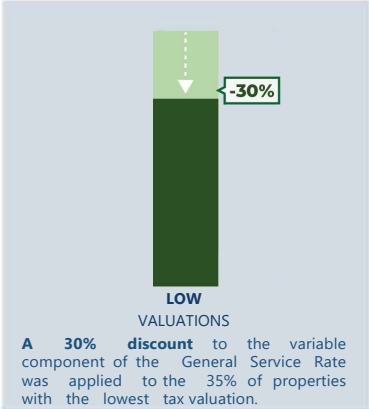
Due Date AMOUNT $

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**

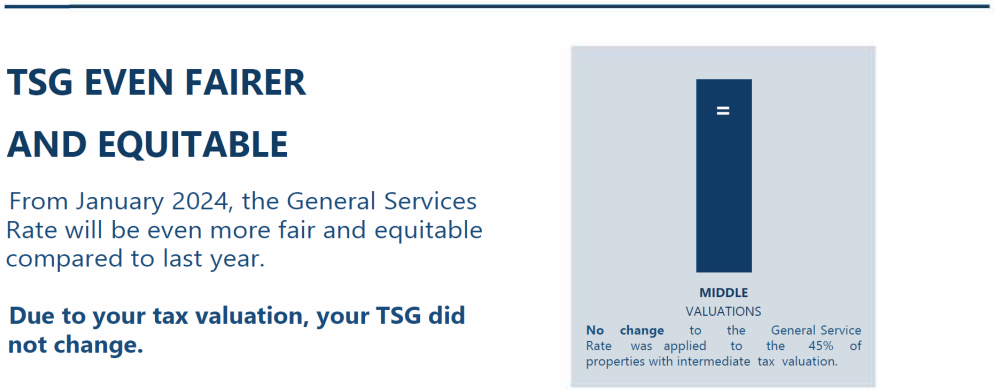
Scan the **QR** to link your **TSG** account.

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

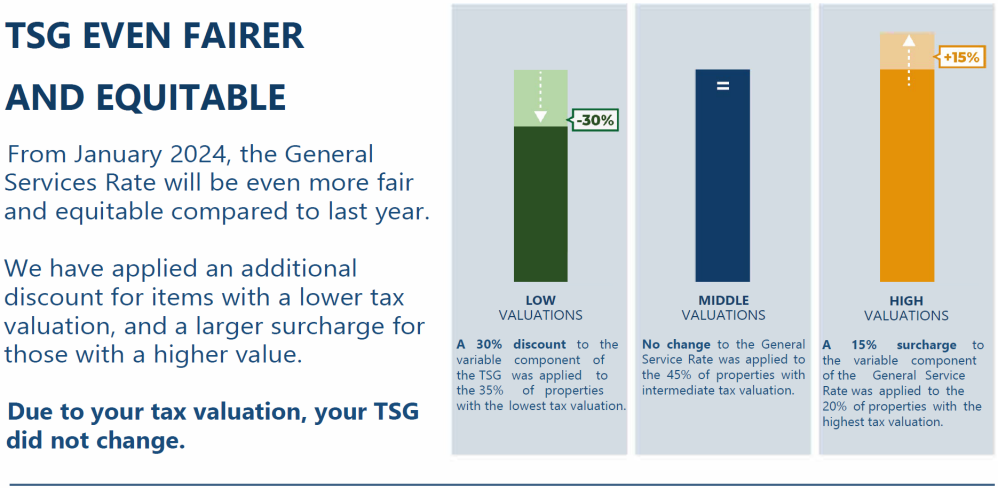


Figure H.3: Sample of Control and Treatment Messages: Middle Households – 2024 Experimental Design

(a) Control Letter Message



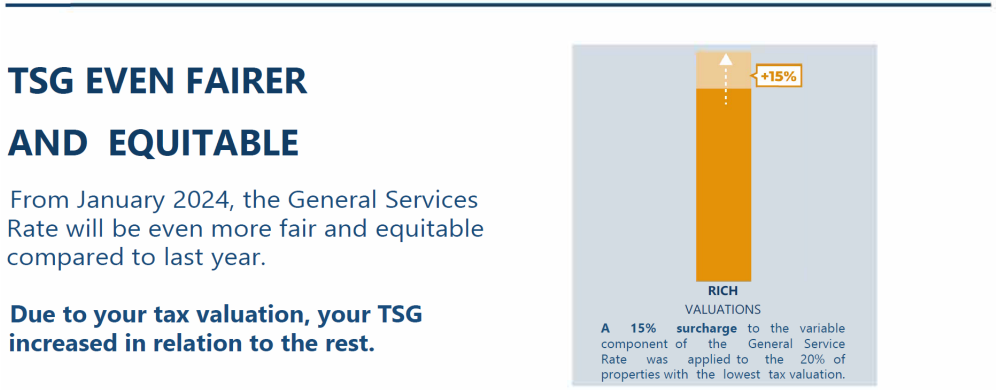
(b) Treatment Letter Message



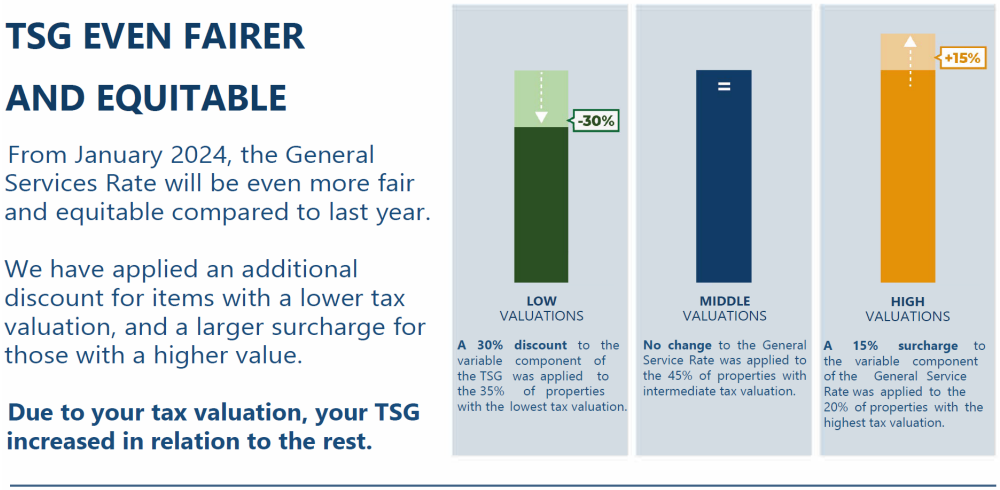
Notes: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained on the second page of the mailer.

Figure H.4: Sample of Control and Treatment Messages: Rich Households – 2024 Experimental Design

(a) Control Letter Message



(b) Treatment Letter Message



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

I Questionnaires

I.1 Taxpayer Survey (English Translation)



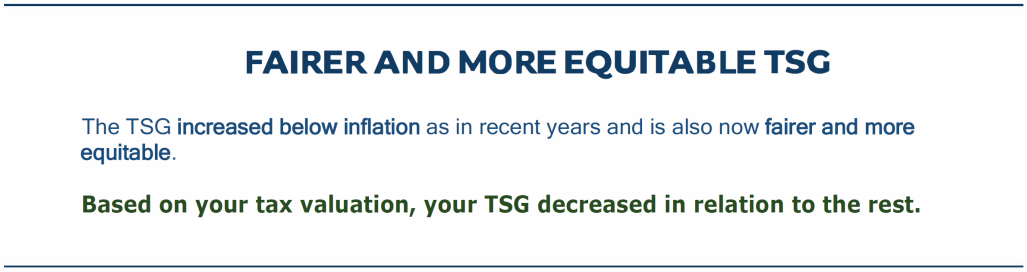
Welcome!

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 about the administration of fees and taxes of the Municipality and about some measures applied during the last year.

Page Break

Additional Information - Control Poor



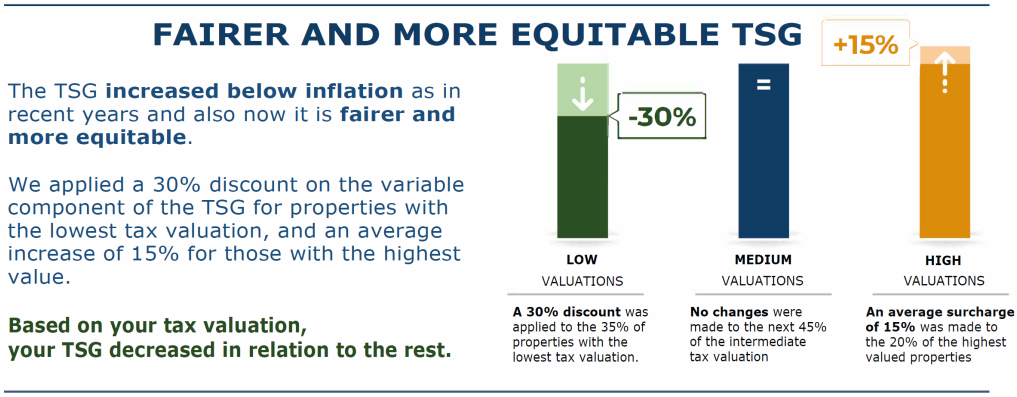
Additional Information - Control Middle



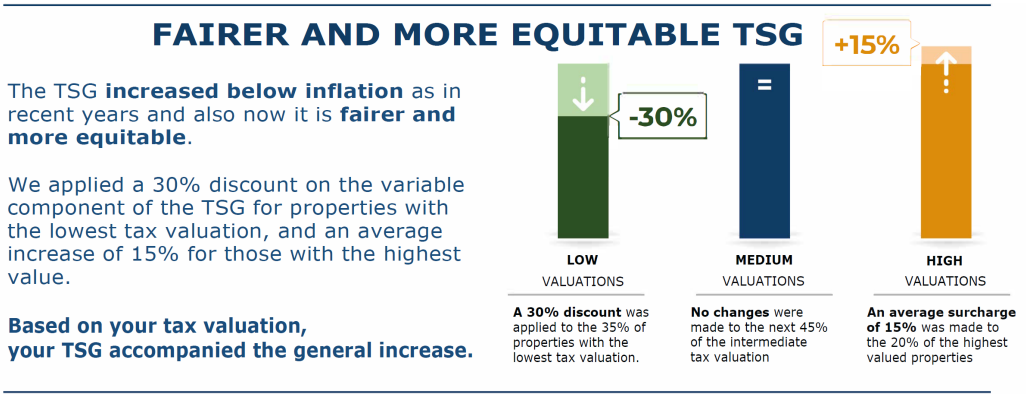
Additional Information- Control Rich



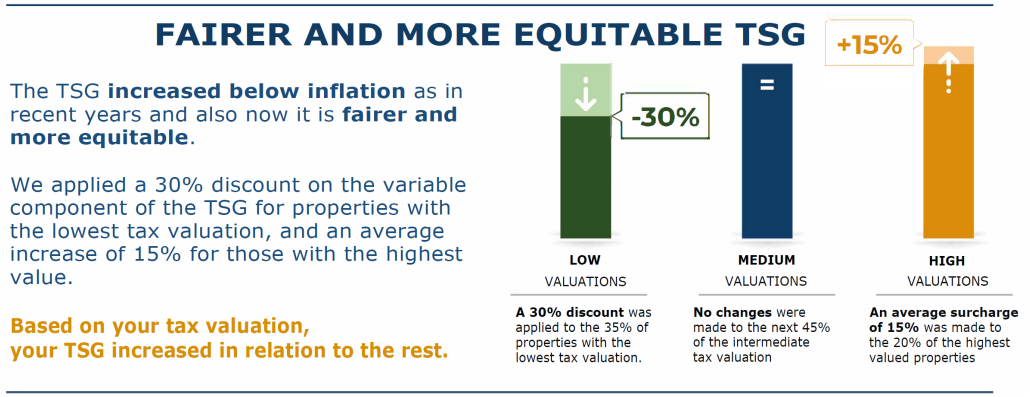
Additional Information - Treatment Poor



Additional Information - Treatment Middle



Additional Information - Treatment Rich





Are you a resident of Tres de Febrero?

- ◯ Yes
- ◯ No

Page Break

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 rates, fees, and procedures
- ☐ TSG: discounts for low-valuation properties and additional charges for high-valuation properties
- ☐ No, I haven't heard of any

Page Break

Did you benefit from any of these policies?

- ☐ Yes, my house was benefited
- ☐ Yes, my business/industry was benefited
- ☐ No, I don't see that the measures benefited me

Page Break



Are you satisfied with the payment methods offered for municipal fees and taxes?

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

Page Break

Which additional payment method would you like for paying your municipal taxes?

Page Break

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?

Should not be concerned about inequality — Should do everything to reduce inequality

0	1	2	3	4	5	6	7	8	9	10
◯	◯	◯	◯	◯	◯	◯	◯	◯	◯	◯

Page Break



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

Very unfair — Very fair

0 1 2 3 4 5 6 7 8 9 10

Page Break

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 40% 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

Page Break

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

Page Break

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

Page Break

What is your overall opinion on the municipal management and what aspects do you think could be improved?

I.2 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

→

Page Break

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.

← →

Page Break

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%.

← →

Page Break

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).

← →

Page Break

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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).

← →

Page Break

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).



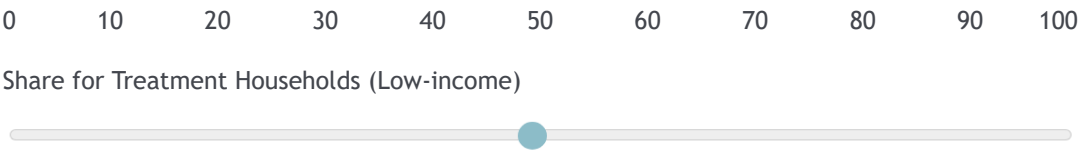
Page Break

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)

←

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Page Break

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)

← →

Page Break

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)

←

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Page Break

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

←

→

Page Break

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

←

→

Page Break

Which of the following describes your current position?

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

← →

Page Break

Please select your discipline

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

← →

Page Break

Have you ever conducted research on preferences for redistribution?

Yes

No

Have you ever conducted research on taxation?

Yes

No

← →

Page Break

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