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# FROM FLAT TO FAIR? THE EFFECTS OF A PROGRESSIVE TAX REFORM

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**ABSTRACT**

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

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A randomized controlled trials registry entry is available at AEARCTR-0010738

# 1 Introduction

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Next, we use administrative records to compare tax compliance between households with treatment and control letters. We start with the results for poor households. We show that the treatment had a positive and significant effect on their tax compliance. In other words, poor households increase their tax compliance when they learn about the progressive nature of the reform. Relative to poor households who received the control letter, the treatment caused a statistically significant (p-value = 0.006) increase in tax compliance of 0.808 percentage points (pp). We estimate a cross-rate elasticity of $\varepsilon^{cross}_{poor}$=0.098 for poor households: a 1% increase in the (perceived) taxes of the rich increases their own tax compliance by 0.098%. To put this magnitude in context, we can compare it with the effect of the own rate: a 1% increase in the tax rate of the rich has the same effect on the compliance of a poor household as lowering the poor household's own tax rate by 0.38%. Although the magnitude of this cross-rate effect is already substantial, this is just an intention-to-treat estimate, so the treatment effects on the treated could be substantially higher.[6] By construction, since the household's own taxes are held constant, the cross-rate effects cannot be attributed to price effects. Instead, our preferred interpretation is that the cross-rate effects are driven by the tax morale mechanism. Indeed, we use the survey data to provide some direct evidence in support of this hypothesis: when informed about the tax hike on the rich, poor households increase their rating of the tax system's fairness.

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

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

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

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

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

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

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

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

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

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

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

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

# 2 Institutional Context and Data

## 2.1 The Property Tax

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

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

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

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

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

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

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

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

## 2.2 Tax Compliance

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

with incomplete tax compliance records.

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

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

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

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

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

## 2.3 The Progressive Tax Reform

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

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

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

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

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

# 3 Own-Rate Effects

## 3.1 Research Design

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

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

discontinuity approach.

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

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

### 3.2 Main Results

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

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

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

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

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

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

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

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

Taken together, these placebo tests provide strong evidence that our findings are not driven by pre-existing trends or confounding factors. The absence of significant effects on the placebo date and placebo threshold tests confirms that the observed changes in tax compliance can be attributed to the progressive tax reform. Furthermore, we conducted RDD manipulation tests around the cutoffs and found no evidence of relevant discontinuities in the density of the running variable (see Appendix B.1 for more details).

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

### 3.3 Own-Rate Elasticities

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

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

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

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

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

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

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

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

### 3.4 Replication: The 2024 Reform

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

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

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

# 4 Cross-Rate Effects

## 4.1 Experimental Design

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

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

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

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

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

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

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

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

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

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

## 4.2 Complementary Taxpayer Survey

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

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

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

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

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

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

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

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

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

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

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

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

### 4.3 Experimental Results: Reform Awareness and Perceptions

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

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

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

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

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

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

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

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

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

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

### 4.4 Experimental Results: Tax Compliance

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

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

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

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

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

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

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

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

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

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

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

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

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

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

### 4.5 Cross-Rate Elasticities

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

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

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

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

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

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

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

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

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

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

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

### 4.6 Replication: The 2024 Reform

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

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

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

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

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

# 5 Counterfactual Analysis

## 5.1 Framework

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

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

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

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

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

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

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

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

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

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

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

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

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

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

## 5.2 Results

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

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

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

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

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

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

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

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

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

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

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

# 6 Conclusions

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

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

Our findings have implications for the design of progressive tax reforms. Counterfactual analysis shows that accounting for behavioral responses significantly reduces both effective progressivity and total revenue compared to projections that ignore these responses. This highlights that policymakers, particularly in settings with limited enforcement, must account for varying compliance behaviors to achieve intended distributional and revenue objectives.

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

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

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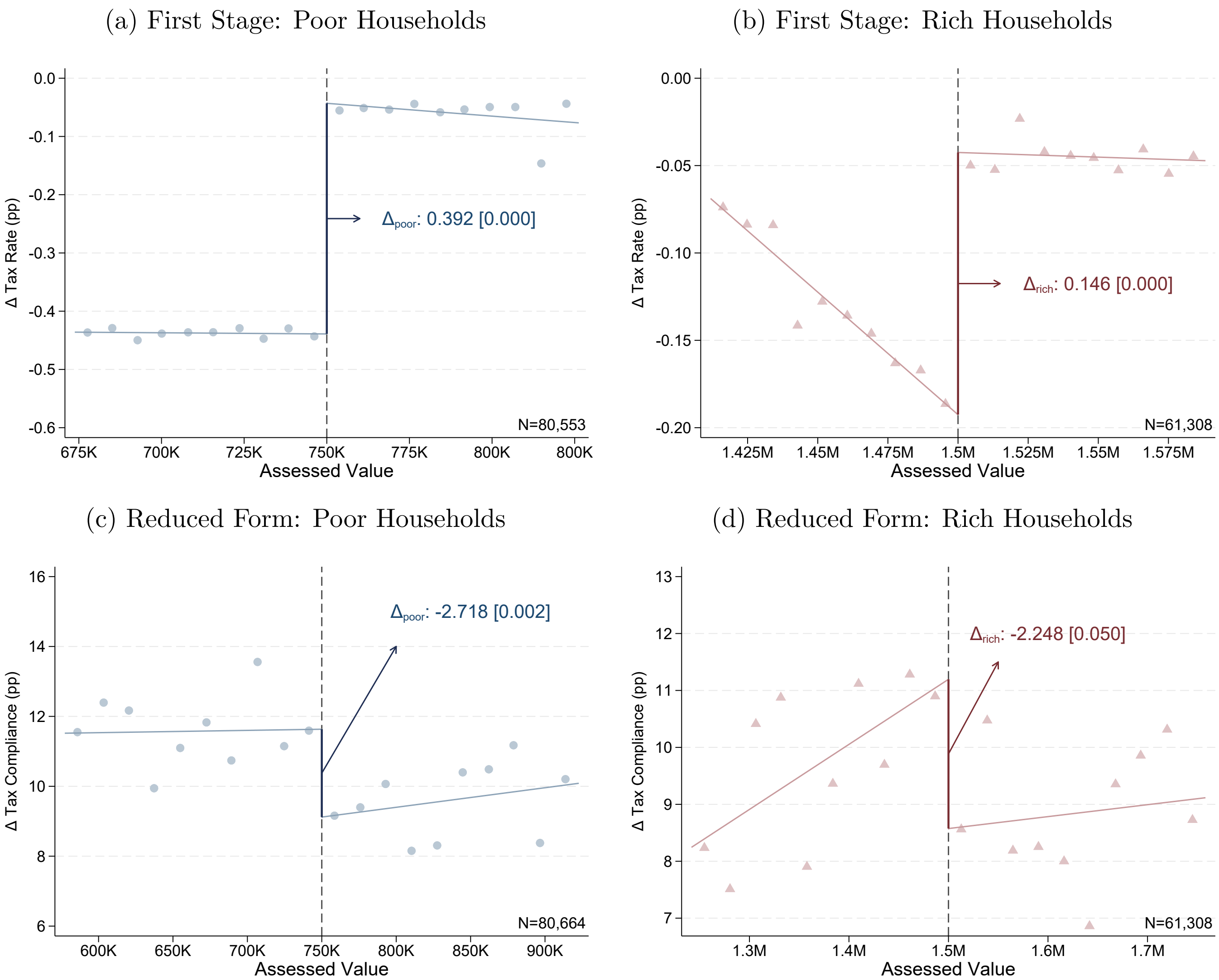
Figure 1: Own-Rate RDD Effects on Tax Compliance for 2023

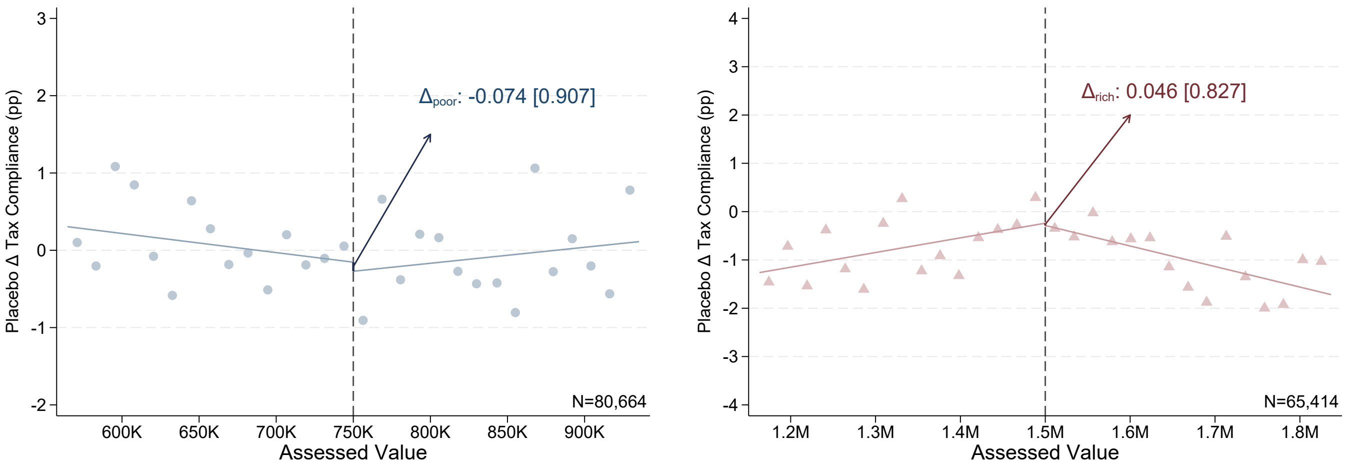



*Note*: This figure calculates the own-rate effects of the progressive property tax reform in 2023, using a two-cutoff RDD. The left figures (in blue) show the results for the poor at the cutoff of AR$ 750K. The right figures (in red) show the results for the rich at the cutoff of AR$ 1.5M. The running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

Figure 2: RDD Falsification Test using a Placebo Reform Date

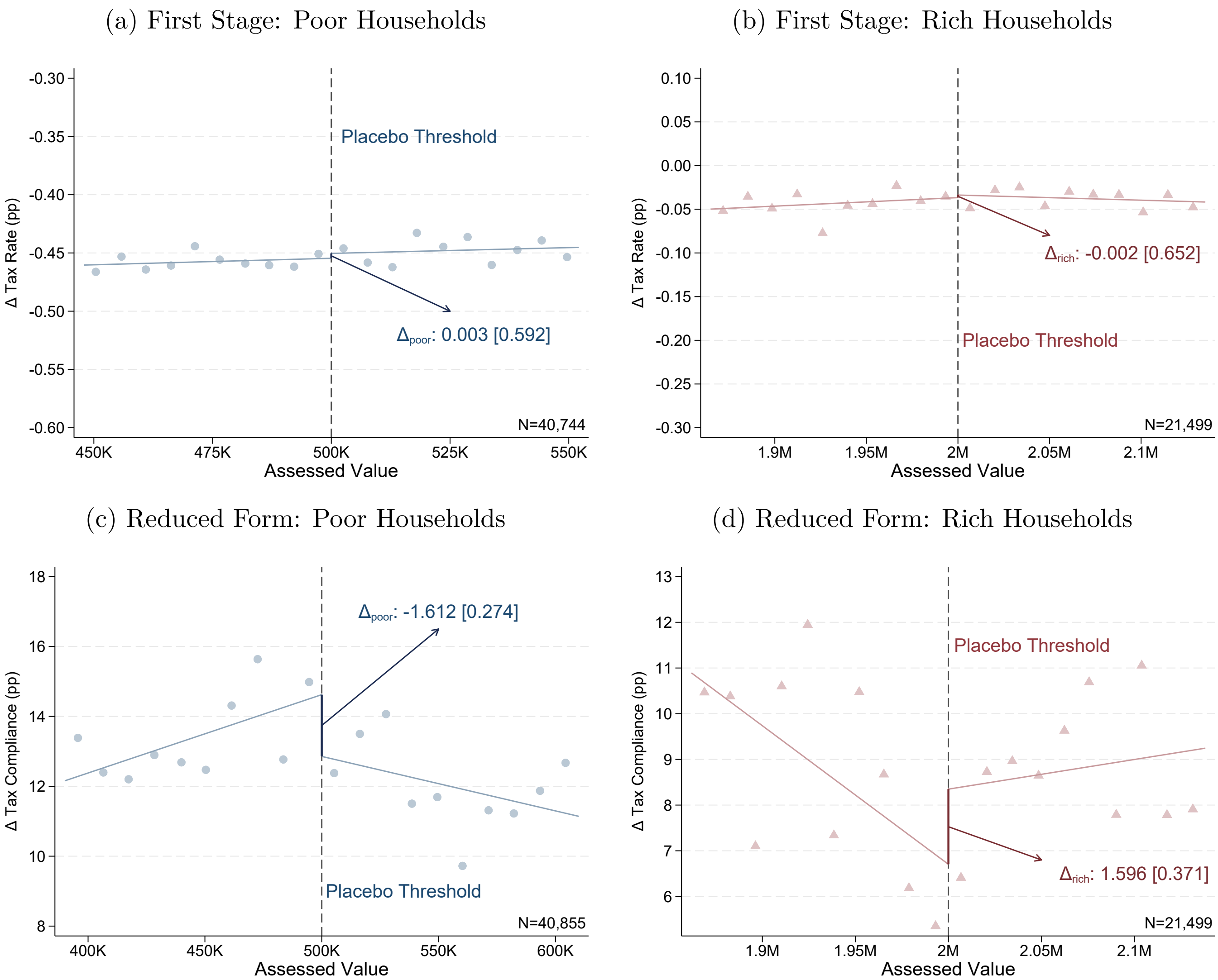
(a) Reduced Form: Poor Households

(b) Reduced Form: Rich Households




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

Figure 3: RDD Falsification Test using Placebo Thresholds for 2023




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

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

(a) Control Letter Message

## FAIRER AND MORE EQUITABLE TSG

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

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

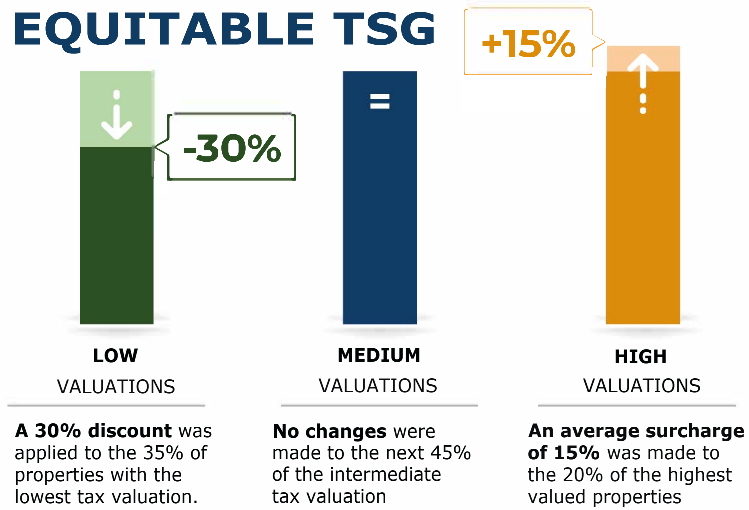
(b) Treatment Letter Message

## FAIRER AND MORE EQUITABLE TSG

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

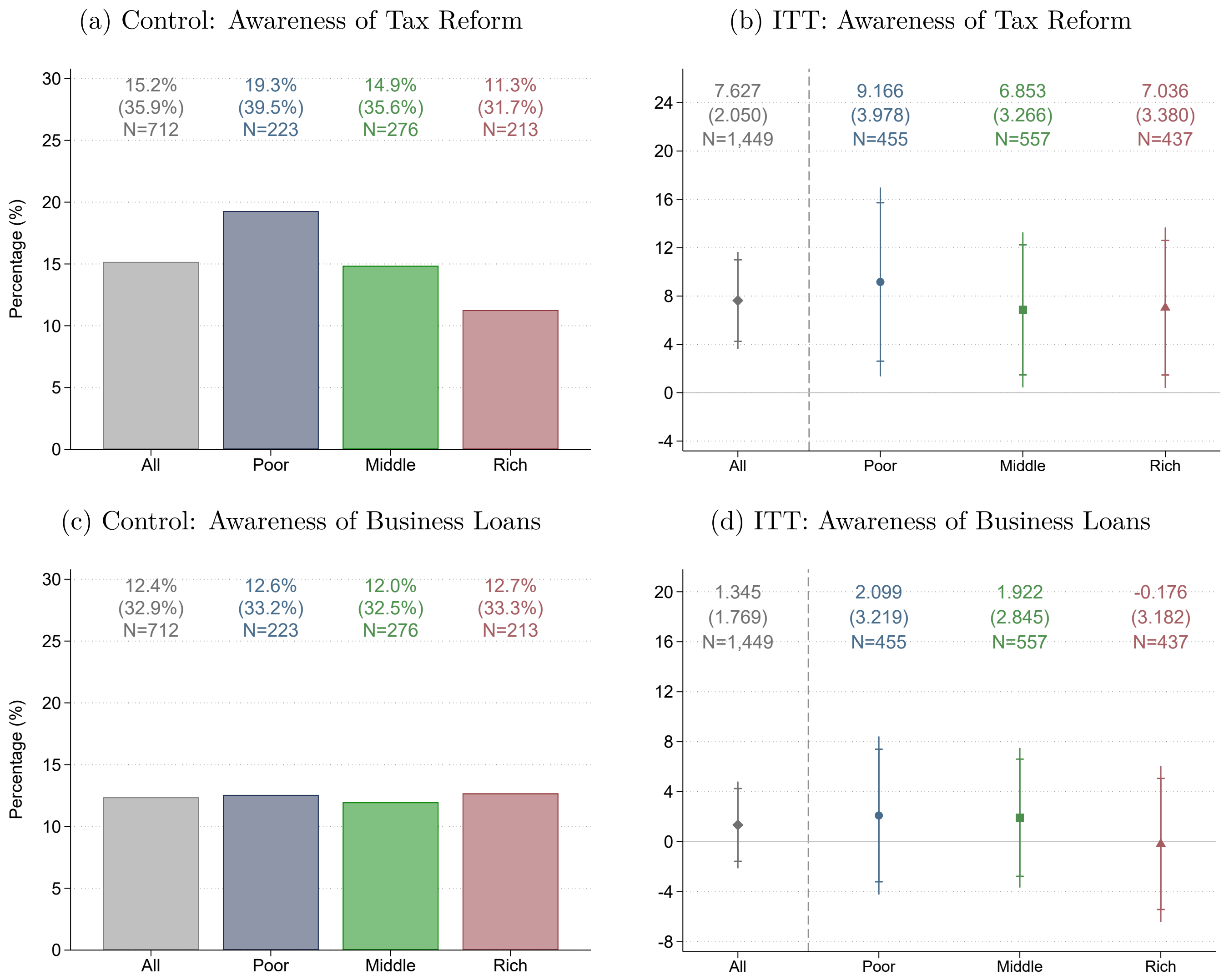
We applied a 30% discount on the variable component of the TSG for properties with the lowest tax valuation, and an average increase of 15% for those with the highest value.

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



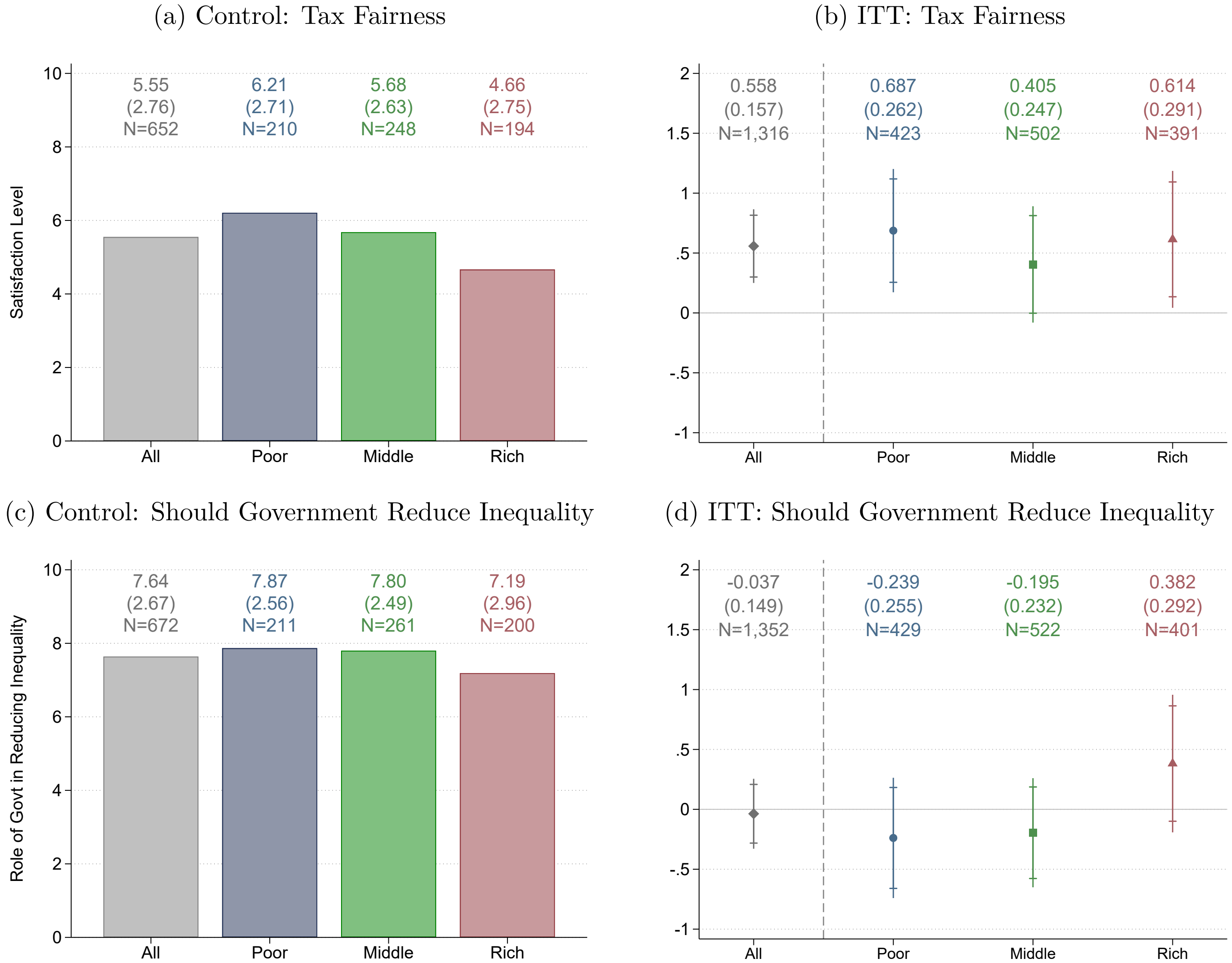

*Note*: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained on the second page of the mailer (for a full sample of the mailer, see Appendix I.1). TSG refers to *Tasa por Servicios Generales*, the local tax levied by the municipality.

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




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

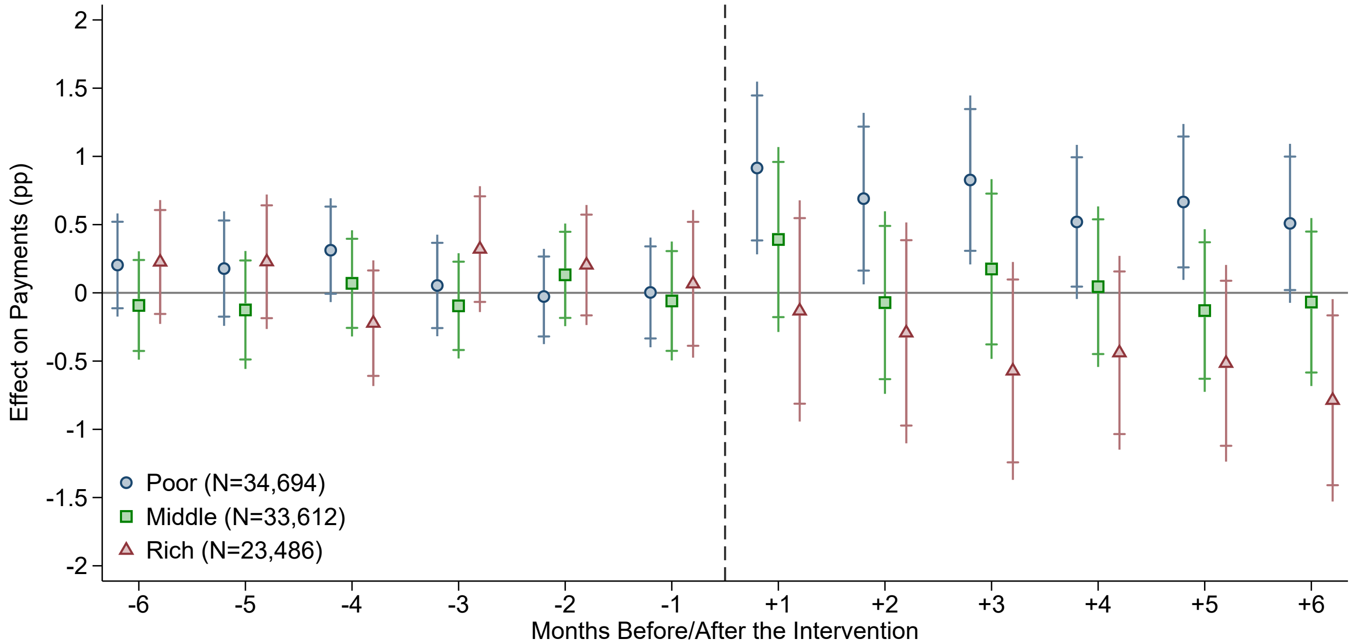
Figure 6: The Effect on Fairness Perceptions: Survey Experiment



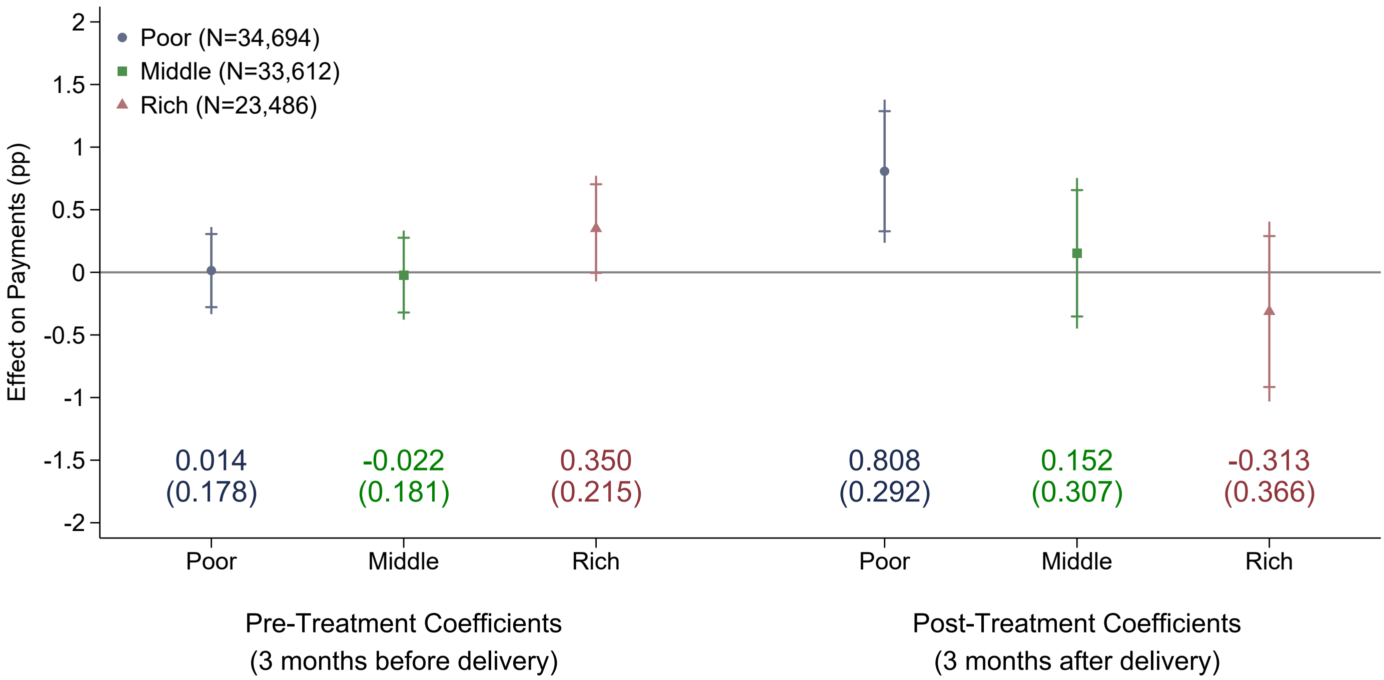

*Note*: This figure uses data from our online survey on taxpayers to estimate the impact of our progressivity information experiment on taxpayers' fairness perceptions. We asked respondents how fair they considered the distribution of the municipal tax rates between rich and poor households, on a scale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

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

(a) Event-Study Analysis (Monthly Level)




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

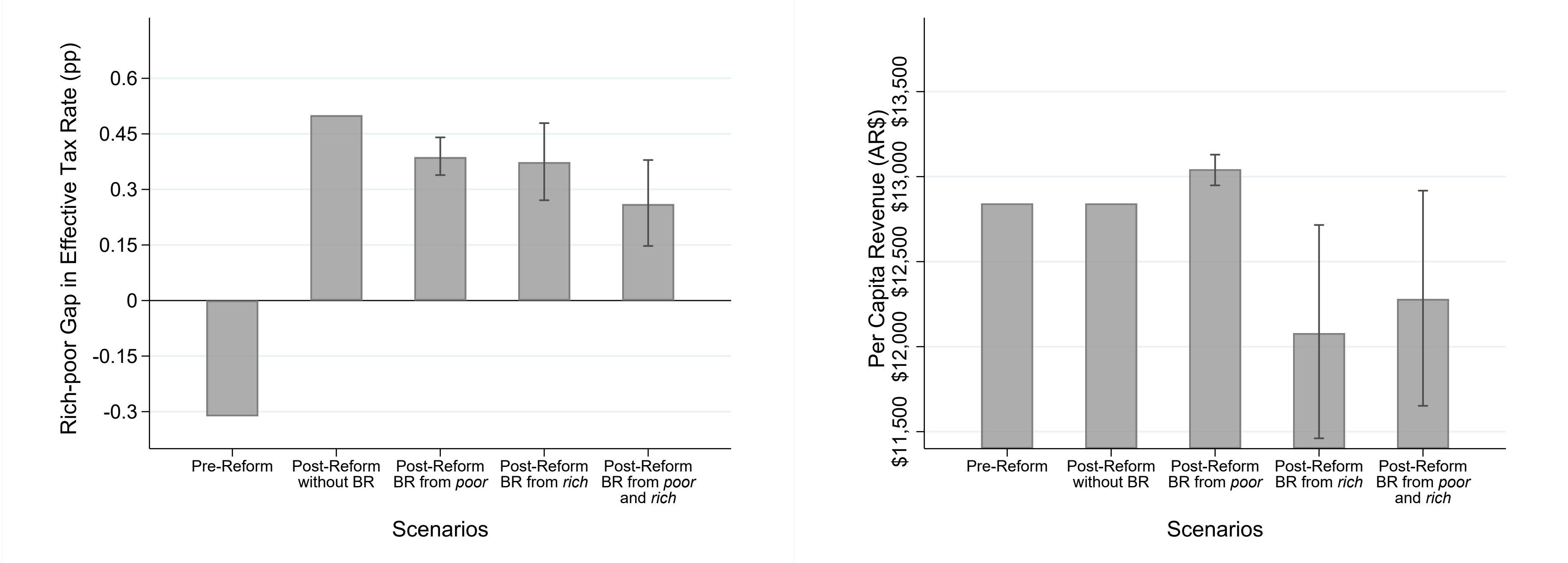



*Note*: This figure compares the likelihood of paying the monthly property tax bill between treatment and control. The control group received a letter with information solely about how the reform changed the household's own tax rate. The treatment group letter included additional information about how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

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

(a) Effective Tax Progressivity

(b) Tax Revenue (Per Capita)




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