

NBER WORKING PAPER SERIES

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

Nicolas Ajzenman
Guillermo Cruces
Ricardo Perez-Truglia
Darío Tortarolo
Gonzalo Vazquez-Bare

Working Paper 33286
<http://www.nber.org/papers/w33286>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2024, revised June 2025

We thank Diego Valenzuela, Julian Amendolaggine, Josefina Currao and Juan Luis Schiavoni for their invaluable support throughout the project, and Matias Fernandez for his help with the infographic. Ignacio Lunghi provided outstanding research assistance. Corresponding author: Ricardo Perez-Truglia (ricardo.truglia@anderson.ucla.edu). This project was reviewed and approved in advance by the Institutional Review Board at the University of Nottingham. The designs for these experiments were pre-registered in the AEA RCT Registry (RCT ID: AEARCTR-0010738). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research, nor the International Bank for Reconstruction and Development/World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2024 by Nicolas Ajzenman, Guillermo Cruces, Ricardo Perez-Truglia, Darío Tortarolo, and Gonzalo Vazquez-Bare. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

From Flat to Fair? The Effects of a Progressive Tax Reform
Nicolas Ajzenman, Guillermo Cruces, Ricardo Perez-Truglia, Darío Tortarolo, and Gonzalo Vazquez-Bare
NBER Working Paper No. 33286
December 2024, revised June 2025
JEL No. C93, D31, H24, H26, H71

ABSTRACT

We study the effects of a progressive tax reform on tax compliance, using a research design that distinguishes between two channels. First, using a quasi-experimental design, we estimate the direct effects of the reform—namely, how changes in a household’s own tax rate affect its own compliance. Second, leveraging a large-scale natural field experiment, we estimate the indirect effects: holding a household’s own tax rate constant, we examine how its compliance is influenced by changes in the tax rates of other, poorer or richer, households. We find substantial direct effects: lowering taxes for poor households increases their compliance, while raising taxes for rich households reduces theirs. We also find sizable indirect effects: when poor households learn about the tax hikes on the rich, their stated perceptions of tax fairness and their actual compliance both increase. Among rich households, learning about tax cuts for the poor also improves perceived fairness, but, if anything, reduces compliance. Using an additional reform and follow-up field experiment conducted a year later, we replicate both the quasi-experimental and experimental results. Together, our findings show that tax compliance responds not only to a household’s own tax burden but also to its perception of fairness of the broader tax system. Our results also underscore the potential disconnect between stated and revealed preferences for redistribution. Finally, we present a counterfactual analysis that illustrates the implications of the direct and indirect effects for designing progressive tax reforms.

Nicolas Ajzenman
McGill University
nicolas.ajzenman@mcgill.ca

Guillermo Cruces
Centro de Estudios Distributivos, Laborales
y Soci
gcruces@cedlas.org

Ricardo Perez-Truglia
University of California, Los Angeles
and NBER
ricardotruglia@gmail.com

Darío Tortarolo
Development Research Group (DECRG),
World Bank
dtortarolo@worldbank.org

Gonzalo Vazquez-Bare
UC Santa Barbara
Department of Economics
gvazquez@econ.ucsb.edu

A data appendix is available at <http://www.nber.org/data-appendix/w33286>
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 regarding 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 responded to a progressive tax reform.

Our novel research design allowed us to disentangle two key causal mechanisms, corresponding to the direct and indirect effects of the reform. The *own-rate* channel 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 taxes for a poor household may increase its tax compliance, while increasing taxes for a rich household may lower its tax compliance. The *cross-rate* channel posits 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 adjust its tax compliance upon learning that, in addition to lowering its own tax rate, the government has increased the tax rate on the rich. Likewise, a rich household may change its tax compliance upon learning that the government has reduced tax rates for the poor, beyond the effect of an increase in its own tax rate. Even middle households, who were not directly affected by the reform, may have adjusted their compliance in response to learning about the tax cut for the poor and the tax increase for the rich. Ex ante, cross-rate effects could be either positive or negative, depending on how different taxpayers are affected and their underlying social preferences. 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 examined a progressive property tax reform enacted by Tres de Febrero, a major municipality in the Province of Buenos Aires, Argentina, and a suburb of the capital city. The tax is levied monthly on the assessed values of properties, with an average annual-

¹For 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).

ized 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.⁵ The second advantage is that, due to limitations in tax enforcement, compliance choices provide insight into an individual’s 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%. Even in developed countries, there are contexts of low enforcement and compliance—such as the reporting of self-employment income (Cullen et al., 2021) or the church tax in Germany (Dwenger et al., 2016)—in which tax morale plays a similarly central role. Thus, there is scope for tax compliance to increase or decrease in response to the reform.

We identified the 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 used a two-cutoff cumulative regression discontinuity design (RDD) (Cattaneo et al., 2016). Moreover, we leveraged the timing of the reform as a second source of causal identification: we compared the evolution of the outcomes right before versus right after the reform took effect.

The RDD estimates revealed significant own-rate effects of the reform on tax compliance, which we can summarize as behavioral elasticities. A 1% reduction in the tax rate

²We use “households” for brevity, but the sample also includes some non-residential properties, which represent approximately 16% of the total.

³The 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.

⁴For 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).

⁵Households with more expensive homes are expected to have, on average, higher incomes as well. This is not only a core prediction of standard economic theory, but also a widely documented empirical correlation (Goodman and Kawai, 1982; Jimenez and Keare, 1984; Zheng et al., 2018; Karmali and Weng, 2022).

for the poor increased their compliance by 0.257%, implying an elasticity of $\varepsilon_{poor}^{own}=0.257$. On the other hand, a 1% increase in the tax rate for the rich reduced their compliance by 0.484%, corresponding to an elasticity of $\varepsilon_{rich}^{own}=0.484$.

These RDD estimates are robust to a host of sensitivity checks. Most notably, we provide two key falsification tests: we found 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 reproduced the RDD analysis exactly as for the first reform. Reassuringly, the results were replicated: we found elasticities that are consistent across the two reforms, both qualitatively and quantitatively.

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. Both types included information on the amounts added to or deducted from recipients bills as a result of the tax reform. We randomly assigned each subject to receive one of two types of letters. The first type included a message on how the reform changed the household’s own tax rate. The second type included the same message, but with additional information about how the reform affected the tax rates of other household groups; for example, a poor household would learn that the reform also raised taxes on the rich.

Using administrative records, we estimated 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. We received 1,723 responses, corresponding to a response rate of 11.8%. A concern in information-provision experiments is that subjects may ignore the information or that the control group may learn it through other channels. To address these concerns, the survey included a question assessing awareness of the progressive tax reform: subjects saw a list of recent policies and indicated which they had heard of. This allowed us to test whether the treatment increased awareness. Finally, to investigate the tax morale mechanism, the survey included a question on perceived fairness of the tax system, rated on a 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 found that households that received the control letter, compared to those who did not receive any letter, exhibited a significant increase in tax compliance. Survey data also indicate that households paid attention to the letter con-

tent: only a few in the control group were aware of the progressive tax reform, while awareness was significantly higher among those who received the treatment letter.

Next, we used administrative records to compare tax compliance between households with treatment and control letters. We began with the results for poor households and found that the treatment had a positive and statistically significant effect on their tax compliance. Specifically, poor households increased their compliance upon learning about the tax hike on the rich. Compared to poor households who received the control letter, the treatment led to a statistically significant increase in tax compliance of 0.808 percentage points (pp, p-value = 0.006). We estimated a cross-rate elasticity of $\varepsilon_{poor}^{cross}=0.098$ for poor households: a 1% increase in the (perceived) taxes of the rich increased the poor household’s own tax compliance by 0.098%. To contextualize this magnitude, we compare it to the effect of the household’s own tax rate: a 1% increase in the tax rate of the rich had the same effect on a poor household’s compliance as lowering the poor household’s own tax rate by 0.38%. While this intention-to-treat effect is already substantial, the true effects are likely larger, as a significant fraction of households may not have read the letters fully or carefully. This cross-rate effect must be driven by changes in tax morale. Consistent with this interpretation, survey data shows that when informed about the tax hike on the rich, poor households reported higher perceptions of the tax system’s fairness.

We now turn to the cross-rate effects for rich households. As with poor households, we found that the treatment increased rich households’ awareness of the progressive tax reform. Moreover, when the rich found out that the poor were getting a tax cut, they were 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 did not find a positive effect on tax compliance for the rich. If anything, the rich became *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. One interpretation of this finding is that, although rich households may *say* they view the tax cut for the poor as fair and claim to be supportive, they may in fact be secretly resentful. We also estimated the cross-rate effects for middle households. Although they did 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 have affected their compliance. Within this group, as with the poor and rich households, the treatment had a significant positive effect on awareness of the progressive tax reform and on perceived fairness of the tax system. However, the effect on tax compliance was close to zero and statistically insignificant.

The results of the field experiment are robust to a host of sensitivity checks. Most importantly, an event-study analysis shows that the timing of treatment effects coincides exactly with the timing of the mailing intervention. Moreover, to assess the replicability of our findings, we conducted a second pre-registered field experiment in January 2024,

leveraging the implementation of a new progressive tax reform introduced at that time. The results for the 2024 experiment were largely similar to those from the 2023 experiment. Furthermore, to assess whether these results were surprising or predictable, we conducted an expert forecast 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.⁶

We show that our findings have implications for the design of progressive tax reforms. As emphasized in the public finance literature ([Saez and Zucman, 2023](#)), policymakers should account for behavioral responses to accurately forecast the effects of tax policy. We note that policymakers should take into account both direct behavioral responses but also indirect ones derived from taxpayers' social preferences. Our counterfactual analysis evaluates a hypothetical tax reform that, absent behavioral responses, would be revenue-neutral and would increase the progressivity of the tax schedule. We adopt a sufficient statistics approach ([Chetty, 2009](#)): the counterfactual analysis depends solely on the four behavioral elasticities estimated with the quasi-experimental and experimental designs. 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. To the extent that indirect effects drive a sizable portion of these results, they underscore the importance of considering not only the direct impact of a reform but also how taxpayers perceive its broader implications for fairness.

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](#)). A growing body of research examines how property tax design can be improved in developing countries, particularly through changes in tax rates and enforcement ([Brockmeyer et al., 2023](#); [Bergeron et al., 2024](#)). There is, however, no direct evidence on the tax compliance effects of making tax schedules more progressive. Our study fills this gap.

Second, our study contributes to the broad literature on social preferences, tax preferences, and redistribution preferences. While there have been some advances in under-

⁶For more details about the design and the results of the forecast survey, see Appendix [K](#).

standing perceptions and support for tax progressivity, they are all based on survey data. Using survey experiments, [Tarroux \(2019\)](#) found that people seem to attach intrinsic value to tax progressivity. [Stantcheva \(2021\)](#) found that educational videos on redistribution increase stated support for progressive taxes, while efficiency-focused videos have no effect. Similarly, [Hoy \(2025\)](#) found that self-reported tax morale rises when individuals are informed about the progressive structure of their tax systems.⁷ Although valuable, these studies rely exclusively on stated preferences, which may be subject to social desirability and other biases ([Bursztyn et al., 2025](#)). For example, rich households may *say* they are willing to pay more in 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. Indeed, we found significant discrepancies between stated and revealed preferences: while middle and rich households reported higher perceived fairness after learning about the progressive reform, this did not translate into increased compliance.

Our evidence on the own-rate effects of rich households relates to the literature examining the tax evasion responses to wealth taxes. Several papers document how high-income or high-wealth individuals react to increased tax burdens, often using registry data and policy changes. For example, [Seim \(2017\)](#), [Brülhart et al. \(2022\)](#), and [Rauh and Shyu \(2024\)](#) study responses to wealth and income taxes in Sweden, Switzerland, and California, respectively, while [Locks \(2023\)](#) and [Londoño-Vélez and Ávila-Mahecha \(2024\)](#) provide related evidence for Brazil and Colombia. These studies show that high-wealth individuals often respond to increases in their own tax rates by reducing their reported tax bases or shifting assets across borders. Our findings extend this literature by estimating compliance effects not only for the rich but also for the poor, and by separately identifying both direct and indirect effects. Beyond estimating a broader set of effects, we do so within a unified framework that allows for meaningful quantitative comparisons across groups (poor, middle, and rich) and channels (direct and indirect).

Lastly, our findings on the cross-rate effects of a progressive reform contribute to the growing body of evidence on tax morale—for a comprehensive literature review, see [Luttmer and Singhal \(2014\)](#) and [Slemrod \(2019\)](#). To mention a few examples, [Dwenger et al. \(2016\)](#) study the role of intrinsic versus extrinsic motivations. [Cullen et al. \(2021\)](#) show that tax compliance is higher when taxpayers trust the government in power. [Giacobasso et al. \(2022\)](#) and [Kresch et al. \(2023\)](#) show that tax compliance increases when taxpayers believe they benefit from government spending. [Besley et al. \(2023\)](#) provides evidence that the United Kingdom’s regressive poll tax increased tax evasion. And [Del Car-](#)

⁷Other related studies are [Ballard-Rosa et al. \(2017\)](#) and [Kerschbamer and Müller \(2020\)](#).

⁸For more discussion of the generalizability of data on social preferences, see [Epper et al. \(2024\)](#) and [Fehr et al. \(2024\)](#). While too numerous to list exhaustively, other studies explore redistributive preferences and fairness norms (e.g., [Tyran and Sausgruber, 2006](#); [Kuziemko et al., 2015](#)).

pio (2014), Nathan et al. (2024) and Best et al. (2025) discuss the role of fairness and inequity in tax compliance. We contribute to this literature by providing novel evidence of tax morale and by identifying a new tax morale channel—preferences for progressivity.

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 non-residential properties.⁹ Although our sample includes some non-residential properties, the vast majority (84%) are residential, so we refer to taxpayers as “households” for brevity. 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.¹⁰ For our analysis, we have access to monthly administrative data from the municipality’s tax records.¹¹

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 liability was AR\$ 32,561.1 per year in current Argentine pesos, equivalent to 529 U.S. dollars.¹² 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 (for details, see Appendix B), 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 0.90%, 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,

⁹Property owners are legally responsible for paying the property tax.

¹⁰In 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).

¹¹We excluded from the analysis 22,935 properties without property value assessments and 142 households with incomplete tax compliance records.

¹²This conversion is based on the USD PPP exchange rate of AR\$ 61.5 for 2022 (World Bank, 2025). Throughout the remainder of the document, unless explicitly stated otherwise, all monetary amounts are expressed in current Argentine pesos.

2024).

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.¹³ 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

In Tres de Febrero, property taxes are billed monthly, with each installment due on the 15th of each month.¹⁴ Our primary outcome of interest is a binary variable that indicates whether a household made a tax payment within three months of the due date. While the three-month threshold is somewhat arbitrary, we adopt it to align with definitions used in related studies (Del Carpio, 2014; Castro and Scartascini, 2015; Carrillo et al., 2021; Cruces et al., 2025; Flores et al., 2025). Importantly, our results are robust to alternative definitions. Intuitively, delinquency tends to be persistent: when a payment is not made within the first few months, it is likely to remain unpaid for an extended period—often years.

Compliance in the pre-reform year (2022) was far from perfect, with only 47.9% of monthly bills paid on time. Such low compliance rates are common in developing countries, where tax agencies often face significant challenges in enforcing property taxes due to limitations in human, technical, and legal resources. For example, as in other developed and developing countries, delinquent taxes are subject to financial penalties in the form of interest charges (Perez-Truglia and Troiano, 2018). However, as in those contexts, simply imposing an interest rate is not a magical solution—the same limitations that hinder enforcement of the principal also apply to the interest. The weaker enforcement capacity in developing countries results in higher tax delinquency compared to developed economies. For example, the 40 largest U.S. cities collect nearly 95% of property taxes

¹³Although the tax reform applied uniformly across property types, the tax structure differs between residential and non-residential properties—see Appendix A.1 for more details.

¹⁴A 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-compliant households contribute at least partially to the tax, since nearly all households pay their electricity bills likely due to the fact that the utility company can cut off power to those who do not. Although households can technically request the removal of this charge from their electricity bill, only a small fraction choose to do so. In 2023, 98% of the households paid at least part of their property tax through this mechanism, highlighting its high compliance rate.

on time (Chirico et al., 2016); in contrast, studies from Argentina, Brazil, Ghana, Haiti, Liberia, Malawi, Mexico, Pakistan and Peru report that less than 50% of households pay their property taxes on time (Giaccobasso et al., 2022).¹⁵ Even in developed countries, there are contexts of low enforcement and compliance—such as self-employment income reporting in the United States (Cullen et al., 2021) or the church local tax in Germany (Dwenger et al., 2016).

The average compliance rate of 47.9% reported above masks substantial heterogeneity between households. 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. As a result, there is considerable room for households to adjust their compliance either upward or downward. And given the interest in tax progressivity, it is relevant to examine differences in tax compliance across the income distribution. In 2022, the compliance rate was relatively similar for the three groups: 45.8% for poor households, 48.7% for middle-income households, and 49.8% for rich households. This pattern of persistent non-compliance across income levels suggests that tax compliance decisions are driven by more than just income or wealth effects and may instead reflect other factors, such as tax morale.

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, 2023.¹⁶ Our taxpayer survey indicates that, at baseline, most respondents, even among the rich, *stated* that they supported a tax reform that would lower taxes for the poor and raise them for the rich (see Appendix C for more details). This suggests a relatively favorable context for a progressive reform, assuming respondents answer such survey questions honestly.

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%

¹⁵See Khan et al. (2015); Kapon et al. (2024); Okunogbe (2021); Brockmeyer et al. (2023); Dzansi et al. (2022); Manwaring and Regan (2024).

¹⁶Source: [link](#). Likewise, the 2024 reform was also approved shortly before it became effective.

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 assess whether our findings replicate.¹⁷ 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 discuss how the changes to taxpayers' own tax rates affected their own tax compliance. We estimated these own-rate effects by combining two sources of identification. First, we took advantage of the sharp discontinuities in tax rates induced by the reform as a function of the property valuation brackets. Second, we exploited the timing of the reform by comparing the outcomes before and after its implementation.¹⁸

The treatment assignment was based on a two-cutoff cumulative design (Cattaneo et al., 2016), where the running variable is the property value in 2021 (V) and the treat-

¹⁷An 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.

¹⁸Since we leveraged threshold discontinuities and temporal changes around the implementation date, our research design resembles a difference-in-discontinuity approach.

ment is the tax rate, τ , which is a function of V , $\tau = \tau(V)$. Specifically, households below the low cutoff ($c_L = \text{AR\$ } 750\text{K}$), households between the two cutoffs and households above the high cutoff ($c_H = \text{AR\$ } 1.5\text{M}$) experience different tax schedules. Following Cattaneo et al. (2023), we analyzed these two cutoffs separately, comparing observations below c_L to observations between c_L and c_H around cutoff c_L , and observations above c_H to observations between c_L and c_H around cutoff c_H . Notice that the treatment $\tau(V)$ is piecewise continuous, with discrete jumps at each cutoff, a setting analyzed by Dong et al. (2023) for the single-cutoff case.

We begin by assessing whether there was a first-stage effect—that is, a sharp change in tax rates around the thresholds. For this analysis, the relevant outcome is the change in tax rates from before to after the reform.¹⁹ In the subsequent reduced-form analysis, we examine how compliance rates changed around those thresholds. More precisely, the outcome of interest is defined as the change in the proportion of bills paid on time (i.e., within three months of the due date) between the six months before and the six months after the reform.^{20,21} We estimated 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

Figure 1 presents our main own-rate results. Panels (a) and (c) focus on poor households. Panel (a) presents 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 corresponds to the change in tax rates after the reform. As expected, there is a sharp discontinuity at the AR\$ 750K threshold: poor 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

¹⁹More specifically, we defined the tax rate as the annual tax liability divided by the property valuation.

²⁰For 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 $0.167 (= \frac{5}{6} - \frac{4}{6})$.

²¹For 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 was 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.

the change in tax compliance. The discontinuity at the threshold indicates that the tax cut for the poor (i.e., left of the threshold) increased compliance by 2.718 pp (p-value = 0.002). This suggests 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) of Figure 1 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 reduced their 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.

Our preferred interpretation of the own-rate effects is that they reflect a combination of a price effect and, potentially, a tax morale effect. The price effect captures the impact of changes in tax obligations (e.g., see Brockmeyer et al., 2023; Bergeron et al., 2024). For poor households, lower tax burdens make payment more affordable and thus more likely. For rich households, higher obligations may reduce affordability and, in turn, compliance. At the same time, tax morale may also contribute. For instance, poor households facing a tax cut may increase compliance out of a sense of reciprocity or due to a perceived improvement in tax fairness. Conversely, rich households may reduce compliance after a tax hike, either as a retaliatory response or because the increase undermines their perception of fairness in the system.

Next, we present a series of robustness and falsification tests. We begin with a placebo date test, in which we replicated the main analysis using a fictitious reform date of July 1, 2023, instead of the actual reform date. Since no tax reform actually took place on that date, we should not have observed any effect on tax compliance. If significant effects were observed around this placebo reform date, it would suggest that our main results—based on the true reform date—might be spuriously driven by, for example, unusual time trends or other confounding policy changes.

The results from this placebo exercise are presented in Figure 2. Because tax rates remain constant within a calendar year, the first-stage results for this placebo are exactly zero by construction, and we therefore omit those graphs. The reduced-form estimates are presented in panels (a) and (b), for poor and rich households, respectively. As expected, the estimated discontinuities at the placebo threshold are close to zero and statistically insignificant ($\beta_{poor}^{own} = -0.074$, p-value = 0.907; $\beta_{rich}^{own} = 0.046$, p-value = 0.827). The null results from the placebo test provides reassurance that the effects of the actual reform reflect true causal impacts rather than spurious correlations. It is important to note that, although July 1 may appear arbitrary, it is in fact the only feasible date for this placebo

test. Since the outcome is defined as the change from six months before to six months after the reform date, a fake date after July 1 could partially capture the effects of the 2024 reform, while an earlier one could partially pick up the effects of the 2022 reform.

As an additional robustness check, Figure 3 presents the results of a placebo test in which we re-estimate the RDD using arbitrary thresholds. Specifically, we use placebo cutoffs at AR\$ 500K and AR\$ 2M, rather than the true thresholds of AR\$ 750K and AR\$ 1.5M. Again, if we observed significant discontinuities at the placebo thresholds, it would suggest that our main results—based on the true thresholds—might be spurious. On the contrary, Figure 3 shows no significant discontinuities at the placebo thresholds, and thus provides reassurance for the validity of our main results. More precisely, panels (a) and (b) show that the estimated discontinuities in tax rates at these points are close to zero and statistically insignificant ($\beta_{poor}^{own} = 0.003$, p-value = 0.592; $\beta_{rich}^{own} = -0.002$, p-value = 0.652). Similarly, panels (c) and (d) show no significant effects on tax compliance ($\beta_{poor}^{own} = -1.612$, p-value = 0.274; $\beta_{rich}^{own} = -1.596$, p-value = 0.371). Moreover, while Figure 3 presents results based on two arbitrary placebo thresholds, Figure D.2 demonstrates that the conclusions hold more generally across a wider range of placebo thresholds.

Taken together, these placebo tests provide strong evidence that our findings are not driven by pre-existing trends or confounding factors. Moreover, we show that the results are robust to a wide host of additional robustness checks. Appendix D.1 presents the density of the running variable and shows no evidence of manipulation around either cutoff. This is as expected, given the context—manipulation by households is virtually impossible, since assessed values are calculated and reported by the provincial tax authority. Appendix D.2 presents additional robustness checks, including the inclusion of pre-reform controls, restricting the sample to residential properties only, using alternative definitions of the dependent variable (e.g., payments within six months of the due date instead of three), applying a uniform kernel, and estimating models with polynomials of different degrees, among others.

3.3 Own-Rate Elasticities

To facilitate interpretation, we express the magnitude of the own-rate effects in terms of behavioral elasticities. Let β_j^{own} represent the reduced-form effect on compliance for group j , where $j \in \{poor, rich\}$, obtained from the RDD. Let α_j denote the first-stage effect of the reform on tax rates for group j , also obtained from the RDD analysis. Let C_j^{own} and τ_j denote the baseline compliance levels and tax rates, captured by the levels around the relevant thresholds. For each elasticity shown below, we report 90% confidence intervals in brackets, calculated with 5,000 bootstrap iterations:

$$\varepsilon_{poor}^{own} = \frac{\frac{\beta_{poor}^{own}}{C_{poor}^{own}}}{\frac{\alpha_{poor}}{\tau_{poor}}} = \frac{\frac{-2.718}{60.1}}{\frac{0.392}{2.221}} = \frac{-0.257}{[-0.388, -0.103]} \quad (1)$$

$$\varepsilon_{rich}^{own} = \frac{\frac{\beta_{rich}^{own}}{C_{rich}^{own}}}{\frac{\alpha_{rich}}{\tau_{rich}}} = \frac{\frac{-2.248}{58.3}}{\frac{0.146}{1.837}} = \frac{-0.484}{[-0.906, -0.023]} \quad (2)$$

The own-rate elasticity for poor households is -0.257, indicating that for every 1% decrease in their tax rate, compliance increases by 0.257%. In turn, the own-rate elasticity for rich households is -0.484, implying that for every 1% increase in their tax rate, their compliance decreases by 0.484%. Both elasticities are not only statistically significant, but also economically meaningful, reflecting substantial behavioral responses to tax rate changes. The elasticity is larger (in absolute value) for the rich (-0.484) than for the poor (-0.257), suggesting that households may be more likely to reduce compliance after a tax hike than to increase it 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).

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 were able to replicate our own-rate analysis of the 2023 reform using data from the 2024 reform. There are two limitations of the 2024 analysis, however. First, the experiment was conducted during a more turbulent political and economic context—explained in more detail in Section 4.6 below, as it is more relevant to the field experiment. Second, for the 2024 analysis, we had only two months of post-treatment data. However, this is not a major limitation, as this time horizon still provides reasonable statistical power.

The own-rate effects from the 2024 reform are presented in Appendix I.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 responded to the tax reduction by increasing their compliance, whereas rich households responded to the tax increase by reducing their compliance. Notably, the own-rate elasticities are nearly identical between the two reforms: 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. This consistency lends confidence to the accuracy and robustness of the results.

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. We designed a natural field experiment to identify these cross-rate effects. 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.^{22,23} For reference, Appendix L.1 displays a sample letter, in this case sent to a recipient from the group of poor households. 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.

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 (for reference, see Appendix L.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.²⁴

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 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 ac-

²²We excluded 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 excluded 12,689 taxpayers with a registered email address from the main sample, as they constituted a separate survey sample. As discussed in the following section, the results remained consistent when they were included.

²³This 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.

²⁴For sample letters sent to middle and rich households, see Appendix L.1.

tual reform—changes to the variable component of the tax. However, due to the existence of a fixed component, the actual discounts and surcharges on the total tax liability were somewhat smaller than those described in the message.²⁵ 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).

Because the reform was neither widely discussed publicly nor covered in the media—and was approved and implemented over a relatively short period—most taxpayers were likely unaware of it or its details. This interpretation is supported by the survey data presented in the results section, which show that only a minority of control group respondents reported being aware of the reform. Given this context, we believed the treatment had a meaningful opportunity to raise awareness and salience of the reform.

Each subject selected to receive a letter was randomly assigned, with equal probability, to either a treatment or a control letter.²⁶ Because subjects in different groups (poor, middle, and rich) were impacted differently by the reform, we wanted to study the treatment effects separately for each group. For that reason, we stratified the randomization across the three property valuation groups. Appendix E shows that, consistent with random assignment, the treatment arms are balanced in observable characteristics.

Lastly, a minor design feature is that a small group of taxpayers (5,913) was randomized to receive no letter at all. The purpose of this secondary treatment arm was to assess whether recipients were attentive to the letters. As shown in prior research on tax reminders (Cruces et al., 2025), letters that include useful payment information and serve as reminders can increase compliance provided they are opened. Thus, comparing the control letter group with the no-letter group offers a straightforward way to validate whether taxpayers were reading the correspondence.

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 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

²⁵For 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.

²⁶For 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.

set aside a group of 14,646 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.²⁷ We thus sent 14,646 emails inviting taxpayers to participate, providing a short description of the study and a direct survey link. Because taxpayers invited to the survey received 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 excluded them from the main experimental sample in our analysis.²⁸

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 11.8%, which is high compared to benchmarks in similar survey-based tax compliance studies.²⁹ After excluding a small number of households that did not have property value assessments, we ended up with a final sample of 1,723 survey responses. Table E.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. One caveat, however, is that the email sample is not fully representative of the broader taxpayer population. Most notably, pre-treatment compliance rates are significantly higher in the email sample than in the main sample.³⁰ Despite this difference, most other baseline characteristics are fairly similar between the email sample and the rest of the taxpayers (see Appendix G). In any case, the survey was primarily designed to provide qualitative insights into the underlying mechanisms, so full representativeness is not essential for its purpose.

Appendix M.2 presents the complete survey instrument, which we describe below. One challenge was that the survey would take place significantly later than the letter: the median respondent completed the survey 52 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 largely dissipated by then. For that reason, we reinforced

²⁷Due to concerns about potential contamination from the survey content, our baseline results excluded the email sample from the analysis of the treatment effects on compliance. Nonetheless, Appendix D.2 shows that the results remain consistent when this group is included.

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

²⁹For example, Bergolo et al. (2020) reported a response rate of 8.9% in an online survey of taxpayers in Uruguay who received email invitations similar to ours.

³⁰The average compliance rate in the control group is 74.1% in the email sample, compared to 47.9% in the main sample.

the message by showing it to subjects in both the email invitation and on the second page of the survey.³¹

The first goal of the survey was to assess whether the treatment message had the intended effect on awareness of the reform. Participants were asked about their awareness of various recently implemented municipal policies. In particular, we asked whether they were aware of a measure introducing “discounts for low-valuation properties and additional charges for high-valuation properties” in the local property tax. This information appeared in the treatment letter but not in the control letter, so we would expect different responses across the two groups. Awareness of other municipal policies—mentioned in both the treatment and control letters—can serve as a placebo outcomes.

The second goal of the survey was to measure the effect of the treatment on perceived tax fairness. Participants were asked to rate the fairness of the property tax schedule: “How fair do you think the current distribution of municipal tax rates is between people with more and fewer resources—that is, how much each of these groups pays?” Responses were recorded on a 0–10 scale, ranging from “Very Unfair” to “Very Fair.” A positive treatment effect would suggest that households viewed the progressive reform as making the schedule fairer, while a negative effect would indicate the opposite. We also included an adapted version of a General Social Survey question on the government’s role in reducing the gap between rich and poor. As discussed below, we used this question as a placebo outcome: because it was designed to capture deeply held general preferences rather than views on a specific policy, we did not expect it to be affected by the information on the property tax reform.

4.3 Effects on Reform Awareness and Perceptions

The validity of mailing experiments like ours relies on households opening and engaging with the letters. In the extreme, if taxpayers did not open the letters—or did not turn to the back of the page—they would not have been exposed to the 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 compared 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 F.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

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

³²The 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

households, meaning that all three groups were equally likely to open the correspondence.

Next, we investigated whether the treatment message, compared to the control message, 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 15.3% of the respondents in the control group indicated 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.

Next, we provide results from a falsification check. The back of the letter also included information about three additional policies the municipality had implemented—business credits, free and express commercial permits, 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 these three policies. So, we should not expect differences in awareness of these policies across treatment and control groups. Panels (c) and (d) of Figure 5 replicate panels (a) and (b), but for the first of the placebo outcomes, awareness of the business credits—results for the other two placebos are presented in Appendix H. As expected, the treatment effect on this placebo outcome is close to zero and statistically insignificant. The overall null effects on the placebo outcomes are reassuring, suggesting that the observed effects on perceived fairness are neither spurious nor driven by mechanical factors such as anchoring.

Having established that taxpayers read and incorporated the information in our treatments, we were able to turn to a series of outcomes related to the taxpayers’ perceived tax fairness. These results are presented in Figure 6. More precisely, panel (a) shows relatively high perceived levels of fairness reported by taxpayers in the control group, with an average stated perception of approximately 5.5 points (on a 1–10 scale), and roughly similar across poor, middle, and rich households. Most importantly, panel (b) of Figure 6 shows that the treatment increased perceived fairness across all groups, indicating that, on average, households viewed the more progressive tax schedule as fairer.³³ In the full sample, the average treatment effect is positive, statistically significant and economically

large effects because they reflect a substantial reminder effect of receiving a letter, beyond the content of the letter itself. In contrast, our experiment produced smaller effects because it compared subtle variations between two letter types, effectively netting out the basic reminder effect.

³³This finding is consistent with prior evidence that some taxpayers have appetite for progressive tax schedules (e.g., Durante et al., 2014; Stantcheva, 2021; Hoy, 2025; Nathan et al., 2024).

significant too (0.57 points on a 0–10 scale). Moreover, this effect is similar across all three groups—poor, middle, and rich households.

We also provide a falsification check using a placebo outcome. We tested whether our treatment had any effect on the respondents’ general views on the role of government in reducing the gap between the rich and the poor, which reflects deeply rooted general preferences. Reassuringly, the results in panel (d) of Figure 6 show that our information treatment did not influence this placebo outcome. The absence of placebo effects strengthens the credibility of our findings, indicating that the treatment’s impact on perceived fairness is unlikely to be an artifact of cognitive biases, such as anchoring, or other mechanical survey effects.

4.4 Effects on Tax Compliance

We now turn to the core outcome of our experiment: the cross-rate effects on actual tax compliance, as measured using 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. We estimated the following regression separately for each pre-treatment and post-treatment month:

$$Y_i = \theta_1 \cdot \text{Treat}_i \cdot \text{Poor}_i + \theta_2 \cdot \text{Treat}_i \cdot \text{Middle}_i + \theta_3 \cdot \text{Treat}_i \cdot \text{Rich}_i + \theta_4 \cdot \text{Poor}_i + \theta_5 \cdot \text{Middle}_i + \theta_6 \cdot \text{Rich}_i + X_i \theta_7 + \varepsilon_i \quad (3)$$

The outcome Y_i is an indicator variable for whether that month’s tax bill was paid within three months of the due date. The key coefficients of interest are the interactions between the treatment indicator (Treat_i) and the group indicators (Poor_i , Middle_i , Rich_i),³⁴ allowing us to estimate group-specific average treatment effects. X_i is a vector of additional control variables: 12 months of pre-treatment outcomes,³⁵ indicator variables for whether the property was an “always payer” or “never payer” in the previous year, property valuation (in logs), an indicator for residential properties, and an indicator for properties that made an annual payment in the previous year.³⁶ ε_i is the usual error term. The standard errors are clustered at the individual level.

Panel (a) of Figure 7 shows the results from the event-study analysis. The x-axis

³⁴An omitted category is not needed because the regression does not include a constant term.

³⁵For the post-treatment periods, these are the compliance indicators during each of the 12 months of 2022. For the pre-treatment periods, these are the compliance indicators during each of the previous 12 months.

³⁶Some control variables have a few missing values. To avoid dropping these observations, for each of these variables, we imputed missing values with zeros and included an additional variable indicating whether the value was imputed.

indicates the number of months before or after the letters were mailed, while the y-axis corresponds to the treatment effects on tax compliance. The pre-treatment coefficients, which are close to zero and statistically insignificant, demonstrate successful random assignment: i.e., prior to the mailing intervention, tax compliance was balanced between treatment and control groups. This is true within each of the three groups—poor, middle, and rich households.

We begin by describing the effects for poor households. In the post-treatment period, there is a clear, positive, and significant impact of the treatment on compliance—the six post-treatment coefficients are mostly statistically significant and range from 0.5 to 1.0 percentage points. This indicates that, relative to poor households who did not receive the information, those who learned about the tax hike on the rich increased their tax compliance. In terms of the evolution of the effects, panel (a) of Figure 7 suggests that the impact may gradually dissipate over time. This pattern is consistent with evidence of waning effects in other correspondence experiments, presumably because the information fades from taxpayers’ immediate memory (e.g., [Bergolo et al., 2023](#)).³⁷

In contrast to the significant positive effects observed for poor households, the treatment effect for rich households was negative, though less precisely estimated and mostly statistically insignificant. Relative to rich households who did not receive the information, those who learned that the poor received a tax cut—if anything—reduced their compliance. This finding stands in sharp contrast to the survey results, which show that rich households responded to the same information by reporting higher perceptions of fairness. For middle households, we find a similar inconsistency between their survey responses and their actual behavior. After learning about the effects of the reform on other groups—namely, the tax cut for the poor and the tax hike for the rich—the survey data indicate that middle households reported increased perceptions of fairness. However, this information did not alter their compliance behavior, suggesting they did not put their money where their mouths were.

One limitation of the event-study analysis is that it measures effects at the most granular level (monthly), which is inefficient in terms of statistical power. To provide a more precise estimate of the treatment effects, we present results at a more aggregated level (quarterly). Specifically, we estimate the following regression separately for the three months before the mailing (the pre-treatment period) and the three months immediately after (post-treatment period):

$$Y_{i,t} = \theta_0 + \theta_1 \cdot \text{Treat}_i + X_i\theta_2 + \omega_t + \varepsilon_{i,t} \quad (4)$$

³⁷Also consistent with this interpretation, the gradual decline in treatment effects aligns with the results of the comparison between control letters and no letters.

The outcome Y_{it} is an indicator for whether month t 's bill was paid within three months of the due date. Treat_i and X_i are defined as in equation (3). One difference with equation (3), however, is that this specification estimates the regression separately for each group (poor, middle, and rich). ω_t is a vector of month fixed effects and ε_i is the usual error term. The standard errors are clustered at the individual level.

Panel (b) of Figure 7 presents the results from this alternative specification. For poor households, we find a positive treatment effect of 0.808 pp (p-value=0.006). As discussed in more detail in Section 4.5 below, this effect is not only statistically significant but also economically meaningful. For rich households, we find a negative treatment effect of -0.313 pp, but even though it is more precisely estimated, it is still statistically insignificant at conventional levels (p-value=0.393). For middle households, we find an effect close to zero (0.152 pp) and statistically insignificant (p-value=0.619).

We show that these results are robust to several checks. Appendix F.1 demonstrates that the treatment effects described above are also evident in the raw time series data on tax compliance. Appendix D.2 shows that the results hold under alternative sets of control variables, different sample restrictions (e.g., limiting to residential properties), and alternative definitions of compliance (e.g., payments made within six months of the due date instead of three), among others.

The cross-rate effect for poor households reported above likely understates the true size due to substantial attenuation bias.³⁸ While we provided evidence that a significant share of households opened the letters, this does not necessarily imply that all recipients read them fully or carefully. The control and treatment messages were on the second page of the letter, and it is likely that a significant share of households either did not flip to the second page or did not pay attention to the letter at all. Thus, the treatment effect reported above (0.808 pp for poor households) should be interpreted as an intention-to-treat (ITT) effect. To obtain the Average Treatment Effect on the Treated (ATET), we need to scale up the ITT by an adjustment factor that accounts for non-compliance. The recent literature on correspondence experiments discusses adjustment factors between 2 and 7.5 (Perez-Truglia and Cruces, 2017; Gerber et al., 2020; Nathan et al., 2020; Bottan and Perez-Truglia, 2025). For example 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 2.³⁹ This would result in a scaled-up cross-rate effect for poor households of 1.616 pp ($= 0.808 \cdot 2$).

³⁸It could be argued that the own-rate effects (Section 3) could be affected by *some* attenuation bias as well, because not every taxpayer was necessarily aware of the change in their own tax liability. However, this attenuation bias is probably minor, and adjusting for it would be more challenging.

³⁹An alternative approach using survey results yields a much higher adjustment factor—see Appendix F.3 for details.

4.5 Cross-Rate Elasticities

We can assess the magnitude of the cross-rate effects for poor and rich households by expressing them as behavioral elasticities.⁴⁰ Let β_j^{cross} represent the treatment effect on compliance for group j as reported in the experiment, where $j \in \{poor, rich\}$. Let C_j^{cross} denote the baseline compliance of each control group in 2023, and let $\frac{\Delta\tau_{-j}}{\tau_{-j}}$ represent the tax rate change for the opposite group. That is, for a poor household, the relevant change in the opposite group's tax rate corresponds to the tax hike on the rich; for a rich household, it corresponds to the tax cut for the poor. 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.⁴¹ The results are presented below, with 90% confidence intervals in brackets, calculated using 5,000 bootstrap iterations:

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. On the other hand, the own-rate elasticities are based on the total tax.

$$\varepsilon_{poor}^{cross} = \frac{\frac{\beta_{poor}^{cross}}{C_{poor}^{cross}}}{\frac{\Delta\tau_{rich}}{\tau_{rich}}} = \frac{\frac{0.808}{55.2}}{0.15} = \frac{0.098}{[0.040, 0.158]} \quad (5)$$

$$\varepsilon_{rich}^{cross} = \frac{\frac{\beta_{rich}^{cross}}{C_{rich}^{cross}}}{\frac{\Delta\tau_{poor}}{\tau_{poor}}} = \frac{\frac{-0.313}{54.8}}{-0.3} = \frac{0.019}{[-0.018, 0.055]} \quad (6)$$

On the one hand, the cross-rate elasticity for rich households (0.019) is statistically insignificant, which is unsurprising given that the treatment effect for this group was also insignificant. However, we can reject the null hypothesis that the cross-rate elasticity for the rich equals that for the poor (p-value = 0.059), highlighting a meaningful difference in responses between the two groups. On the other hand, the cross-rate elasticity for poor households (0.098) is statistically significant. This is not surprising given that the treatment effect for the poor was significant, but this elasticity helps illustrate the economic magnitude of the response: for each 1% increase in the tax rate of rich households, poor

⁴⁰We do not compute a cross-rate elasticity for middle households because we lack sufficient information. The treatment for this group provided information about both a tax cut for the poor and a tax hike for the rich. As a result, even if their compliance responded to the treatment, we would not be able to attribute that response to one change or the other.

⁴¹Intuitively—and for simplicity—the calculations in this section assume that households interpreted the information about changes to other groups tax rates as applying to the entire tax, rather than just the variable component. This is likely a reasonable assumption given the limited-attention context and the fact that details about the variable component appeared in fine print. In any case, this simplified approach is the more conservative one, as the alternative would result in smaller denominators and thus larger elasticities.

households increase their compliance rate by 0.098%. To put this in perspective, we can compare it to the own-rate elasticity for poor households (-0.257): a 1% increase in the tax rate of the rich has the same effect on poor households' compliance as lowering the poor households' own tax rate by 0.38% ($= -\frac{0.098}{-0.257}$). Moreover, because of substantial attenuation bias from imperfect treatment uptake, the true cross-rate elasticity is likely at least twice as large. Under a conservative adjustment factor of 2, a 1% increase in the tax rate of the rich would have the same effect as lowering the poor households own tax rate by 0.76%. In sum, this evidence suggests that, beyond the direct effect of ones own tax rate, perceptions of the broader tax schedule can play an important role in shaping tax morale.

4.6 Replication: The 2024 Reform

Taking advantage of the new progressive reform in 2024, we conducted a second large-scale field experiment to assess whether the results of the 2023 experiment replicated. In addition to the changes to the variable components from the 2023 reform, the 2024 reform made the fixed component more progressive. The 2024 treatment message was similar to that 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 used the 2024 experiment as an opportunity to refine the design by making the distinction between the treatment and control messages even more subtle. In the 2023 letters, both treatment and control versions displayed the magnitude of the progressivity discount for the recipient on the front page. However, a limitation of the 2023 control letter was that it did not include an infographic and therefore did not reiterate that information about the size of the discount in the message on the second page. As shown in the screenshots provided in Appendix L.2, the 2024 letters further minimized the differences between treatment and control messages. In contrast to 2023, when only the treatment group received information in the form of an infographic, in 2024 both groups received one. The treatment group's infographic showed the tax changes for all three groups—poor, middle, and rich—while the control group's infographic featured a single bar representing the change for the households own group only.⁴³

⁴²For details on the 2024 experimental design see Appendix I.1.

⁴³For 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: the discount for poor households, the surcharge for rich households, and no change for middle households.

While the reform and letters were similar in 2023 and 2024, two key contextual differences could have attenuated the treatment effects in 2024. First, the 2024 reform and its letters were introduced during a turbulent economic and political period, right after a heated presidential election, which may have made the reform seem less significant relative to broader national events. Second, the similarity between the reforms (and messages) may have led some taxpayers in 2024 to misinterpret our communications as referring to the previous year’s reform, potentially weakening their response.

With these caveats in mind, we replicated our analysis of the 2023 experiment using data from the 2024 experiment. The complete set of results is presented in Appendices I.3 and I.4 and is summarized below. Consistent with the 2023 results, we again observe in 2024 a significant difference in compliance between subjects who were assigned to the control letter and those who were assigned 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 2023 experiment. For poor households, we again found 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 estimated for the 2023 experiment. As in 2023, in the 2024 experiment we found a negative but statistically insignificant effect on rich households and an effect for middle households that is close to zero and statistically insignificant. The fact that the two experiments yielded consistent findings, despite notable differences in context, highlights the robustness of our results.

5 Counterfactual Analysis

5.1 Framework

In this section, we conduct a counterfactual analysis to illustrate the policy implications of our findings. We consider the effects of a hypothetical progressive reform that, similar to the actual reform implemented in January 2023, reduces the tax rate on poor households by $\% \Delta \tau_{poor}$ and increases the tax rate on rich households by $\% \Delta \tau_{rich}$. These parameters are calibrated so that, in a world with fixed tax compliance, the reform is revenue-neutral and achieves a high progressivity target: rich households pay an effective tax rate 0.5 pp higher than that of poor households. This outcome is achieved by changing the tax rates by $\% \Delta \tau_{poor} = -45.5\%$ and $\% \Delta \tau_{rich} = +17.32\%$. We aim to understand the effects of this reform on effective tax progressivity and tax revenues under behavioral responses, and contrast it to a counterfactual scenario that ignores behavioral responses.⁴⁴

⁴⁴Throughout 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

Our hypothetical economy was calibrated to reflect many features of the real-world context, but we simplified certain aspects for the sake of brevity. We assume households are homogeneous within each property value bracket, allowing us to focus on average effects.⁴⁵ Let subscript $j \in \{poor, middle, rich\}$ denote poor, middle, and rich households. Let γ_j be the share of households in each of these three groups. Let superscript s denote the scenarios: $s = 0$ represents the status quo (i.e., before the reform), while $s = A$ is the *actual* post-reform scenario (with 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. When behavioral responses are included, there is an additional channel operating through compliance: i.e., the amount of taxes paid ($T_j^s = L_j^s \cdot C_j^s$) varies due to changes in tax compliance (C_j^s). To compare the outcomes with and without behavioral responses, we adopted a sufficient statistic approach (Chetty, 2009). The behavioral responses can be approximated as a linear function of the four behavioral elasticities estimated above (ε_{poor}^{own} , ε_{rich}^{own} , $\varepsilon_{poor}^{cross}$ and $\varepsilon_{rich}^{cross}$):

$$C_{poor}^A = C_{poor}^0 \cdot (1 + \varepsilon_{poor}^{own} \cdot \% \Delta \tau_{poor} + \varepsilon_{poor}^{cross} \cdot \% \Delta \tau_{rich}) \quad (7)$$

$$C_{rich}^A = C_{rich}^0 \cdot (1 + \varepsilon_{rich}^{own} \cdot \% \Delta \tau_{rich} + \varepsilon_{rich}^{cross} \cdot \% \Delta \tau_{poor}) \quad (8)$$

Following the conservative approach outlined in Section 4, we doubled the cross-rate elasticities to account for attenuation bias. Since each of the four elasticities is subject to

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 necessarily translate into effective progressivity.

⁴⁵In 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.

⁴⁶In 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.

sampling error, we used bootstrap for inference.⁴⁷

There are some implicit assumptions in this exercise that are worth noting. First, to simplify the exposition of the results, we assume that the behavioral responses of middle households are exactly zero.⁴⁸ Second, there is an implicit extrapolation assumption for the own-rate elasticities. These elasticities were estimated by means of a regression discontinuity design and thus correspond to the local average treatment effects for households near the corresponding thresholds. We applied these estimates to all taxpayers within each group, implicitly assuming that the effects estimated near the thresholds are reasonable approximations of the group averages.⁴⁹

5.2 Results

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

The key takeaway from panel (a) of Figure 8 is that the reform ends up being less progressive than intended due to the behavioral responses. The first bar from panel (a) shows a slightly regressive tax system at baseline: 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%. 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: rich households pay an effective rate of 1.25% versus 0.75% for poor households. The third bar incorporates the behavioral responses of poor households only.⁵⁰ 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 counterfactual without the behavioral responses. This difference is precisely estimated and highly statistically significant (p-

⁴⁷Within each bootstrap sample, we estimated the four elasticities of interest and used those values to compute C_j^A following equations (7) and (8). We then used the distribution of the results across the bootstrap samples to calculate the 90% confidence intervals and p-values.

⁴⁸For the own-rate channel, this is true by construction since the tax rates for this group were not affected by the reform. And for the cross-rate channel, there is support for this assumption insofar as the treatment effects for middle households were close to zero and statistically insignificant.

⁴⁹A similar extrapolation assumption applies to the cross-rate elasticity too.

⁵⁰In practice, this operates by simply taking equation (7) and setting the behavioral elasticities corresponding to rich taxpayers to zero.

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 rich and poor groups simultaneously. Since both effects reduce progressivity, the net result is also a reduction in effective tax progressivity of 0.240 pp (p-value = 0.002). In other words, the effective progressivity is 48% lower than it would have been without behavioral responses.

The key takeaway from panel (b) of Figure 8 is that the reform—though intended to be revenue-neutral—ultimately reduced tax revenues due to behavioral responses. Panel (b) mirrors panel (a) but focuses on tax revenues instead of effective tax progressivity. 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, shows the same per capita revenue as in the pre-reform scenario. This outcome is by design, as the reform was calibrated to be revenue-neutral in the absence of 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 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 the net effect is imprecisely estimated and statistically insignificant (p-value = 0.145).

Appendix J provides more details and discussion about the results of the counterfactual analysis. In particular, while the above analysis breaks down the behavioral responses between rich and poor households, Appendix J.2 further decomposes those effects into own-rate versus cross-rate effects. We show that, while not a majority, a sizable portion of the counterfactual results are driven by the indirect channel.

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 evidence in the context of a municipal property tax reform in Argentina.

Our analysis revealed significant *direct* effects of the progressive reform: reductions in tax rates for poor households significantly increased their compliance, while increases for rich households resulted in decreased compliance. Additionally, we documented significant *indirect* effects: when informed about tax increases on rich households, poor households increased their perceived fairness of the tax schedule and they also increased their tax compliance. These findings demonstrate that behavioral responses to tax reform are shaped not only by the direct pecuniary incentives but also by perceptions of the fairness of the broader tax system. In contrast, while middle and rich households also stated improved perceptions of fairness due to the progressive reform, their tax compliance did not increase. 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 express support for more progressive taxes, but talk is cheap—and they do not always put their money where their mouth is. One interpretation of this finding is that, although middle and rich households may *say* they view the tax cut for the poor as fair and claim to be supportive, they may in fact be secretly resentful.

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 in other contexts. For instance, 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. For example, the magnitude of the effects documented in this paper is likely to differ in other settings, depending on contextual factors such as the prevailing ideological orientation of the society (e.g., left-leaning vs. right-leaning).

Lastly, our findings have implications for the design of progressive tax reforms. In-

deed, 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). In most countries, property taxes are either proportional or even regressive, 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. Our counterfactual analysis showed that accounting for behavioral responses—direct and indirect—significantly reduces 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.

References

- Antinyan, A. and Asatryan, Z. (2024). Nudging for tax compliance: a meta-analysis. *The Economic Journal*, page ueae088.
- Bachas, P., Jensen, A., and Gadenne, L. (2024). Tax equity in low-and middle-income countries. *Journal of Economic Perspectives*, 38(1):55–80.
- Ballard-Rosa, C., Martin, L., and Scheve, K. (2017). The structure of American income tax policy preferences. *The Journal of Politics*, 79(1):1–16.
- Bergeron, A., Tourek, G., and Weigel, J. L. (2024). The state capacity ceiling on tax rates: Evidence from randomized tax abatements in the DRC. *Econometrica*, 92(4):1163–1193.
- Bergolo, M., Ceni, R., Cruces, G., Giacobasso, M., and Perez-Truglia, R. (2023). Tax audits as scarecrows: Evidence from a large-scale field experiment. *American Economic Journal: Economic Policy*, 15(1):110–53.
- Bergolo, M. L., Leites, M., Perez-Truglia, R., and Strehl, M. (2020). What makes a tax evader? Working Paper 28235, National Bureau of Economic Research.
- Besley, T., Jensen, A., and Persson, T. (2023). Norms, enforcement, and tax evasion. *Review of Economics and Statistics*, 105(4):998–1007.
- Best, M. C., Caloi, L., Gerard, F., Kresch, E. P., Naritomi, J., and Zoratto, L. (2025). Greener on the other side: Inequity and tax compliance. *Working Paper*.
- Bottan, N. L. and Perez-Truglia, R. (2022). Choosing Your Pond: Location Choices and Relative Income. *The Review of Economics and Statistics*, 104(5):1010–1027.
- Bottan, N. L. and Perez-Truglia, R. (2025). Betting on the House: Subjective expectations and Market Choices. *American Economic Journal: Applied Economics*, 17(1):459–500.
- Brockmeyer, A., Estefan, A., Ramírez Arras, K., and Suárez Serrato, J. C. (2023). Taxing property in developing countries: Theory and evidence from Mexico. *NBER Working Paper No. 28637*.
- Brühlhart, M., Gruber, J., Krapf, M., and Schmidheiny, K. (2022). Behavioral responses to wealth taxes: Evidence from Switzerland. *American Economic Journal: Economic Policy*, 14(4):111–150.
- Bursztyn, L., Haaland, I., Röver, N., Roth, C., and Haaland, I. K. (2025). The social desirability atlas. Technical report, University of Bonn and University of Cologne, Germany.
- Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.
- Carrillo, P. E., Castro, E., and Scartascini, C. (2021). Public good provision and property tax compliance: Evidence from a natural experiment. *Journal of Public Economics*, 198:104422.
- Castro, L. and Scartascini, C. (2015). Tax compliance and enforcement in the pampas evidence from a field experiment. *Journal of Economic Behavior & Organization*, 116:65–82.
- Cattaneo, M. D., Idrobo, N., and Titiunik, R. (2023). *A Practical Introduction to Regression Discontinuity Designs: Extensions*. Cambridge Elements: Quantitative and Computational Methods for Social Science, Cambridge University Press.

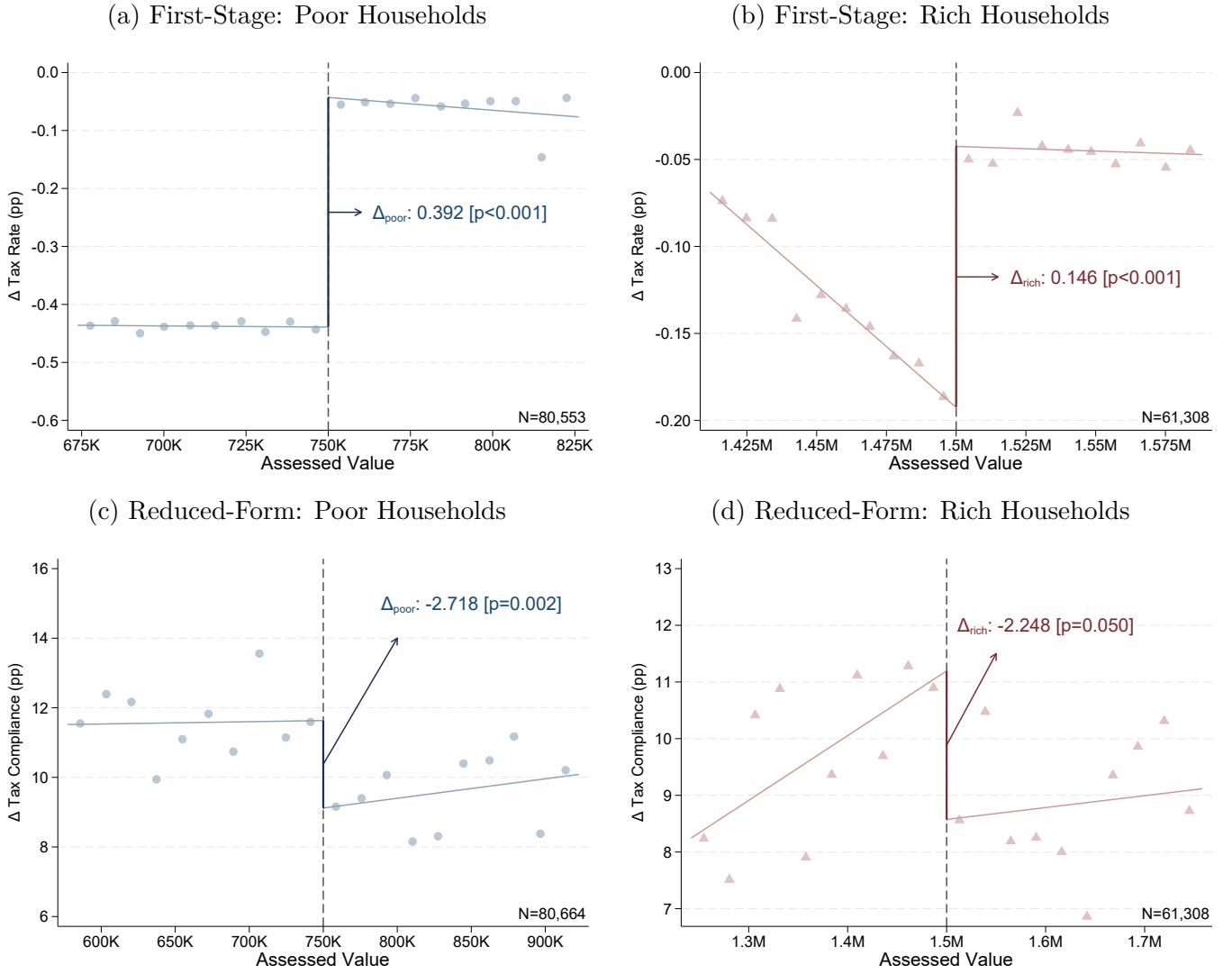
- Cattaneo, M. D., Jansson, M., and Ma, X. (2018). Manipulation testing based on density discontinuity. *Stata Journal*, 18(1):234–261.
- Cattaneo, M. D., Jansson, M., and Ma, X. (2020). Simple local regression distribution estimators. *Journal of the American Statistical Association*, 115(531):1449–1455.
- Cattaneo, M. D., Keele, L., Titiunik, R., and Vazquez-Bare, G. (2016). Interpreting regression discontinuity designs with multiple cutoffs. *Journal of Politics*, 78(4):1229–1248.
- Cavallo, A., Cruces, G., and Perez-Truglia, R. (2017). Inflation expectations, learning, and supermarket prices: Evidence from survey experiments. *American Economic Journal: Macroeconomics*, 9(3):1–35.
- CEDLAS (2024). Socio-Economic Database for Latin America and the Caribbean (SEDLAC). Joint project with the World Bank’s Poverty and Equity Global Practice.
- Chancel, L., Piketty, T., Saez, E., and Zucman, G. (2022). *World Inequality Report 2022*. Harvard University Press.
- Chetty, R. (2009). Sufficient statistics for welfare analysis: A bridge between structural and reduced-form methods. *Annual Review of Economics*, 1:451–488.
- Chirico, M., Inman, R. P., Loeffler, C., MacDonald, J., and Sieg, H. (2016). An experimental evaluation of notification strategies to increase property tax compliance: free-riding in the city of brotherly love. *Tax Policy and the Economy*, 30(1):129–161.
- Cruces, G., Tortarolo, D., and Vazquez-Bare, G. (2025). Design of partial population experiments with an application to spillovers in tax compliance. *The Review of Economics and Statistics*, pages 1–45.
- Cullen, J. B., Turner, N., and Washington, E. (2021). Political alignment, attitudes toward government, and tax evasion. *American Economic Journal: Economic Policy*, 13(3):135–66.
- Del Carpio, L. (2014). Are the neighbors cheating? Evidence from a social norm experiment on property taxes in Peru. *Unpublished Manuscript, Princeton University*.
- Dong, Y., Lee, Y.-Y., and Gou, M. (2023). Regression discontinuity designs with a continuous treatment. *Journal of the American Statistical Association*, 118(541):208–221.
- Dray, S., Landais, C., and Stantcheva, S. (2023). Wealth and property taxation in the United States. *NBER Working Paper No. 31080*.
- Durante, R., Putterman, L., and Van der Weele, J. (2014). Preferences for redistribution and perception of fairness: An experimental study. *Journal of the European Economic Association*, 12(4):1059–1086.
- Dwenger, N., Kleven, H., Rasul, I., and Rincke, J. (2016). Extrinsic and intrinsic motivations for tax compliance: Evidence from a field experiment in Germany. *American Economic Journal: Economic Policy*, 8(3):203–232.
- Dzansi, J., Jensen, A., Lagakos, D., and Telli, H. (2022). Technology and tax capacity: Evidence from local governments in ghana. Technical report, National Bureau of Economic Research.
- Epper, T., Fehr, E., and Senn, J. (2024). Social preferences and redistributive politics. *Review of Economics and Statistics*, forthcoming.

- Fehr, E., Epper, T., and Senn, J. (2024). Social preferences and redistributive politics. *The Review of Economics and Statistics*, pages 1–45.
- Fisher-Post, M. and Gethin, A. (2023). Government redistribution and development: Global estimates of tax and transfer progressivity, 1980-2019. *PSE Working Paper halshs-04423529*.
- Flores, T., Cruces, G., Bermúdez, J. C., Schiavoni, J. L., Scot, T., and Tortarolo, D. (2025). Exploring the Gender Divide in Real Estate Ownership and Property Tax Compliance. Policy Research Working Paper WPS11060, World Bank.
- Gerber, A., Hoffman, M., Morgan, J., and Raymond, C. (2020). One in a million: Field experiments on perceived closeness of the election and voter turnout. *American Economic Journal: Applied Economics*, 12(3):287–325.
- Giaccobasso, M., Nathan, B., Perez-Truglia, R., and Zentner, A. (2022). Where do my tax dollars go? Tax morale effects of perceived government spending. *American Economic Journal: Applied Economics*, forthcoming.
- Goodman, A. C. and Kawai, M. (1982). Permanent income, hedonic prices, and demand for housing: New evidence. *Journal of Urban Economics*, 12(2):214–237.
- Heathcote, J., Storesletten, K., and Violante, G. L. (2017). Optimal tax progressivity: An analytical framework. *The Quarterly Journal of Economics*, 132(4):1693–1754.
- Hoy, C. (2025). How does progressivity impact tax morale? Experimental evidence across developing countries. *Journal of Development Economics*, 172:103398.
- Jimenez, E. and Keare, D. H. (1984). Housing consumption and permanent income in developing countries: Estimates from panel data in El Salvador. *Journal of Urban Economics*, 15(2):172–194.
- Kapon, S., Del Carpio, L., and Chassang, S. (2024). Using divide-and-conquer to improve tax collection. *The Quarterly Journal of Economics*, 139(4):2475–2523.
- Karmali, N. M. and Weng, X. (2022). Housing demand and affordability in India. *Policy Research Working Paper. World Bank Group*.
- Kerschbamer, R. and Müller, D. (2020). Social preferences and political attitudes: An online experiment on a large heterogeneous sample. *Journal of Public Economics*, 182:104076.
- Khan, A. Q., Khwaja, A. I., and Olken, B. A. (2015). Tax farming redux: Experimental evidence on performance pay for tax collectors *. *The Quarterly Journal of Economics*, 131(1):219–271.
- Kresch, E. P., Walker, M., Best, M. C., Gerard, F., and Naritomi, J. (2023). Sanitation and property tax compliance: Analyzing the social contract in Brazil. *Journal of Development Economics*, 160:102954.
- Kuziemko, I., Norton, M. I., Saez, E., and Stantcheva, S. (2015). How elastic are preferences for redistribution? Evidence from randomized survey experiments. *American Economic Review*, 105(4):1478–1508.
- Locks, G. (2023). Behavioral responses to inheritance taxes: Evidence from Brazil. Available at SSRN 4973341.
- Londoño-Vélez, J. and Ávila-Mahecha, J. (2024). Behavioural responses to wealth taxation: Evidence from Colombia. *Review of Economic Studies*. rdae076.

- Lustig, N. (2023). *Commitment to equity handbook: Estimating the impact of fiscal policy on inequality and poverty*. Brookings Institution Press.
- Luttmer, E. F. P. and Singhal, M. (2014). Tax Morale. *Journal of Economic Perspectives*, 28(4):149–168.
- Manwaring, P. and Regan, T. (2024). No shame in my name: Public disclosure and tax compliance in a low-capacity state. Technical Report UGA-20088, Working paper.
- McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142(2):698–714. The regression discontinuity design: Theory and applications.
- Nathan, B., Perez-Truglia, R., and Zentner, A. (2020). My Taxes are Too Darn High: Why Do Households Protest their Taxes? *American Economic Journal: Economic Policy*, forthcoming.
- Nathan, B., Perez-Truglia, R., and Zentner, A. (2024). Paying your fair share: Perceived fairness and tax compliance. *NBER Working Paper No. 32588*.
- OECD (2018). OECD Regions and Cities at a Glance 2018. Technical report, OECD Publishing, Paris.
- Okunogbe, O. M. (2021). Becoming Legible to the State : The Role of Identification and Collection Capacity in Taxation. Policy Research Working Paper Series 9852, The World Bank.
- Perez-Truglia, R. and Cruces, G. (2017). Partisan interactions: Evidence from a field experiment in the United States. *Journal of Political Economy*, 125(4):1208–1243.
- Perez-Truglia, R. and Troiano, U. (2018). Shaming tax delinquents. *Journal of Public Economics*, 167:120–137.
- Piketty, T. and Saez, E. (2007). How progressive is the US federal tax system? A historical and international perspective. *Journal of Economic Perspectives*, 21(1):3–24.
- Rauh, J. and Shyu, R. (2024). Behavioral responses to state income taxation of high earners: Evidence from California. *American Economic Journal: Economic Policy*, 16(1):34–86.
- Saez, E. and Zucman, G. (2019a). Progressive wealth taxation. *Brookings Papers on Economic Activity*, 2019(2):437–511.
- Saez, E. and Zucman, G. (2019b). *The Triumph of Injustice: How the Rich Dodge Taxes and How to Make Them Pay*. W. W. Norton.
- Saez, E. and Zucman, G. (2023). Distributional tax analysis in theory and practice: Harberger meets Diamond-Mirrlees. *NBER Working Paper No. 31912*.
- Seim, D. (2017). Behavioral responses to wealth taxes: Evidence from Sweden. *American Economic Journal: Economic Policy*, 9(4):395–421.
- Slemrod, J. (2019). Tax Compliance and Enforcement. *Journal of Economic Literature*, 57(4):904–954.
- Stantcheva, S. (2021). Understanding tax policy: How do people reason? *The Quarterly Journal of Economics*, 136(4):2309–2369.

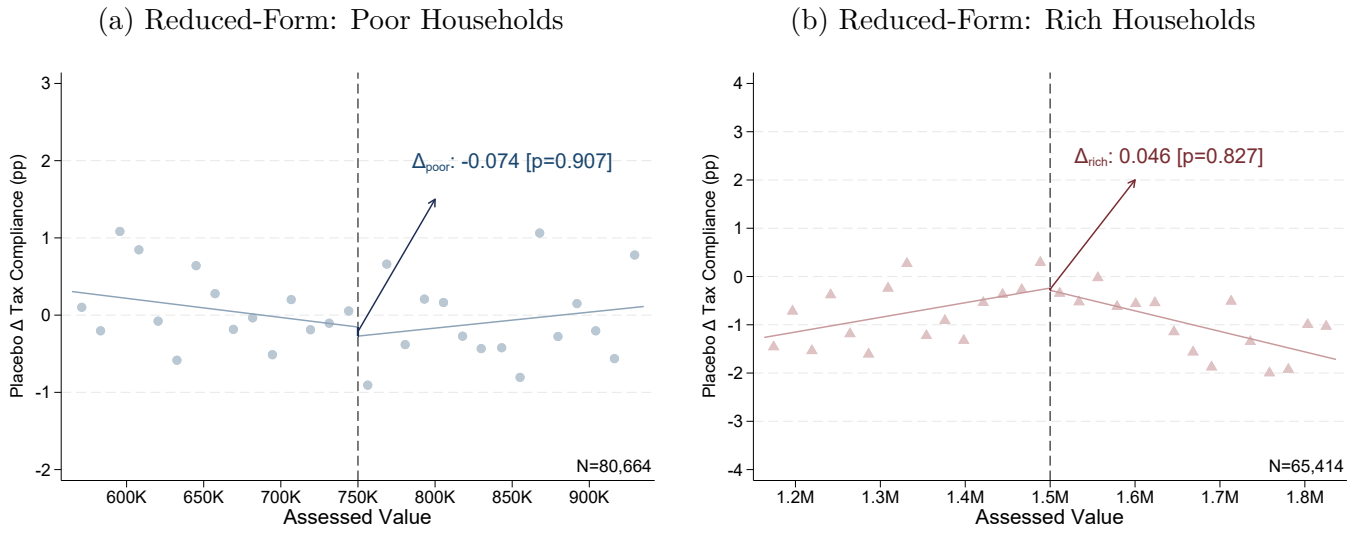
- Tarroux, B. (2019). The value of tax progressivity: Evidence from survey experiments. *Journal of Public Economics*, 179:104068.
- Tax Foundation (2024). Property taxes by State & County, 2024. *Tax Foundation Research*. Accessed: 2024-12-10.
- Tyran, J.-R. and Sausgruber, R. (2006). A little fairness may induce a lot of redistribution in democracy. *European Economic Review*, 50(2):469–485.
- World Bank (2025). PPP conversion factor, GDP (LCU per international \$). Retrieved February 9, 2025.
- Zheng, X., Xia, Y., Hui, E. C., and Zheng, L. (2018). Urban housing demand, permanent income and uncertainty: Microdata analysis of Hong Kong’s rental market. *Habitat International*, 74:9–17.

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



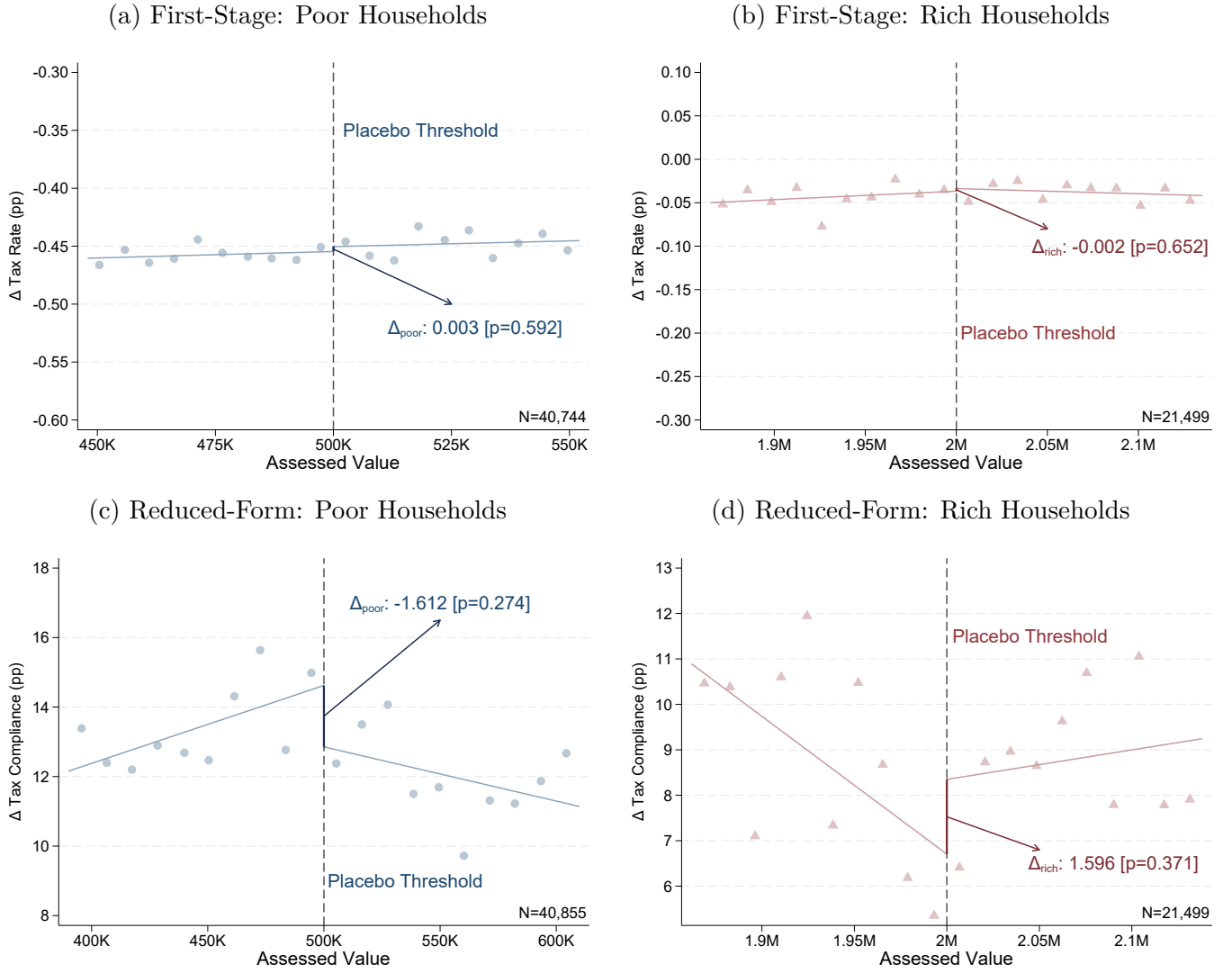
Note: This figure calculates the own-rate effects of the progressive property tax reform in 2023, using a two-cutoff RDD. The left figures (in blue) show the results for the poor at the cutoff of AR\$ 750K. The right figures (in red) show the results for the rich at the cutoff of AR\$ 1.5M. The running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



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.

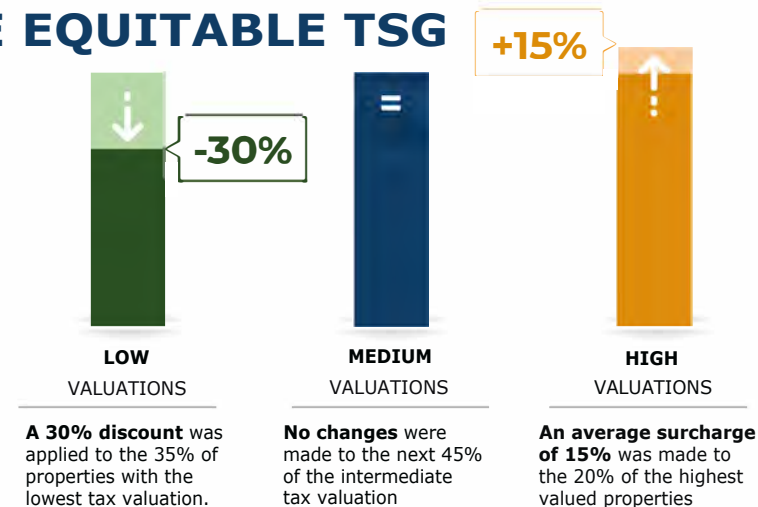
(b) Treatment Letter Message

FAIRER AND MORE EQUITABLE TSG

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

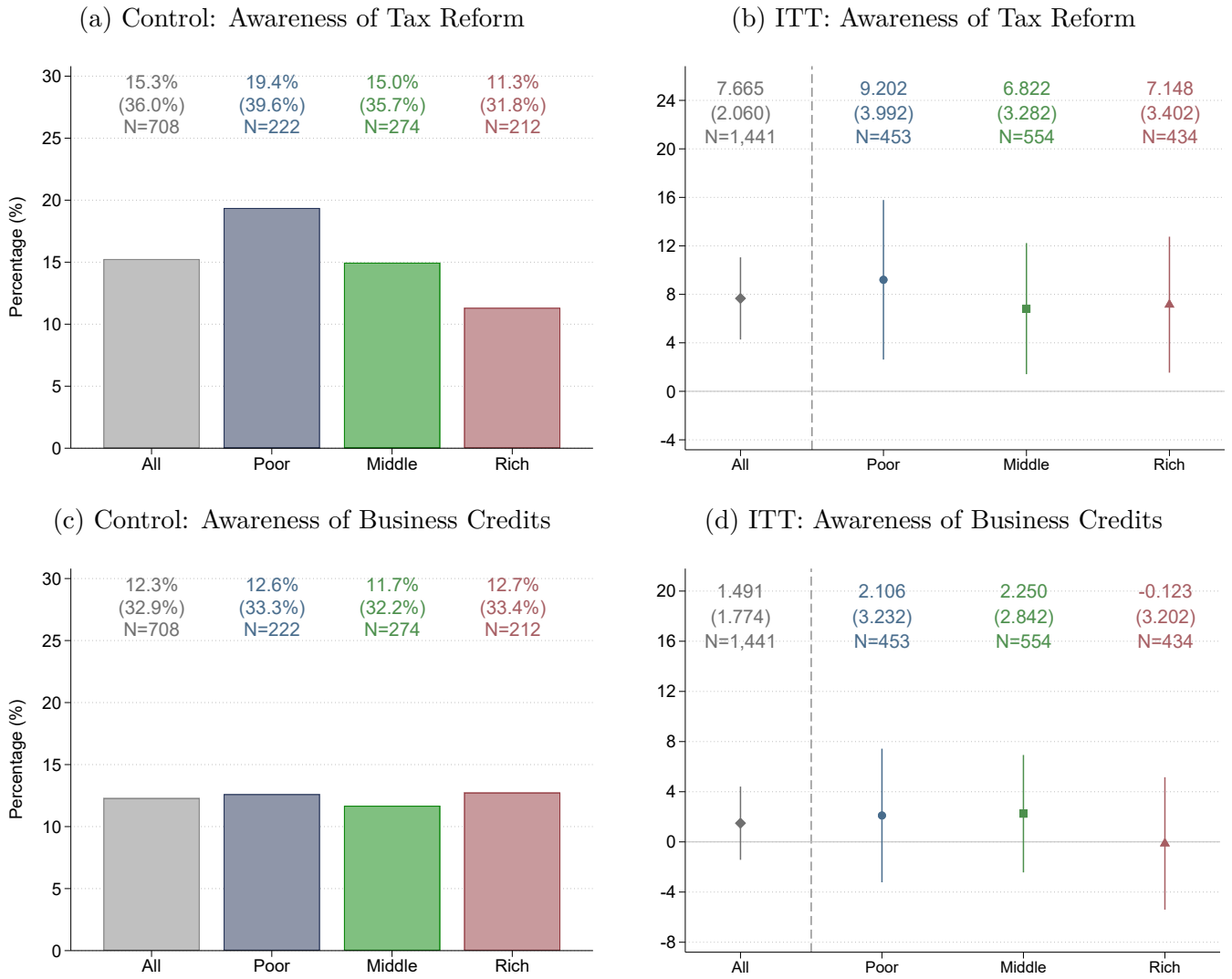
We applied a 30% discount on the variable component of the TSG for properties with the lowest tax valuation, and an average increase of 15% for those with the highest value.

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



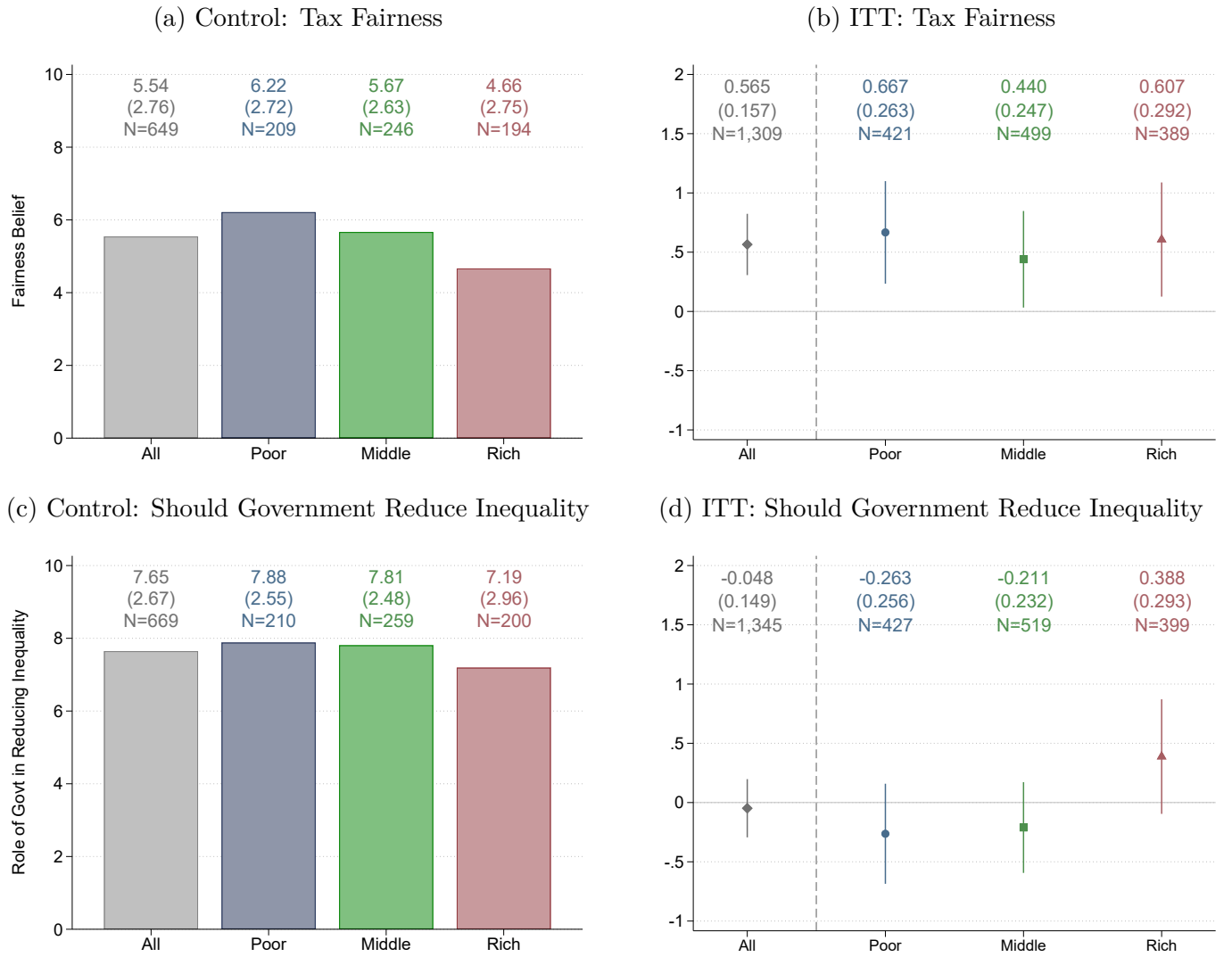
Note: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained on the second page of the mailer (for a full sample of the mailer, see Appendix L.1). TSG refers to *Tasa por Servicios Generales*, the local tax levied by the municipality.

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 credits. The vertical spikes denote 90% confidence intervals. The figure also presents the means (left panels) and treatment effects and their standard errors (right panels).

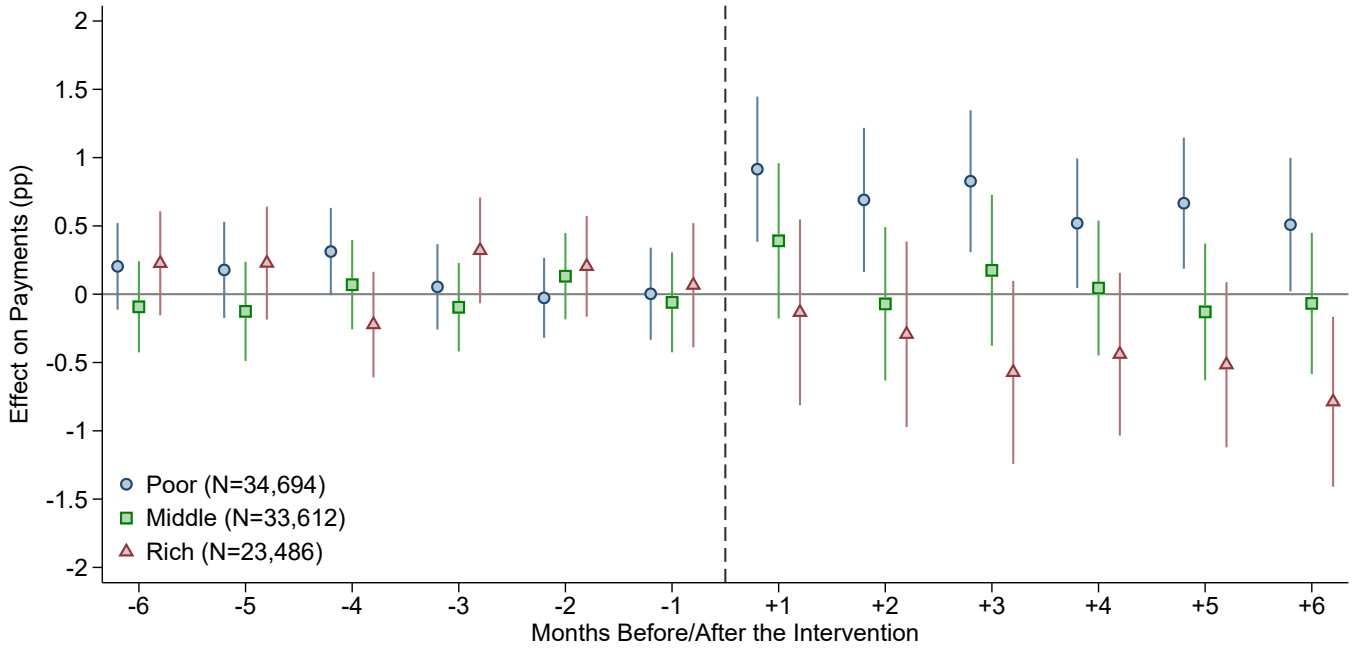
Figure 6: The Effect on Fairness Perceptions: Survey Experiment



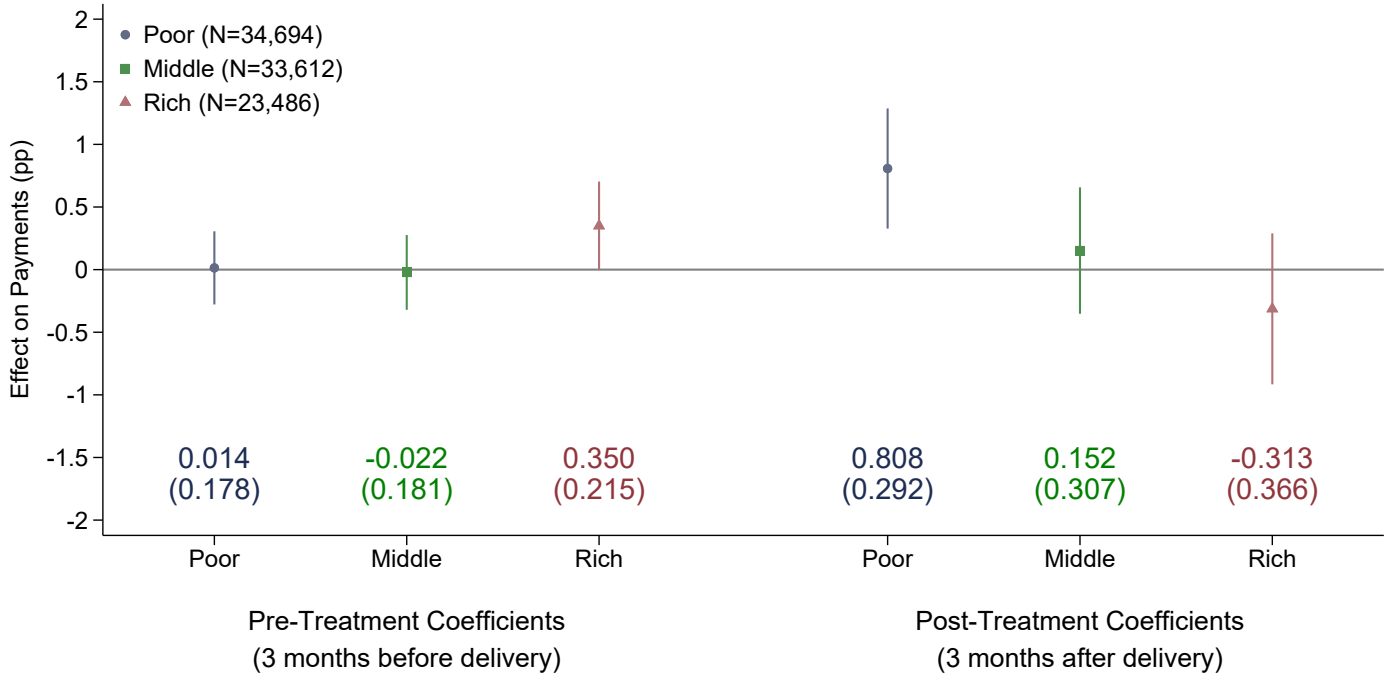
Note: This figure uses data from our online survey on taxpayers to estimate the impact of our progressivity information experiment on taxpayers' fairness perceptions. We asked respondents how fair they considered the distribution of the municipal tax rates between rich and poor households, on a scale 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% 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)

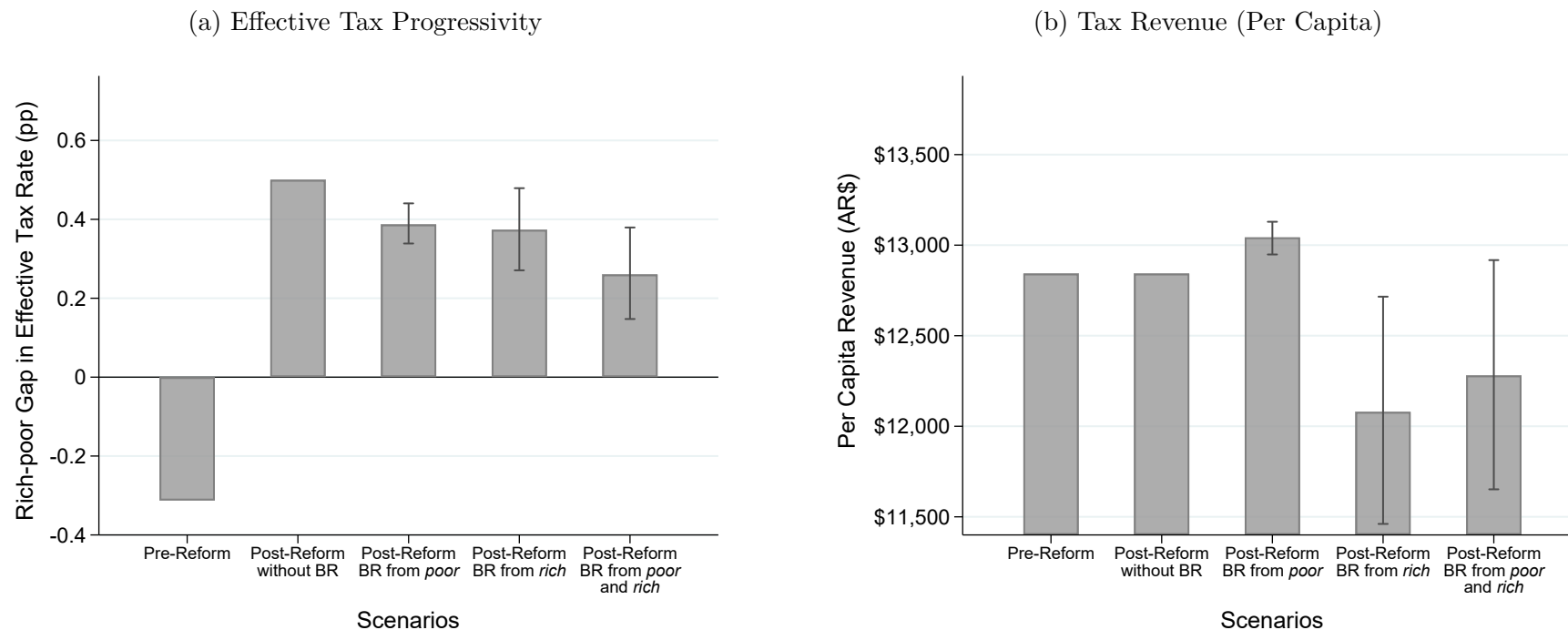


(b) Pre- and Post-Treatment Effects (Quarterly level)



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 estimated the coefficient for each monthly bill in separate regressions, including the 90% confidence interval for each group. We reported clustered standard errors for the pooled estimates in parentheses. The sample includes residential and non-residential properties and excludes units that made payments for 2023 before we sent the letters. We controlled for the past 12 months payments in each pre-treatment regression, and for the 2022 monthly payments in each post-treatment regression. Additionally, we included controls for property valuation (in logs), a residential indicator, indicators for 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



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.