Online Appendix (For Online Publication Only)

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

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

June 12, 2025

A Details about the Institutional Context

A.1 Local Property Tax

Tres de Febrero is a major urban municipality in the Greater Buenos Aires metropolitan area, Argentina.[51] Our study focuses on a local property tax known as *Tasa por Servicios Generales* (TSG), which funds public services such as public spaces, street lighting, urban sanitation, security, and the fire department, among others.[52] This type of tax is common across all Argentine municipalities and serves as the primary source of local revenue. Tres de Febrero is no exception, with the TSG accounting for 20% of its total resources and 45% of its own-source revenue in 2021.

TSG bills are issued monthly and are due during the first weeks of each billing period.[53] Alternatively, residents have the option of making an advance annual payment in the first months of the year instead of monthly installments.

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

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

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

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

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

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

Agency of the Province of Buenos Aires (ARBA).

The fixed components of the tax correspond to specific services, including security, health, fire departments, and maintenance of public spaces. These components are measured in 'modules,' with each module valued at AR$ 10 in 2022 (see Table A.2). While cadastral values are seldom updated, both the variable and fixed charges are adjusted annually through a module factor.[54]

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

A.2 The Progressive Tax Reform

For political and equity reasons, the municipality implemented progressive reforms to the municipal property tax during 2022-24. The 2022 and 2023 reforms modified the *variable* component of equation (A.1), while the 2024 reform transformed the previously uniform *fixed* component into a group-specific charge. Each reform used assessed property value thresholds to determine eligibility.[55]

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

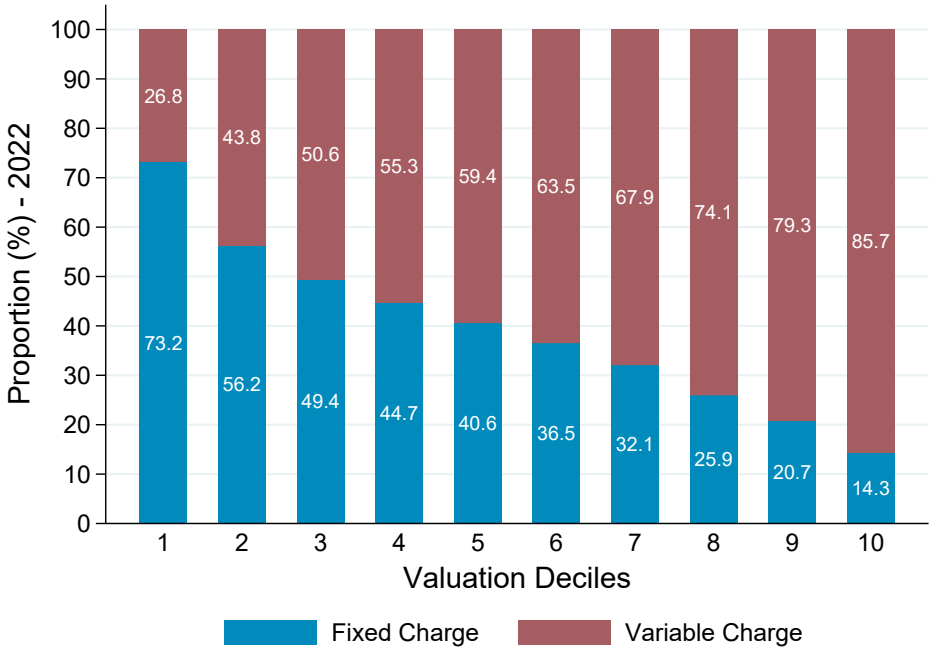
Note: This table reports statutory tax rates by category of property as defined in the tax code (*Ordenanza Impositiva*) every year. The first two columns display the number of properties in 2018. The tax base used to calculate the monthly fee corresponds to the assessed value in 2018 provided by the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

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

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

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

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



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

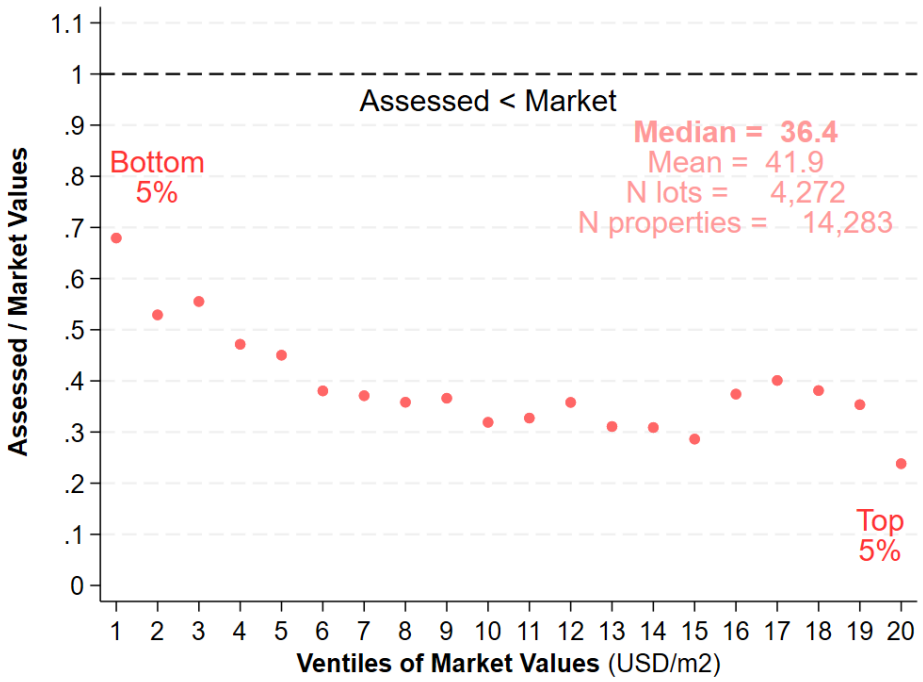
B Market vs. Assessed Property Values

In this section, we used proprietary data on real estate prices to document that assessed values are systematically below market values. Moreover, the degree of under-assessment is slightly weaker for properties at the bottom of the distribution, which constitutes an additional source of tax regressivity.

We matched cadastral records from the municipal registry with real estate listings from *Properati*, a major online platform in Argentina. We began with a universe of 54,371 listings and applied a series of filters to ensure data quality and comparability. First, we retained only listings for sale in U.S. dollars (excluding rentals and listings in pesos), removed observations with missing or implausibly low prices, and excluded entries without valid geolocation. We then performed a geographic match between listings and cadastral parcels using spatial proximity, and focused on the strictest criterion—where the listing's coordinates fell exactly within the parcel polygon (distance = 0 meters). We restricted the analysis to residential properties or those with mixed residential and commercial use, yielding a final matched sample of 14,172 observations (26.07% of the original listings).

For each matched property, we computed both assessed values (AV) and market values (MV) in USD per square meter, enabling direct comparisons across units. Figure B.1 shows the distribution of AVs and MVs for the matched sample. The figure reveals a substantial and systematic gap: assessed values are consistently and significantly lower than market values. This finding supports anecdotal accounts of widespread fiscal under-assessment in this context, likely driven by infrequent updates to cadastral valuations by the provincial tax authority. The under-assessment is not uniform across the value distribution; it is less pronounced in the bottom two deciles, introducing an additional source of effective tax regressiveness.

Figure B.1: Comparison of Assessed Values vs. Market Values



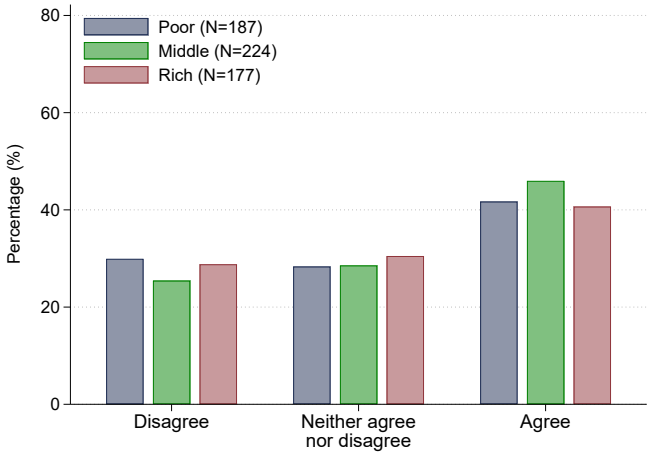
Note: This figure displays the ratio of assessed to market property values across ventiles of the market value distribution. Assessed values correspond to 2018 valuations from the provincial cadastral agency (ARBA). Market values are taken from Properati listings posted between 2020 and 2023, where prices are reported directly in USD on the website. Both values are converted to USD per square meter, to make them directly comparable. The sample is restricted to properties with exact or near-exact geospatial matches (within zero meters of distance) to cadastral parcels.

C Support for Redistribution among Survey Respondents

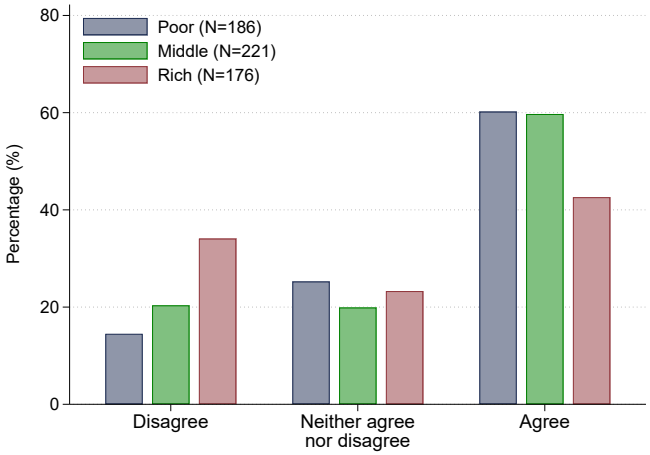
Figure C.1 presents some descriptive statistics from the survey, specifically about the taxpayers' preferences for redistribution. Panel (a) shows support for reducing taxes on low-valuation properties, with respondents evaluating whether lower taxes for the poorest households would be justified despite reducing available funds for public services, while panel (b) examines support for increasing taxes on high-valuation properties, asking whether the richest should contribute more to increase public revenue. The results indicate that while a substantial share of respondents agrees with lower taxes for the poor, support is even stronger for raising taxes on the rich. Panel (c) presents responses to a separate question, where participants were asked whether they support a simultaneous tax cut for the poorest and a tax increase for the richest, keeping overall public revenue unchanged, showing that the majority of taxpayers favor a more progressive tax system overall.

Figure C.1: Support for Different Changes in the Tax

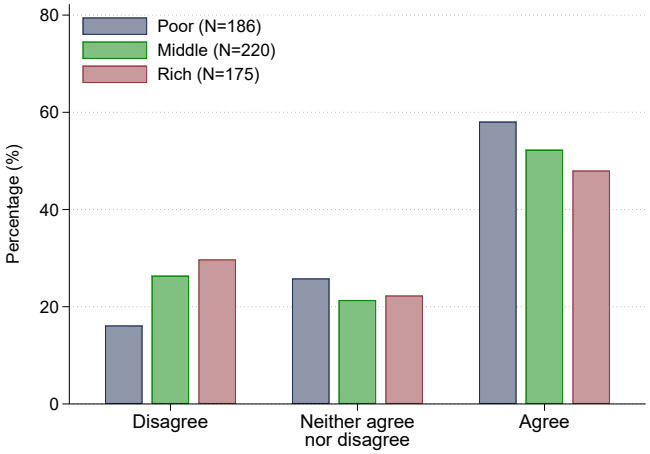
(a) Lower Tax for Poor



(b) Higher Tax for Rich



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



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

D Robustness Checks

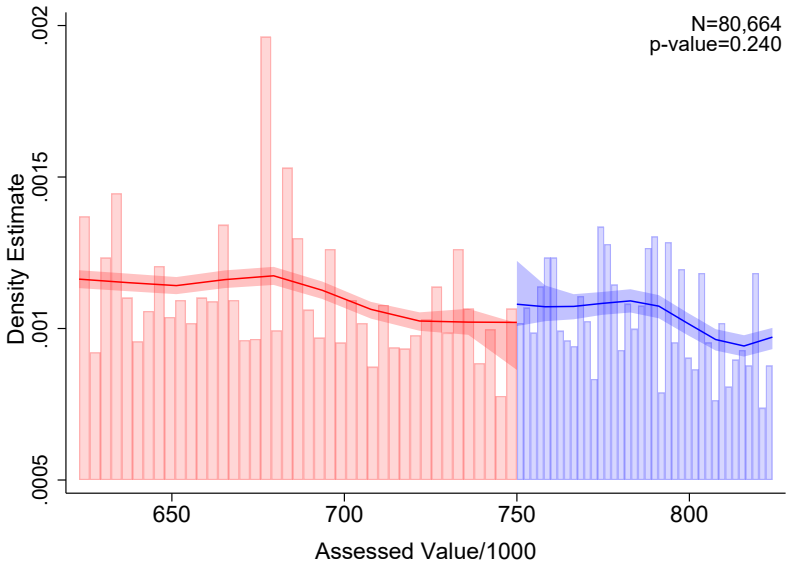
D.1 Manipulation Test

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

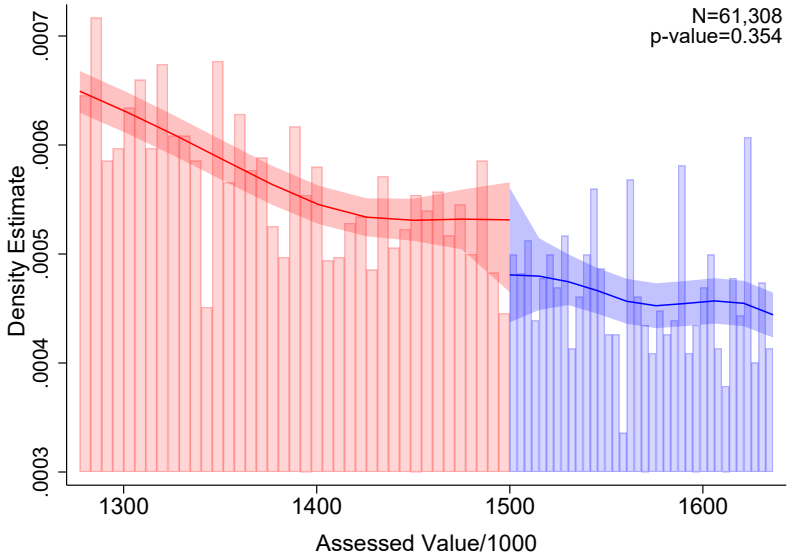
The McCrary density test and accompanying figures revealed no evidence of unusual bunching around the thresholds that determine treatment eligibility. While we detected a slight discontinuity for poor properties, the institutional context suggests this is unlikely to reflect deliberate manipulation.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

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

(a) Poor Cutoff - 2023



(b) Rich Cutoff - 2023



Note: Panels (a) and (b) show the McCrary (2008) manipulation test (with their corresponding p-value) of the property assessed values at the low-value threshold (poor) and the high-value threshold (rich) in 2023. The shaded areas represent the 90% confidence intervals. The x-axis represents assessed property value in AR$ thousands.

D.2 Alternative Specifications

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

Tables D.1 and D.2 present multiple robustness checks for the RDD analysis used to estimate own-rate effects. These checks incorporate: (i) additional pre-treatment controls, (ii) including just households in the sample, (iii) alternative definitions of the outcome variable, (iv) including in the sample only those households which received a control or treatment letter, (v) the use of an uniform kernel (rather than a triangular kernel), (vi) the use of an epanechnikov kernel, (vii) the use of a zero degree polynomial and (viii) the use of a second degree polynomial. The estimates remain stable across these specifications, supporting the validity of the main findings.

Table D.3 focuses on robustness checks for the experimental analysis used to estimate cross-rate effects. These tests examine the sensitivity of the results to: (i) including only pre-treatment payment controls, (ii) incorporating the email sample into the regressions, (iii) including just households in the sample, (iv) using an alternative definition of the outcome, and (v) including the observations of those taxpayers who made up-front payments for 2023 before the letters were sent. The findings remain stable across all specifications.

Finally, Figure D.2 displays the placebo exercise done in Figure 3, but using different thresholds for our placebo. Our results are generally robust to the selection of the threshold.

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

Table D.1: Robustness Tests: Own-Rate Effects I

	Robustness Checks for RDD Results					
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of Reform on Tax Compliance:						
Low-Valuation HHs	−2.718***	−1.165*	−2.515***	−2.597***	−3.931***	−1.358
	[0.002]	[0.082]	[0.005]	[0.003]	[0.001]	[0.240]
					[pval diff.=0.142]	
High-Valuation HHs	−2.248*	−1.364	−2.800**	−2.502**	−1.444	−3.308*
	[0.050]	[0.123]	[0.023]	[0.024]	[0.569]	[0.064]
					[pval diff.=0.320]	
Effect of Reform on Tax Rates:						
Low-Valuation HHs	0.392***	0.390***	0.366***	0.392***	0.400***	0.384***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
					[pval diff.=0.246]	
High-Valuation HHs	0.146***	0.145***	0.126***	0.146***	0.144***	0.148***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
					[pval diff.=0.706]	
Specification						
Additional Controls		✓				
Just Households			✓			
Alternative outcome (6 months of late payment)				✓		
Just Control Sample					✓	
Just Treatment Sample						✓
Observations (Compliance)						
Low-Valuation HHs	29,180	31,365	28,208	33,393	30,821	30,487
High-Valuation HHs	61,308	61,308	54,648	61,308	15,009	16,243
Observations (Rates)						
Low-Valuation HHs	13,017	12,756	15,934	13,017	30,821	30,487
High-Valuation HHs	61,308	61,308	54,648	61,308	7,067	7,411
Average Tax Compliance (Baseline)						
Low-Valuation HHs	60.4%	60.1%	60.9%	60.2%	57.2%	56.7%
High-Valuation HHs	57.4%	57.4%	58.3%	57.4%	60.5%	60.0%
Average Tax Rate (Baseline)						
Low-Valuation HHs	2.2%	2.2%	2.1%	2.2%	1.8%	1.9%
High-Valuation HHs	1.8%	1.8%	1.7%	1.8%	2.2%	2.2%

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

Table D.2: Robustness Tests: Own-Rate Effects II

	Robustness Checks for RDD Results				
	(1)	(2)	(3)	(4)	(5)
Effect of Reform on Tax Compliance:					
Low-Valuation HHs	−2.718***	−3.313***	−2.860***	−2.412***	−3.011***
	[0.002]	[0.001]	[0.001]	[0.004]	[0.003]
High-Valuation HHs	−2.248*	−2.071*	−2.181*	−1.878*	−2.538*
	[0.050]	[0.069]	[0.057]	[0.077]	[0.070]
Effect of Reform on Tax Rates:					
Low-Valuation HHs	0.392***	0.389***	0.392***	0.383***	0.395***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
High-Valuation HHs	0.146***	0.147***	0.147***	0.133***	0.152***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Specification					
Uniform Kernel		✓			
Epanechnikov Kernel			✓		
Zero Degree Polynomial				✓	
Second Degree Polynomial					✓
Observations (Compliance)					
Low-Valuation HHs	29,180	22,837	27,215	11,663	44,615
High-Valuation HHs	61,308	61,308	61,308	61,308	61,308
Observations (Rates)					
Low-Valuation HHs	13,017	18,087	12,589	13,718	24,529
High-Valuation HHs	61,308	61,308	61,308	61,308	61,308
Average Tax Compliance (Baseline)					
Low-Valuation HHs	60.4%	60.8%	60.5%	60.6%	60.1%
High-Valuation HHs	57.4%	57.3%	57.3%	57.5%	57.5%
Average Tax Rate (Baseline)					
Low-Valuation HHs	2.2%	2.2%	2.2%	2.2%	2.2%
High-Valuation HHs	1.8%	1.8%	1.8%	1.9%	1.9%

Note: This table summarizes Figure 1 using different specifications for the estimates. Column (1) shows the main estimates, column (2) shows the estimates using an uniform kernel rather than a triangular one, column (3) uses an epanechnikov kernel rather than a triangular one, column (4) uses a zero degree polynomial and column (5) uses a second degree polynomial. Each column indicates the estimated effect around the cut-off point, where the p-value, taken from a robust bias-corrected inference, is indicated in brackets.

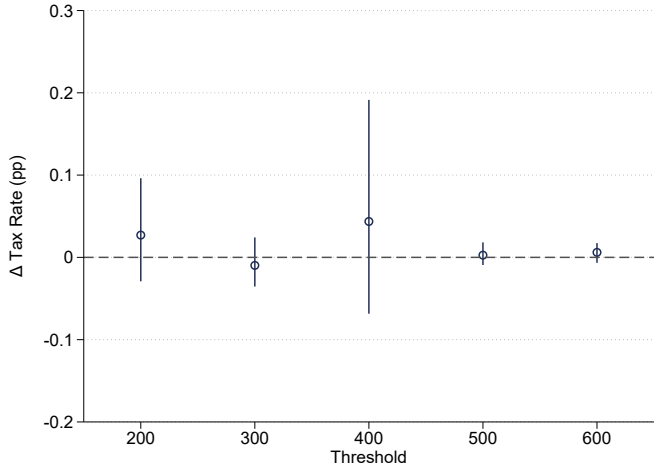
Table D.3: Robustness Tests: Cross-Rate Effects

	Robustness Checks for Experimental Results					
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of Reform on Tax Compliance:						
Low-Valuation HHs	0.808***	0.795***	0.698**	0.756**	0.570**	0.741***
	(0.292)	(0.295)	(0.278)	(0.304)	(0.290)	(0.279)
Medium-Valuation HHs	0.152	0.170	0.132	0.247	0.166	0.157
	(0.307)	(0.309)	(0.286)	(0.320)	(0.304)	(0.298)
High-Valuation HHs	−0.313	−0.421	−0.245	−0.177	−0.254	−0.288
	(0.366)	(0.369)	(0.341)	(0.418)	(0.367)	(0.360)
Specification						
Just pre-treat payment controls		✓				
Email Sample Included			✓			
Just Households				✓		
Alternative outcome (6 months of late payment)					✓	
Up-Front Payers Included						✓
Observations						
Low-Valuation HHs	34,694	34,694	38,678	31,999	34,694	36,519
Medium-Valuation HHs	33,612	33,612	38,325	30,945	33,612	34,819
High-Valuation HHs	23,486	23,486	26,716	17,945	23,486	24,022
Average Tax Compliance (Baseline)						
Low-Valuation HHs	54.5%	54.5%	57.3%	54.5%	54.5%	56.8%
Medium-Valuation HHs	56.0%	56.0%	59.1%	56.5%	5.06%	57.5%
High-Valuation HHs	56.1%	56.1%	59.0%	57.1%	56.1%	57.0%

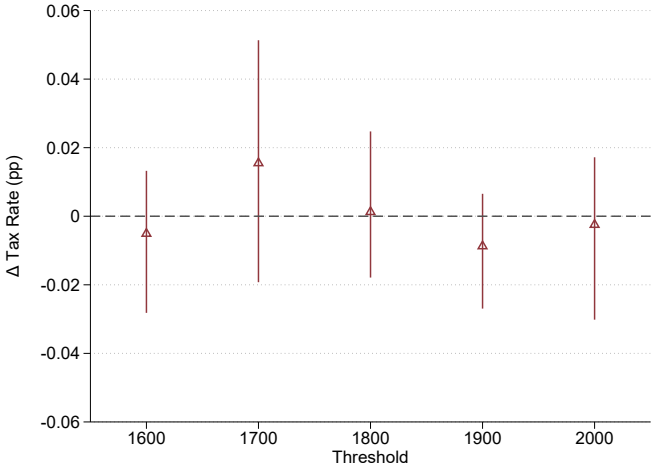
Note: This table summarizes Figure 7 using different specifications for the estimates. Column (1) shows the main estimates, column (2) excludes every other control not related to pre-treatment payments, column (3) includes the e-mail survey sample, column (4) includes only residential properties in the sample (excludes non-residential properties), column (5) uses an alternative outcome that includes the possibility of paying 6 months after the due date instead of only 3 months later, and column (6) includes the observations of those taxpayers who made up-front payments for 2023 before the letters were sent. Standard errors clustered at the individual level in parenthesis.

Figure D.2: RDD Falsification Test: Full Distribution of Placebo Thresholds for 2023

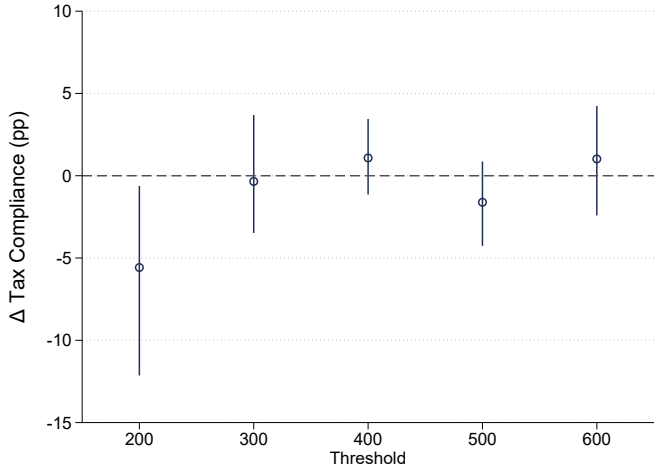
(a) First-Stage: Poor Households



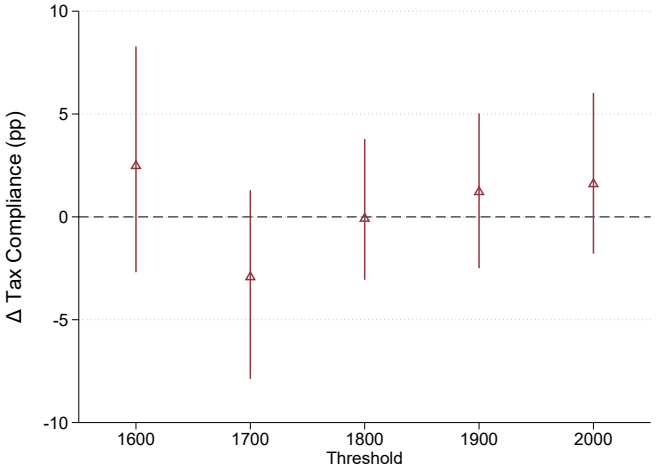
(b) First-Stage: Rich Households



(c) Reduced-Form: Poor Households



(d) Reduced-Form: Rich Households



Note: This figure calculates the own-rate effects using different placebo thresholds for poor and rich households. The x-axis shows the threshold used in each estimation. Point estimates and their confidence intervals (at a 90% confidence level) are displayed. 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.

E Balance Tables

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

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

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

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

Table E.1: Balance Table: 2023 Experiment

		All			Poor			Middle			Rich		
	All	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value
Share Household (%)	88.1	88.3	88.0	0.168	92.3	92.1	0.491	92.0	92.1	0.855	76.8	76.1	0.199
	(32.4)	(32.2)	(32.5)		(26.6)	(26.9)		(27.1)	(27.0)		(42.2)	(42.7)	
Share of Payments (2021) (%)	44.0	44.2	43.9	0.294	42.4	41.9	0.306	44.7	45.0	0.530	46.1	45.1	0.116
	(45.8)	(45.8)	(45.8)		(45.4)	(45.5)		(45.7)	(45.8)		(46.4)	(46.1)	
Share of Payments (2022) (%)	44.0	44.2	43.8	0.236	42.1	41.6	0.299	44.7	44.9	0.733	46.4	45.6	0.134
	(45.3)	(45.3)	(45.4)		(44.9)	(44.9)		(45.3)	(45.4)		(45.8)	(45.8)	
2023 Assessed Value (million $s)	129.8	127.8	131.8	0.296	47.5	47.4	0.814	105.5	105.8	0.169	279.4	292.5	0.381
	(587.5)	(391.8)	(733.7)		(18.9)	(18.8)		(21.1)	(21.3)		(754.9)	(1,431.9)	
2022 Tax Rate (%)	2.80	2.80	2.80	0.87	4.00	4.01	0.96	2.08	2.08	0.73	2.06	2.07	0.52
	(4.34)	(4.20)	(4.49)		(6.61)	(7.09)		(0.59)	(0.50)		(0.79)	(0.81)	
Share Poor (%)	37.8	37.7	37.8	0.751	-	-	-	-	-	-	-	-	-
	(48.5)	(48.5)	(48.5)										
Share Rich (%)	25.6	25.4	25.8	0.201	-	-	-	-	-	-	-	-	-
	(43.6)	(43.5)	(43.7)										
Observations	91,792	46,082	45,710		17,394	17,300		16,982	16,630		11,706	11,780	

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

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

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

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

Table E.3: Balance Table: 2024 Experiment

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

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

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

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

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

Table E.5: Balance Table: Complementary Survey

		All			Poor			Middle			Rich		
	All	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value	Control	Treat	P-value
Share Household (%)	93.0	91.9	94.1	0.081	95.7	97.1	0.381	96.7	97.0	0.874	81.7	87.6	0.060
	(25.5)	(27.2)	(23.6)		(20.4)	(16.8)		(17.8)	(17.2)		(38.7)	(33.0)	
Share of Payments (2021) (%)	81.5	80.3	82.7	0.124	77.9	82.0	0.176	81.4	83.0	0.528	81.2	83.2	0.483
	(32.6)	(33.8)	(31.4)		(35.8)	(33.0)		(32.5)	(30.5)		(33.4)	(30.9)	
Share of Payments (2022) (%)	84.1	83.7	84.5	0.536	82.3	87.7	0.023	84.7	82.3	0.285	83.7	84.0	0.898
	(28.8)	(28.8)	(28.8)		(29.6)	(25.2)		(27.6)	(30.6)		(29.6)	(29.8)	
2023 Assessed Value (million $s)	135.0	132.1	137.7	0.268	51.5	52.8	0.346	107.1	105.7	0.399	246.9	261.0	0.162
	(105.6)	(95.4)	(114.4)		(15.9)	(15.5)		(21.5)	(21.1)		(95.2)	(132.1)	
2022 Tax Rate (%)	2.279	2.315	2.246	0.183	3.109	2.927	0.193	1.997	2.001	0.782	1.941	1.858	0.131
	(1.079)	(1.182)	(0.969)		(1.803)	(1.383)		(0.236)	(0.210)		(0.651)	(0.603)	
Share Poor (%)	30.7	30.1	31.3	0.592	-	-	-	-	-	-	-	-	-
	(46.1)	(45.9)	(46.4)										
Share Rich (%)	30.6	29.9	31.3	0.521	-	-	-	-	-	-	-	-	-
	(46.1)	(45.8)	(46.4)										
Observations	1,723	844	879		254	275		338	329		252	275	

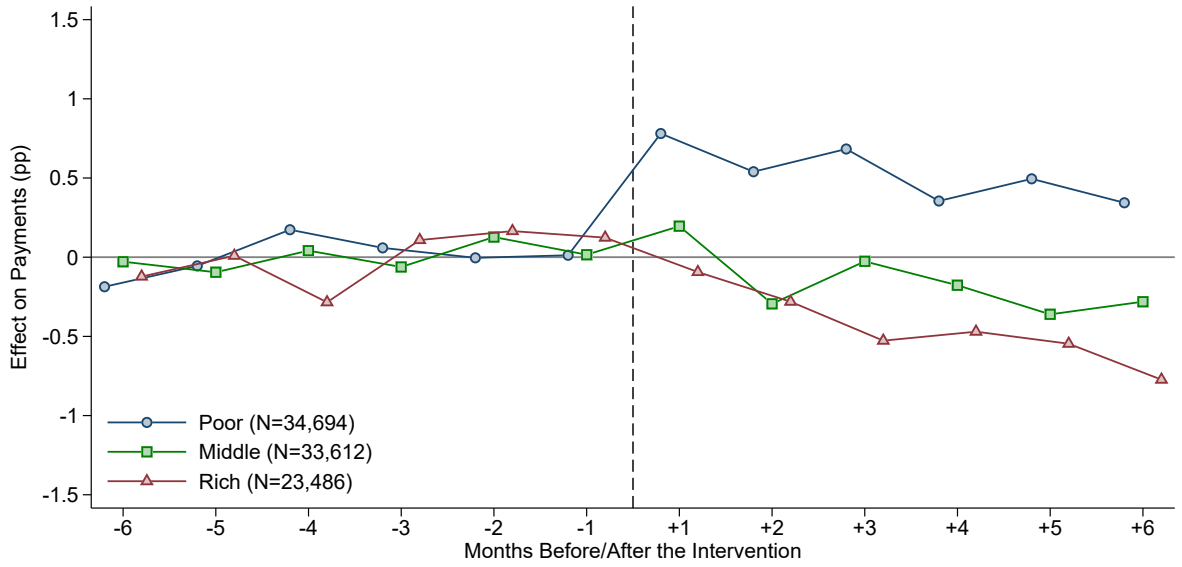
Note: This balance table presents the difference between control and treatment groups across different categories (All, Poor, Middle, and Rich) for our survey experiment. Standard deviation in parenthesis. The p-value columns reflect the results of difference tests between control and treatment groups.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

F Additional Experimental Results

F.1 Differences in Payment Rates

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

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



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

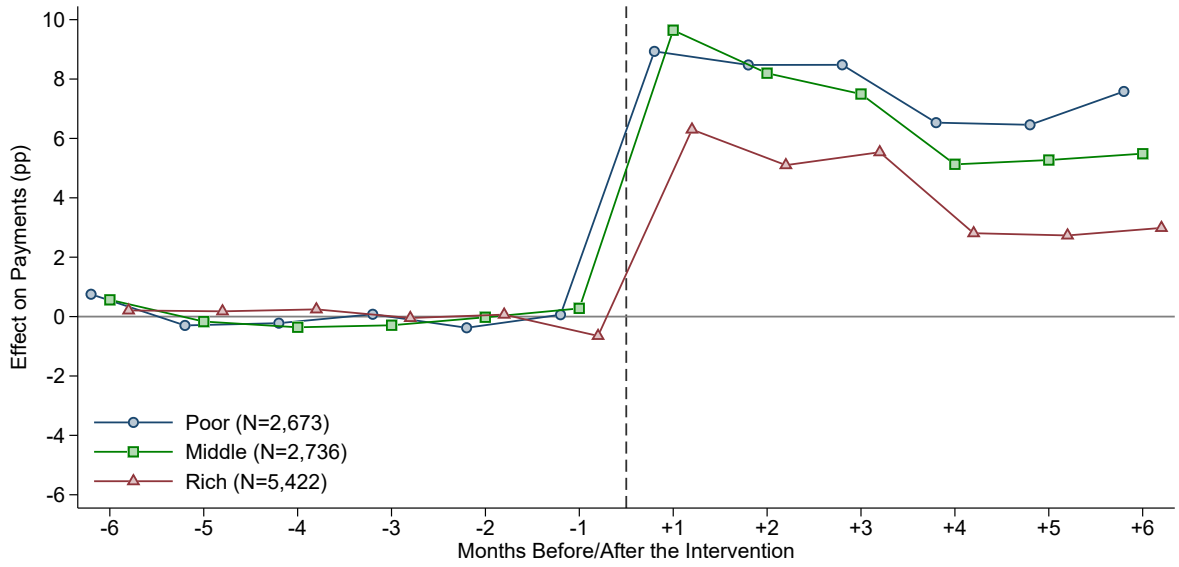
F.2 Control Letter vs No Letter

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

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

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

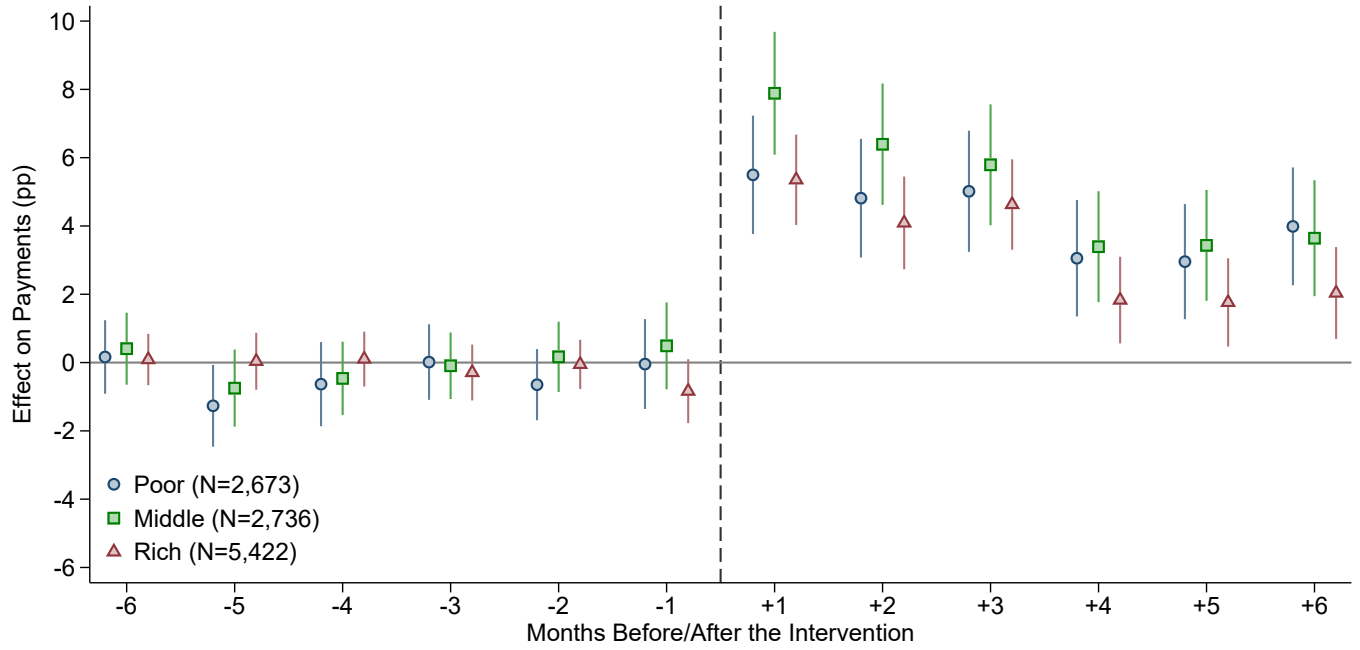
Figure F.2: Difference in Payment Rates between Control and No-Letter Groups in 2023



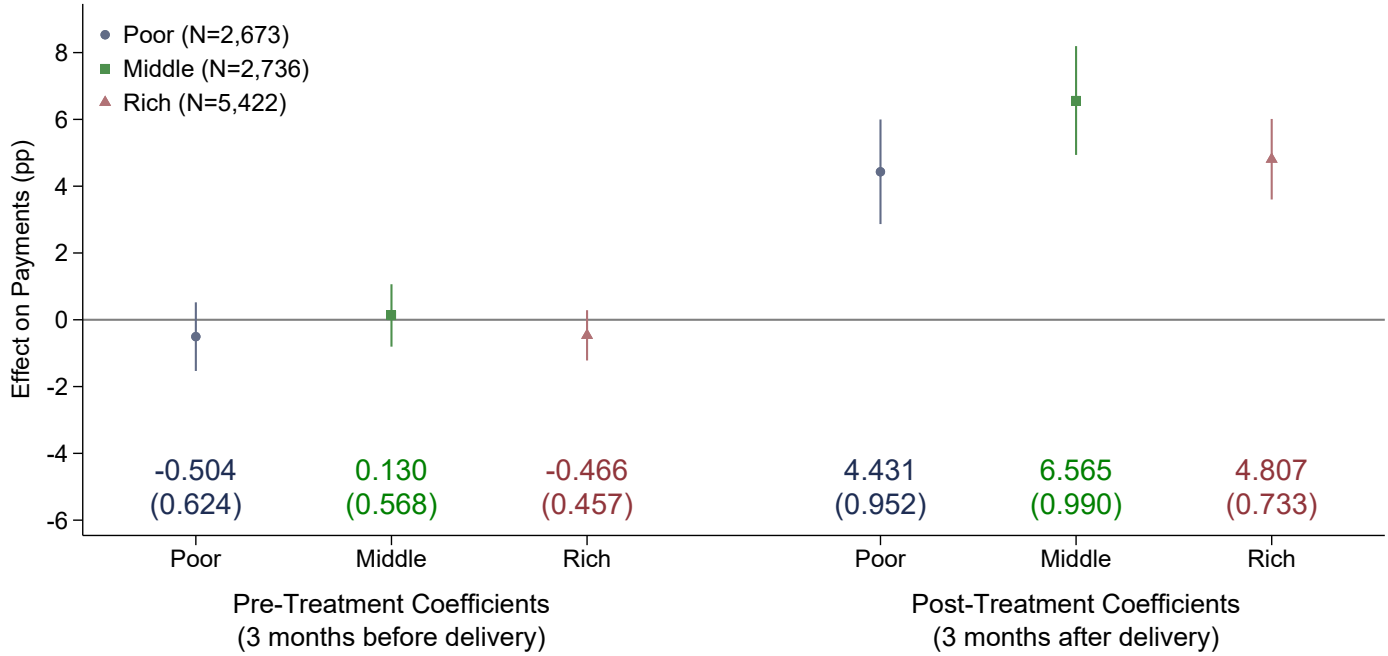
Note: This figure compares the likelihood of paying the monthly bill between treatment and control, normalized by the average pre-treatment difference. We estimated the coefficient for each monthly bill in separate regressions. The sample includes residential and non-residential properties and excludes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

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

(a) Event-Study Analysis (Monthly Level)



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



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

F.3 Experimental Uptake: Alternative Estimates of Reading Rates

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

In our context, we can use the evidence on awareness of the reform from the survey sample. Specifically, Figure 5 (panels (a) and (b)) present the treatment effect on the probability of knowing about the reform, and we can use this treatment effect as a proxy for experimental uptake. If 19.4% of poor households in the control group were aware of the reform and 100% in the treatment group were exposed to the information, we would expect awareness to increase by 80.6 pp due to our treatment (from 19.4% to 100%). However, we identified an effect of approximately 9.2 pp (panel (b)), implying a reading rate or uptake of 11.4% ($\frac{9.2}{80.6}$), or a non-uptake adjustment factor of around 8.8.

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

G Comparison of Sample Subgroups

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

Table G.1: Summary Statistics by Sample Group

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

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

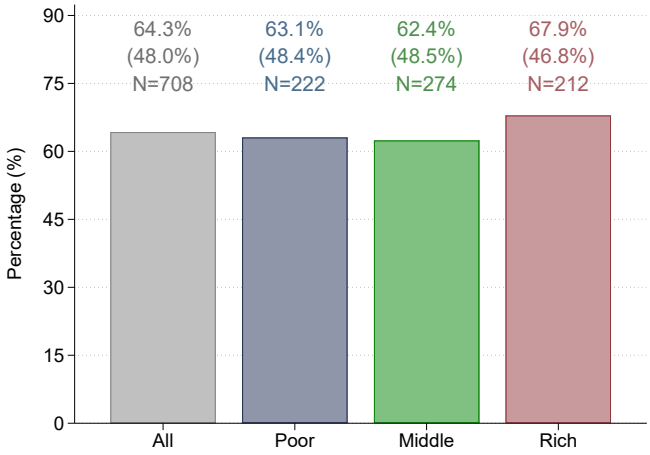
H Survey Questionnaire Additional Placebos

In this appendix, we present additional results from the survey experiment, focusing on placebo outcomes designed to test the specificity of the treatment effects. In particular, we examined self-reported awareness of two additional municipal policies that were unrelated to the progressive tax reform. These policies were mentioned in both control and treatment letters, and thus serve as placebo outcomes.

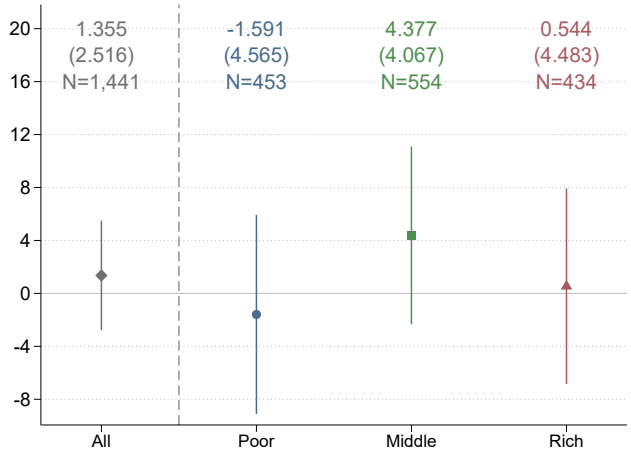
The results are presented in Figure H.1. The two left panels correspond to the free and express commercial permits, while the two right panels correspond to the simplified paperwork for new businesses. Panels (a) and (c) of Figure H.1 show the proportion of respondents in the control group who reported being aware of each of these policies. Awareness is relatively high, which may stem from organic familiarity with the policies or simply from the fact that all letters—including the control letters—publicized them. Most importantly, panels (b) and (d) present the treatment effects on these two placebo outcomes. In panel (b), we find that—consistent with the placebo outcome results reported in Figure 5—the coefficients are close to zero and statistically insignificant. In panel (d), the results are somewhat more mixed, as one of the coefficients—the leftmost one, corresponding to the full sample—is statistically significant. However, our preferred interpretation is that this is a spurious finding: given the large number of placebo analyses reported in the paper, it is expected that a few may appear statistically significant purely by chance.

Figure H.1: Taxpayers' Awareness of Placebo Reforms: Survey Experiment

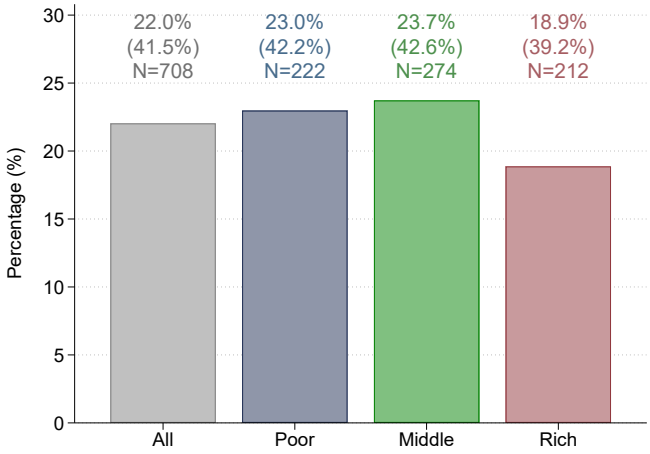
(a) Control: Awareness of New Commercial Permits



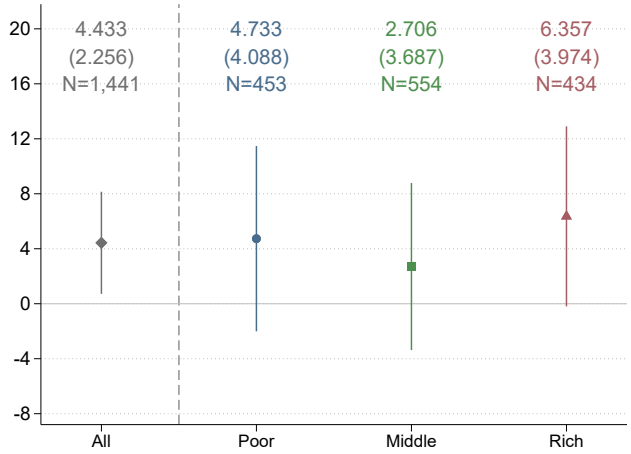
(b) ITT: Awareness of New Commercial Permits



(c) Control: Awareness of Reduced Paperwork



(d) ITT: Awareness of Reduced Paperwork



Note: This figure uses data from our online survey on taxpayers to assess awareness of two placebo reforms, implemented by the municipality and shown to both control and treatment letters. 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 Tres de Febrero free and express commercial permits and on the knowledge of the Tres de Febrero reduced paperwork for new businesses. The vertical spikes denote 90% confidence intervals.

I Replication of the 2023 Findings in 2024

I.1 Experimental Design in the 2024 Reform

The municipality implemented a second progressive tax reform in 2024. We re-randomized households into treatment and control letters within each of the property value groups—that is, we did not keep the treatment status of 2023 in 2024.[56] A minor difference for the 2024 experiment with respect to 2023 is that we excluded from the treatment and control letters the "empty lot", "civil entities", "religious entities" and "wholesale establishments" categories (around 1% of the sample). In the 2024 design, we also included within each of the poor, middle, and rich households groups a small subsample of households that did not receive any letter (3.6% of the total) to gauge the pure effect of receiving a letter (see results from this exercise in Appendix I.4)—in 2024 we included no letter households across the whole sample, whereas the analysis of no letter in 2023 in Appendix F.2 was limited to commercial properties only. Finally, we excluded from the mailing experiment sample the 17.35% of the sample that had a registered email with the Municipality, and we reserved this sample for a survey about perception of taxes and rates which we did not want to contaminate with the information from the treatment and control letters.

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

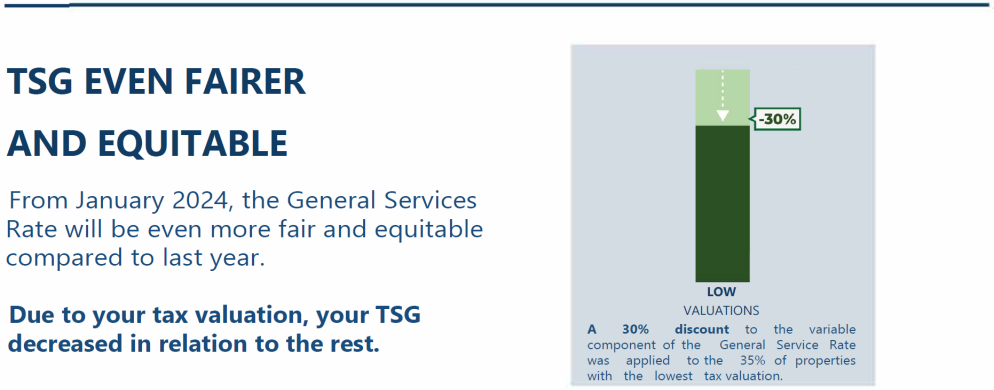
An example of the treatment and control infographic for poor households is shown in Figure I.1, while Appendix L.2 provides a full sample letter along with the treatment and control infographics for middle and rich households. One important detail is the information about the discounts and surcharges.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

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

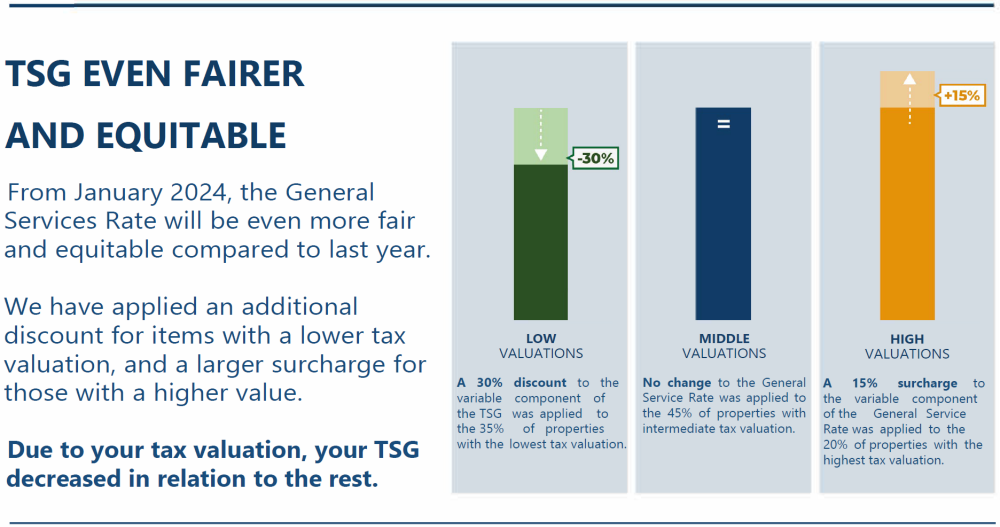
fixed component adjustment introduced in 2024. The justification for this approach was that when considering the total tax liability from 2022 to 2024, the cumulative effect of the reforms translated into an approximate 22.3% reduction for poor households and a 17.7% increase for rich households. Since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

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

(a) Control Letter Message



(b) Treatment Letter Message



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

I.2 Own-Rate Effects in the 2024 Reform

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

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

Finally, we replicated the elasticities estimated in equations (1) and (2) using data from 2024. 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. Both elasticities are expressed in terms of two-month to two-month changes (given dataset limitations in 2024), comparing periods before and after the reform. For each elasticity shown below, we report 90% confidence intervals in brackets, calculated with 5,000 bootstrap iterations:

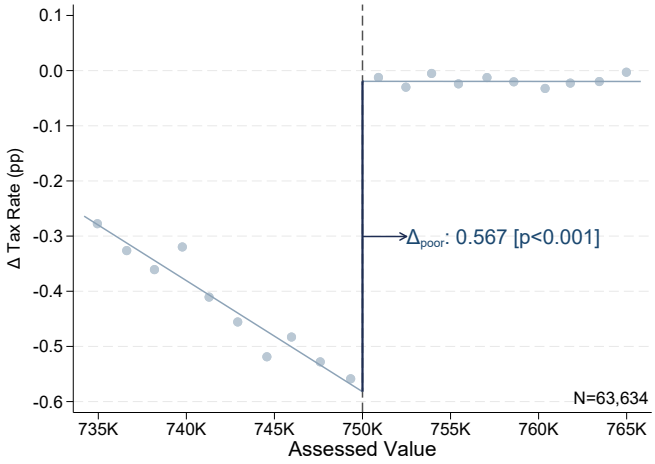
$$\varepsilon_{poor}^{own} = \frac{\frac{\beta_{poor}^{own}}{C_{poor}^{own}}}{\frac{\alpha_{poor}}{\tau_{poor}}} = \frac{\frac{-2.559}{35.3}}{\frac{0.567}{2.252}} = \underset{[-0.529,-0.087]}{\text{-0.288}} \tag{I.1}$$

$$\varepsilon_{rich}^{own} = \frac{\frac{\beta_{rich}^{own}}{C_{rich}^{own}}}{\frac{\alpha_{rich}}{\tau_{rich}}} = \frac{\frac{-2.605}{35.6}}{\frac{0.294}{1.894}} = \underset{[-1.083,-0.038]}{\text{-0.471}} \tag{I.2}$$

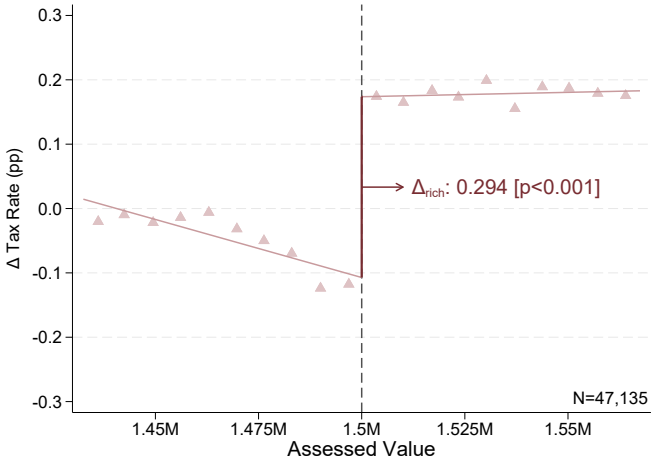
We estimated a significant own-rate elasticity for poor households of -0.288, indicating that for every 1% decrease in their tax rate, compliance increased by 0.288%. Similarly, the own-rate elasticity for rich households is -0.471, implying that for every 1% increase in their tax rate, compliance decreased by 0.471%. To test whether the difference in elasticities between the two groups is statistically significant, we conducted 5,000 bootstrap iterations, which revealed no significant difference (p-value = 0.678).

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

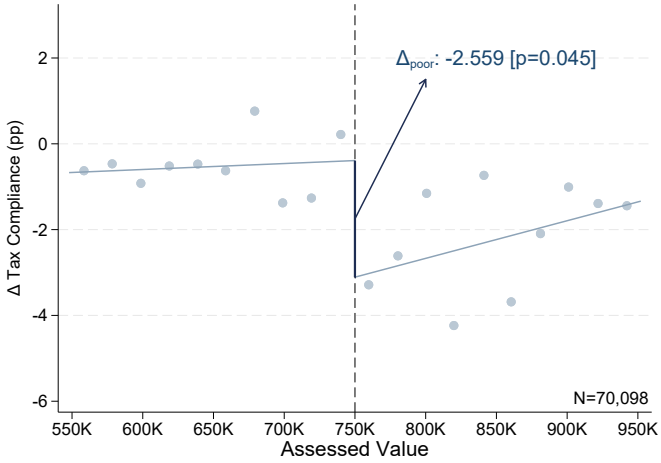
(a) First-Stage: Poor Households



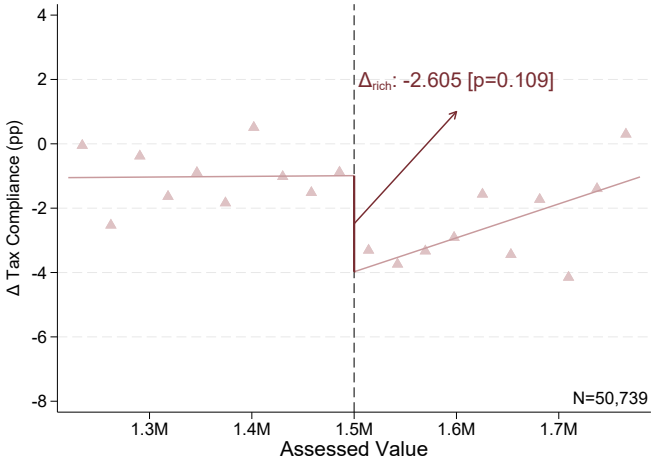
(b) First-Stage: Rich Households



(c) Reduced-Form: Poor Households



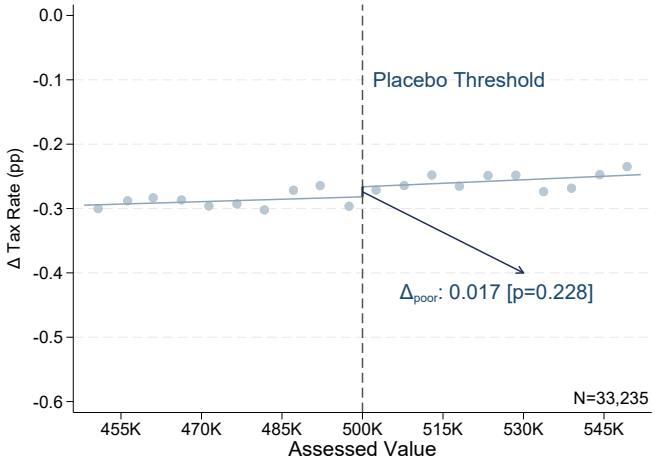
(d) Reduced-Form: Rich Households



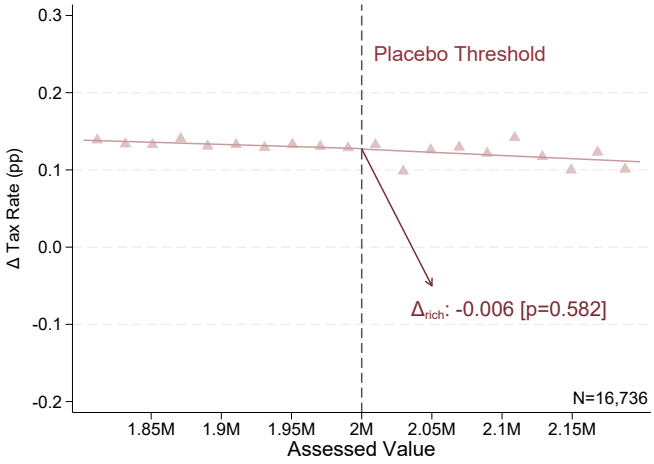
Note: Panels (a) and (b) represent the first-stage for poor and rich, in which we evaluate the increase in the amounts owed between the last bimester of 2023 and the fist bimester of 2024, in relation to the value of the property, in the cut-off of AR$ 750K and AR$ 1.5M, respectively. Panels (c) and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

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

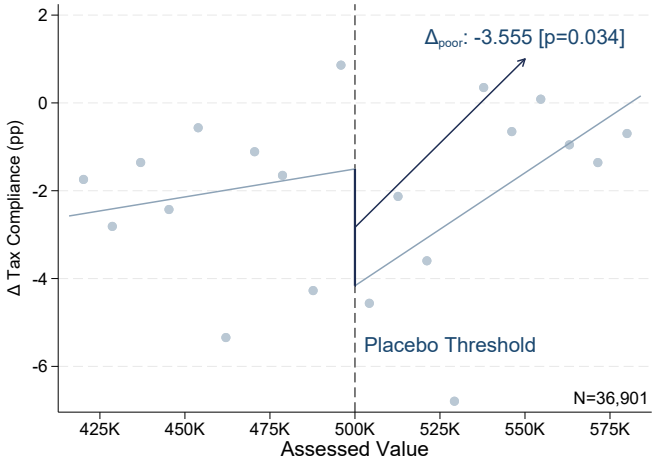
(a) First-Stage: Poor Households



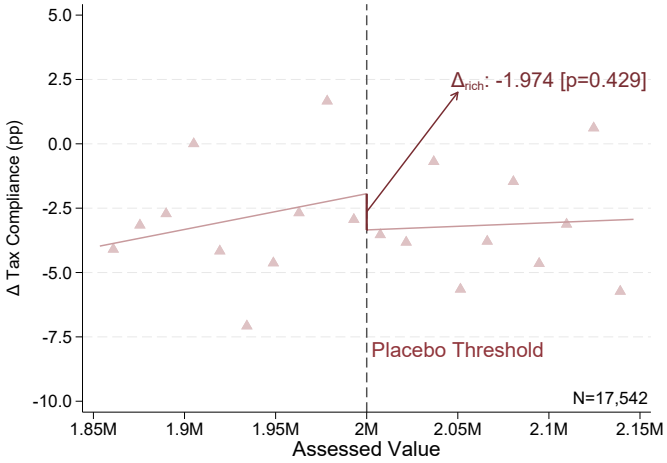
(b) First-Stage: Rich Households



(c) Reduced-Form: Poor Households



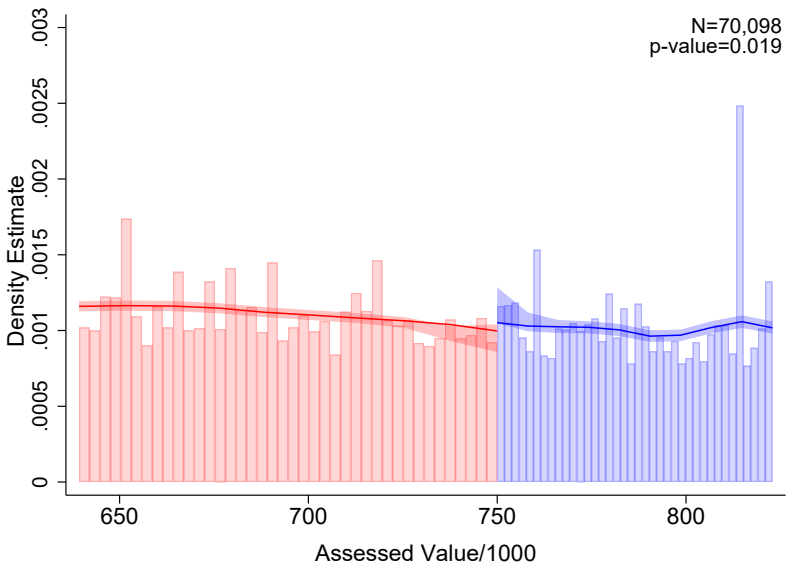
(d) Reduced-Form: Rich Households



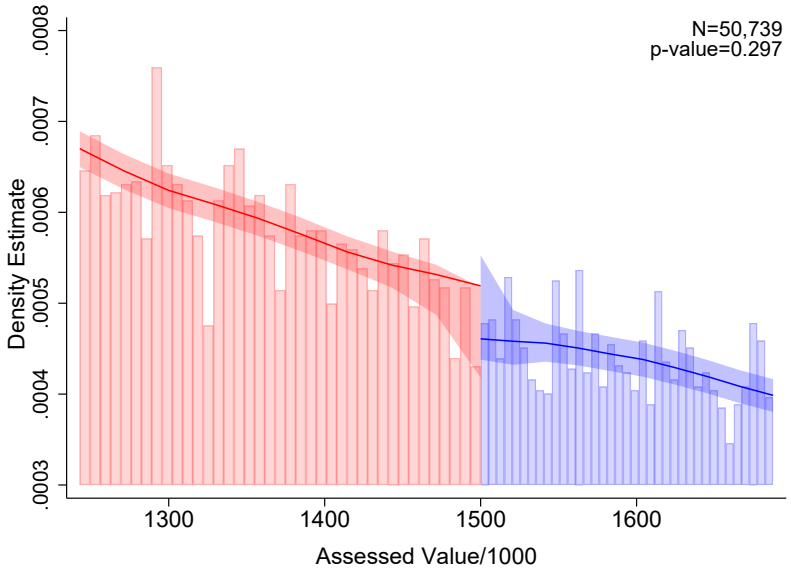
Note: Panels (a) and (b) represent the first-stage RDD for poor and rich, in which we evaluate the increase in the amounts owed between the last bimester of 2023 and the fist bimester of 2024, in relation to the value of the property, in the cut-off of AR$ 500K and AR$ 2M, respectively. Panels (c) and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

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

(a) Poor Cutoff - 2024



(b) Rich Cutoff - 2024



Note: Panels (a) and (b) show the McCrary (2008) manipulation test (with their corresponding p-value) of the property assessed values at the low-value threshold (poor) and the high-value threshold (rich) in 2024. The shaded areas represent the 90% confidence intervals. The x-axis represents assessed property value in AR$ thousands.

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

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

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

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 2024, 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 tax liability. The results are presented below, with 90% confidence intervals in brackets, calculated using 5,000 bootstrap iterations:

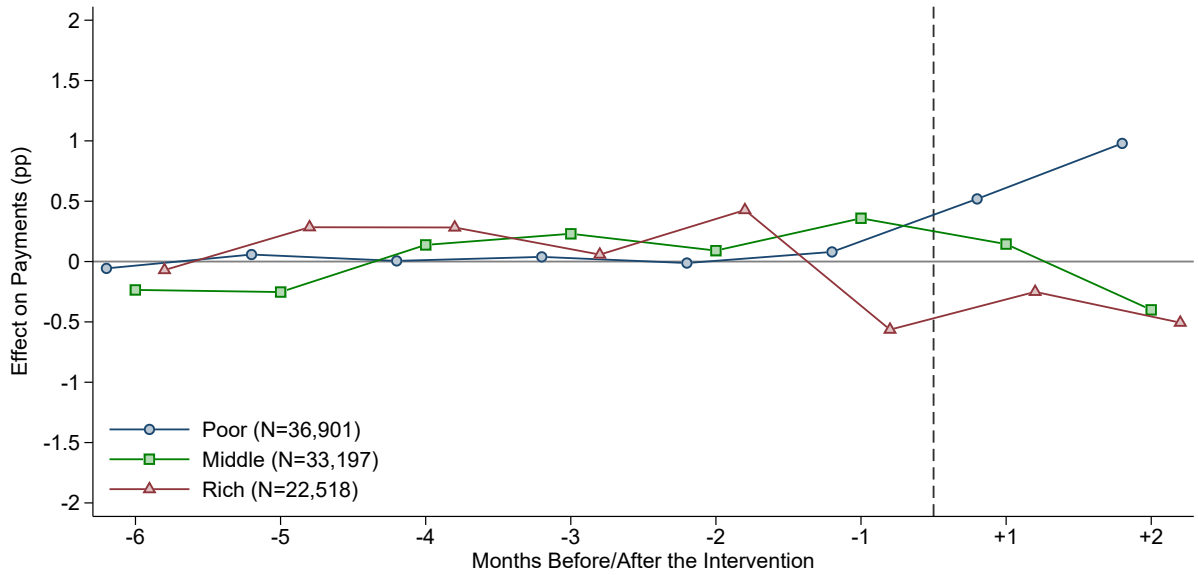
$$\varepsilon_{poor}^{cross} = \frac{\frac{\beta_{poor}^{cross}}{C_{poor}^{cross}}}{\frac{\Delta\tau_{rich}}{\tau_{rich}}} = \frac{\frac{0.721}{38.4}}{0.15} = \underset{[0.025,0.229]}{0.125} \tag{I.3}$$

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

We estimated a significant cross-rate elasticity for poor households of 0.125, indicating

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

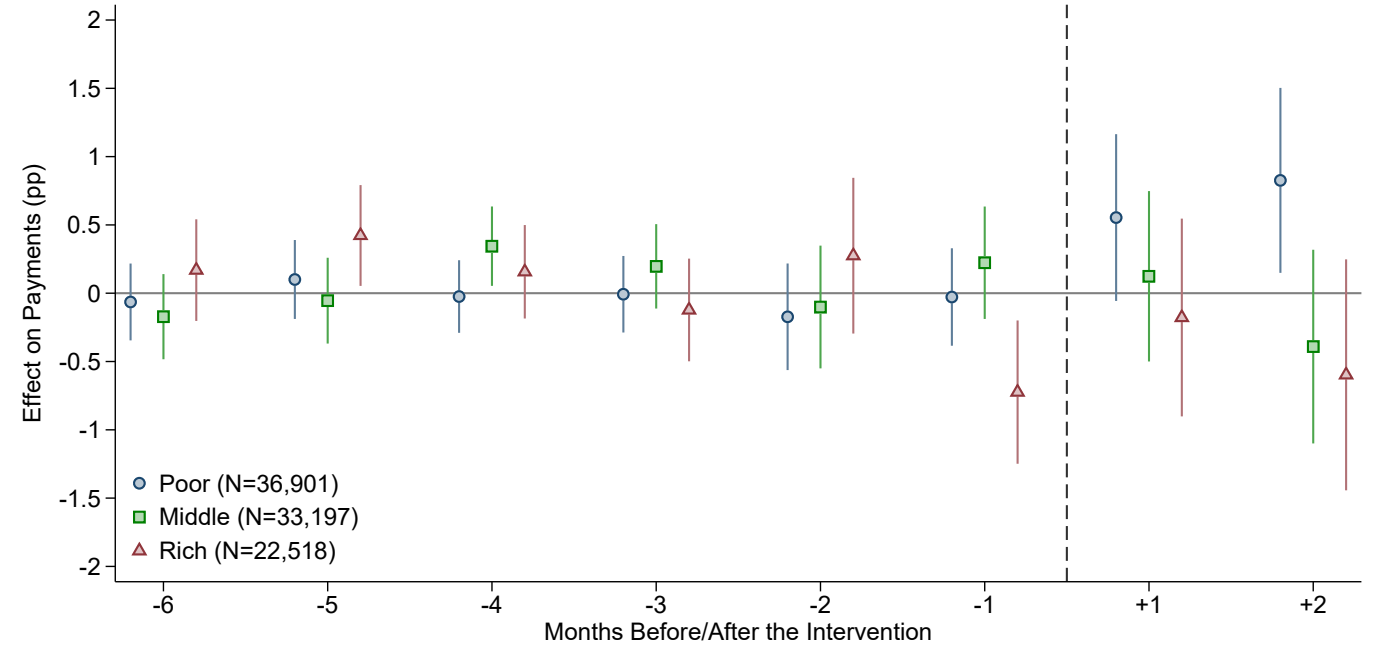
Figure I.5: Difference in Payment Rates between Control and Treatment Letters: 2024



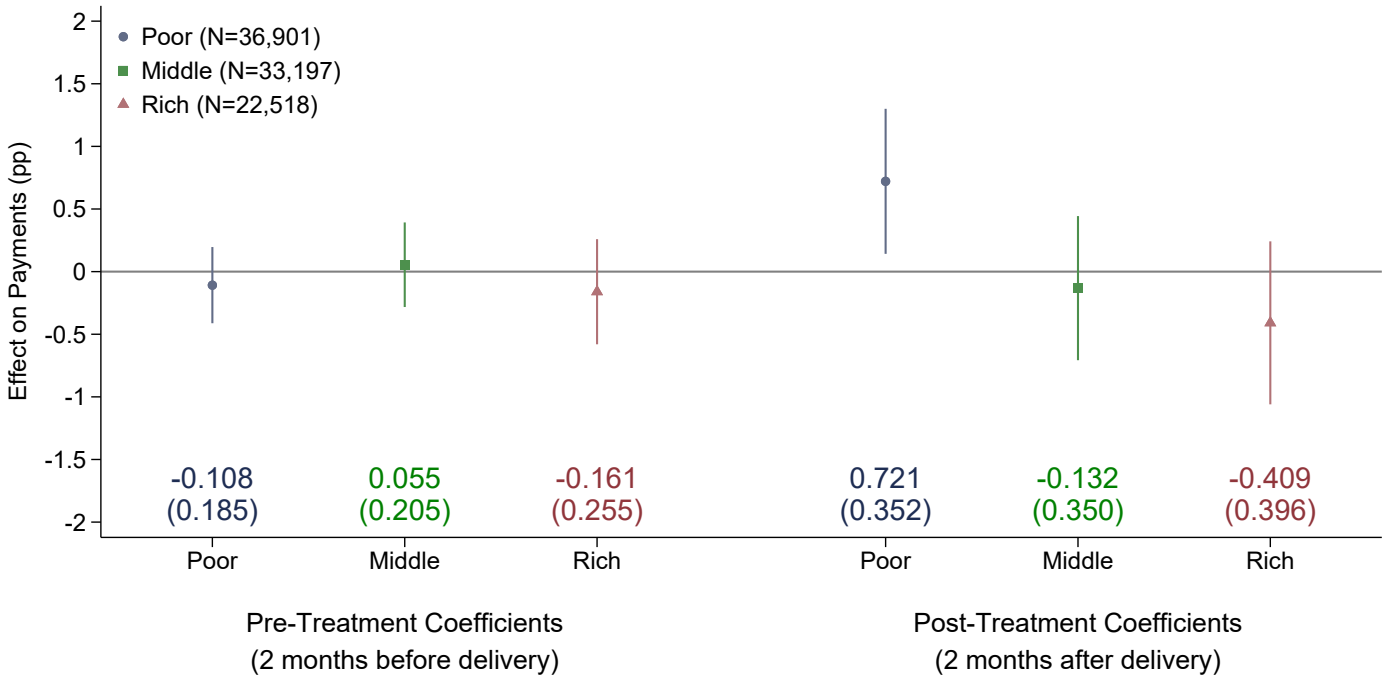
Note: This figure compares the likelihood of paying the monthly bill between treatment and control, normalized by the average pre-treatment difference. We estimated the coefficient for each monthly bill in separate regressions. The sample includes residential and non-residential properties and excludes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

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

(a) Event-Study Analysis (Monthly Level)



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



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

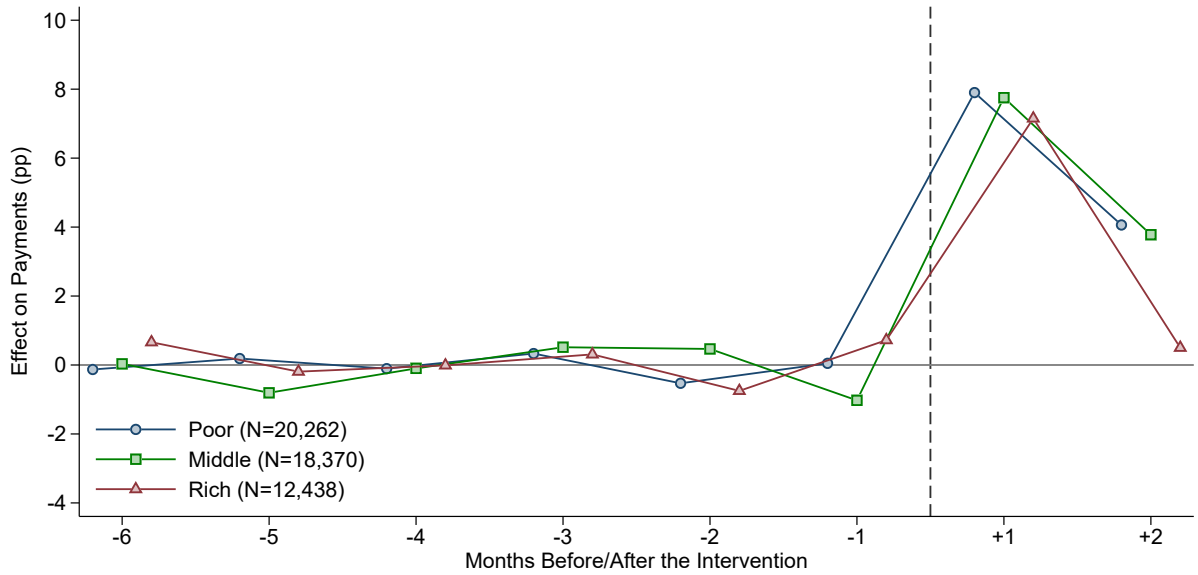
I.4 Control Letter vs No Letter

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

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

In conclusion, the results of this section validated and replicated the findings from the 2023 analysis.

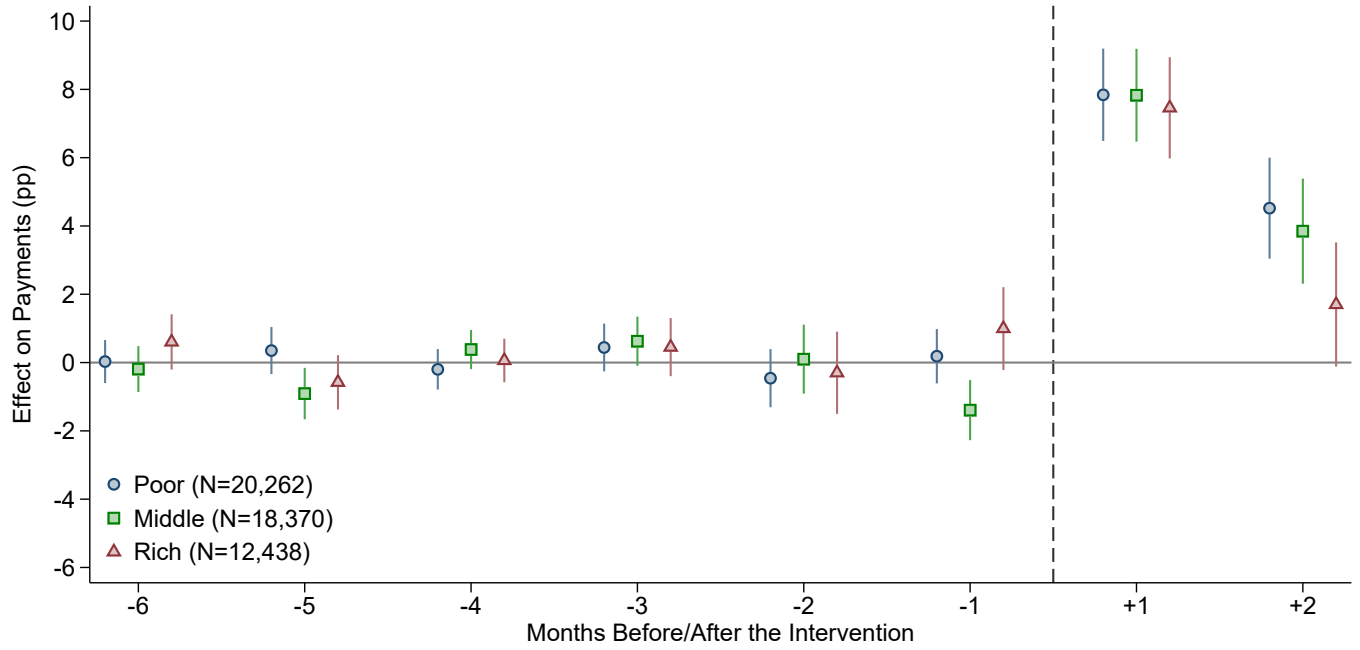
Figure I.7: Difference in Payment Rates between Control and No-Letter: 2024



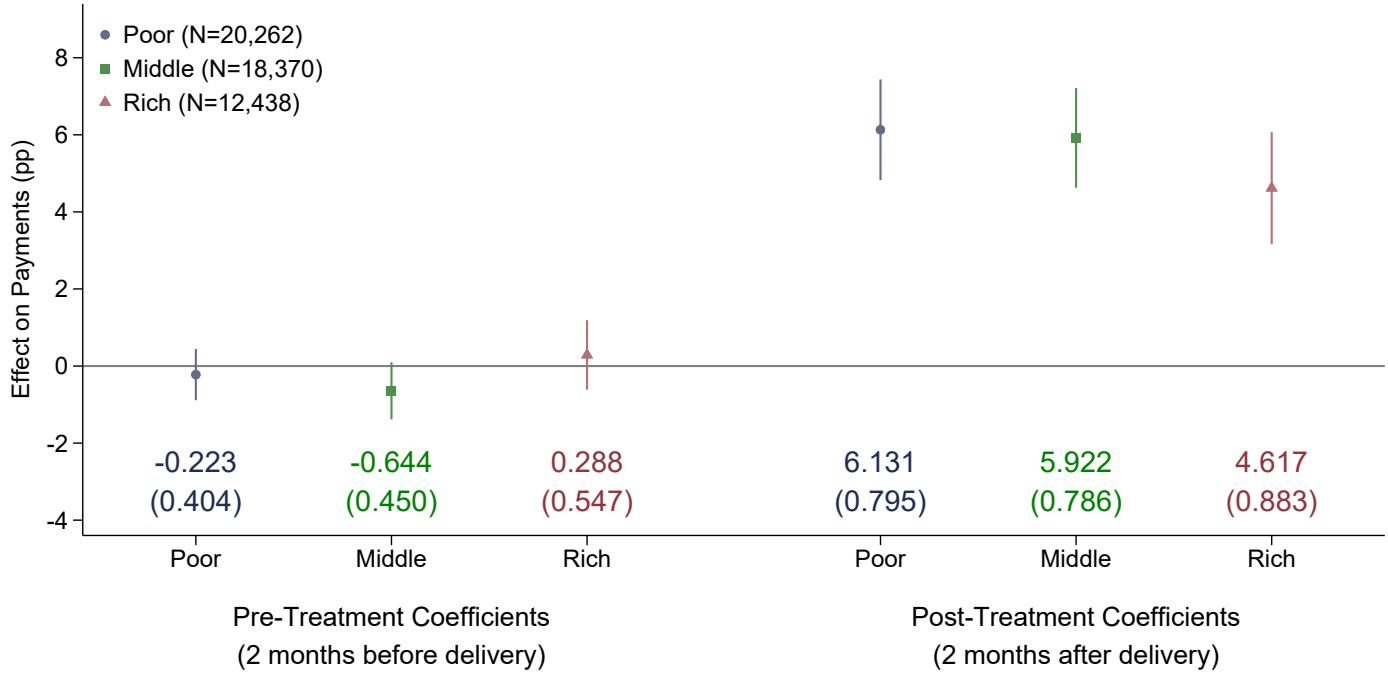
Note: This figure compares the likelihood of paying the monthly bill between treatment and control, normalized by the average pre-treatment difference. We estimated the coefficient for each monthly bill in separate regressions. The sample includes residential and non-residential properties and excludes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

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

(a) Event-Study Analysis (Monthly Level)



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



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

J Counterfactual Analysis

J.1 Additional Details of the Counterfactual Analysis

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

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

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

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

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

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

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

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

J.2 Relative Contributions of Own-Rate and Cross-Rate Effects to the Total Behavioral Response

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

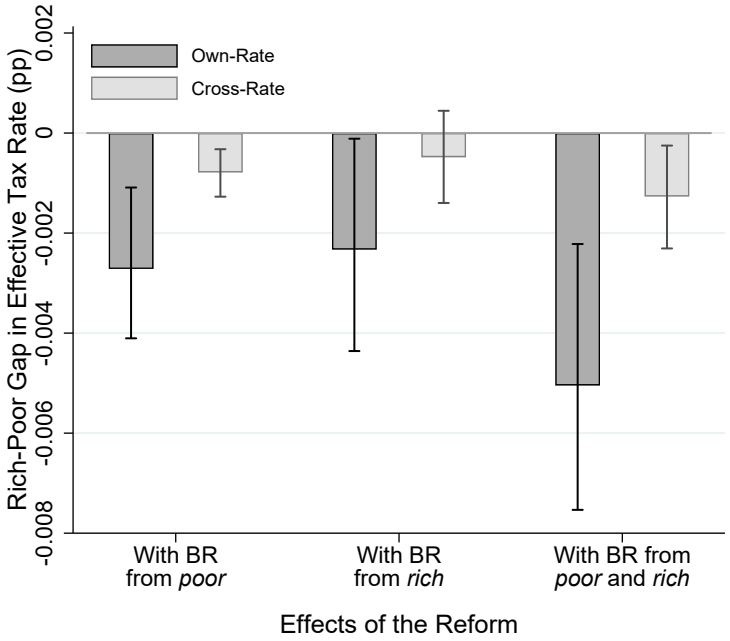
We now present the methodology used to separate the effects of changes in taxpayers' own rates (own-rate effects) from changes in others' rates (cross-rate effects) and their contribution to compliance and revenue outcomes.

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

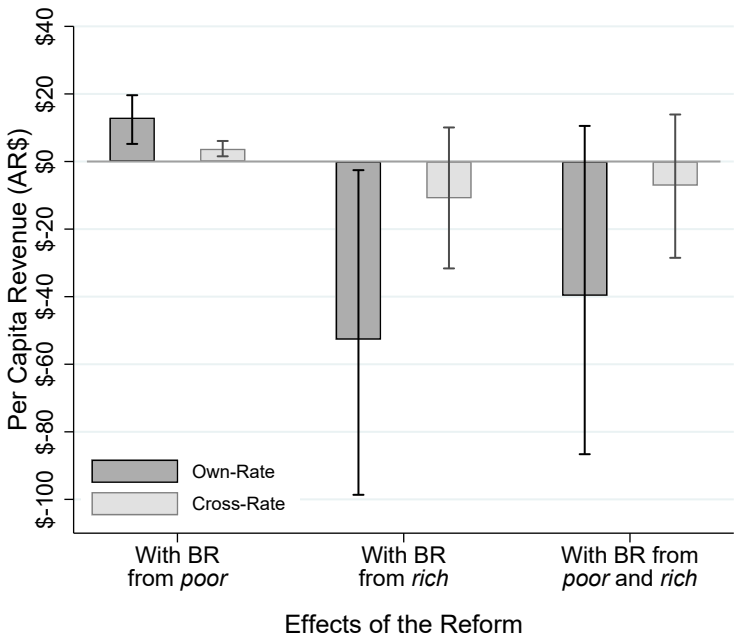
In contrast, panel (b) of Figure J.1 highlights that revenue increases for poor households through both the own-rate and cross-rate channels. Conversely, revenue declines for rich households through these same channels, though the cross-rate effect for rich properties is not statistically significant.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

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

(a) Rich-Poor Gap: Decomposition



(b) Per Capita Revenue: Decomposition



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

K Forecast Survey

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

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

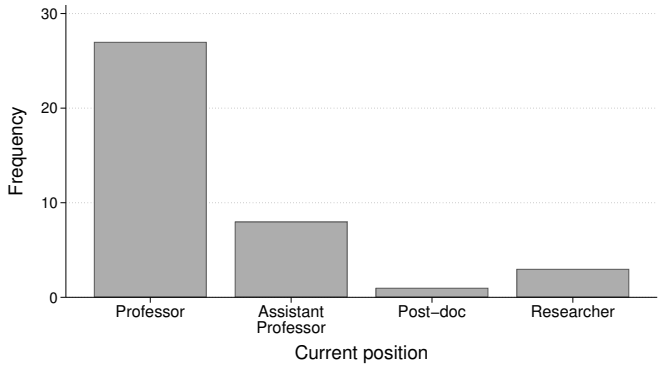
Figure K.2 shows that experts' predictions revealed significant variability across valuation groups. For poor properties, the majority expected a positive compliance response, attributing this to increased perceptions of fairness arising from the reform. However, for middle properties, predictions were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

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

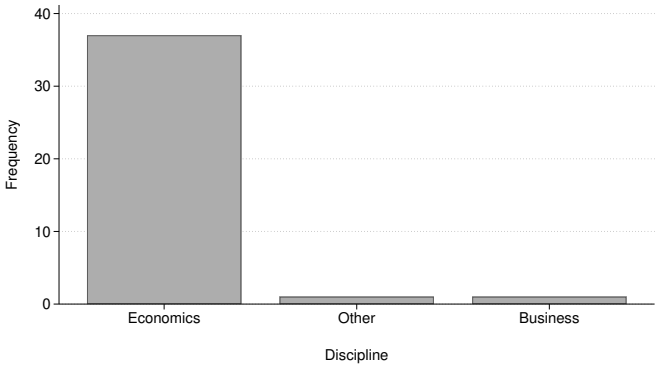
Overall, the survey results underscore the distinctive nature of the experimental findings. Although experts accurately anticipated positive compliance effects among poor properties, they significantly overestimated the strength of the treatment's impact. In contrast, the responses for medium- and high-value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

Figure K.1: Descriptive Statistics: Forecast Survey

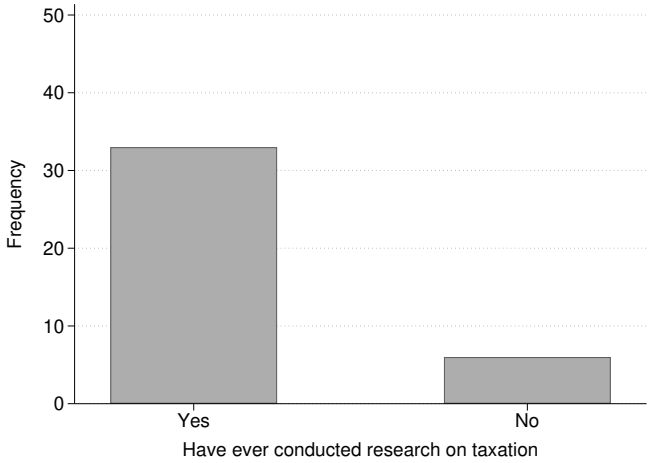
(a) Current Positions of Participants



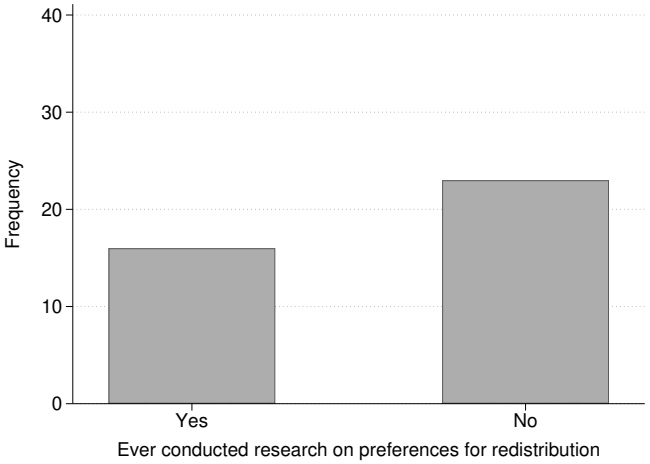
(b) Disciplines of Participants



(c) Research on Taxation



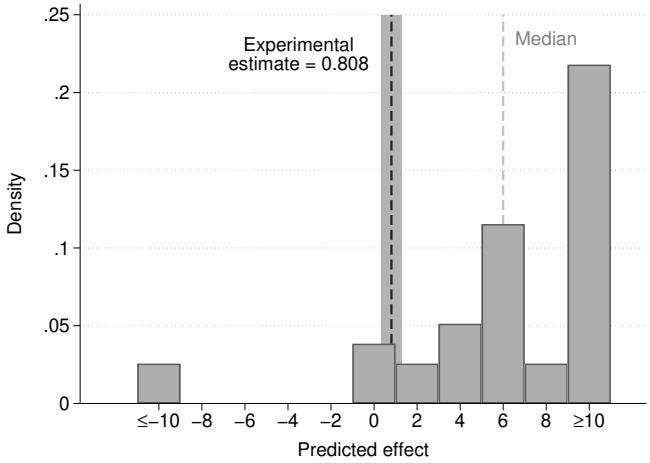
(d) Research on Redistribution



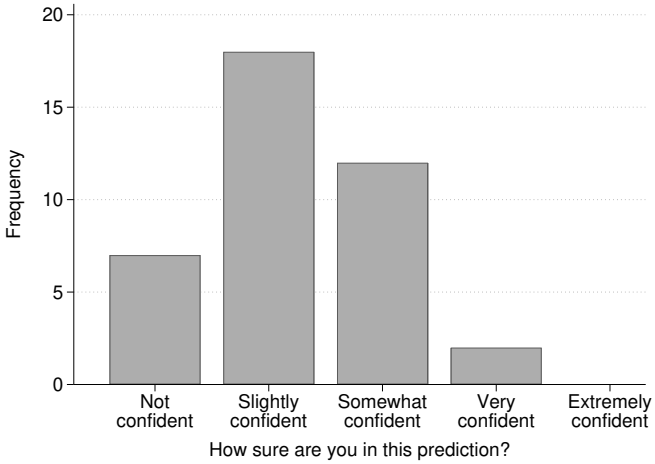
Note: These figures summarize the descriptive statistics of the expert sample from the forecast survey. Panel (a) shows the distribution of participants' current positions, while panel (b) presents the disciplines they specialize in. Panels (c) and (d) display the proportion of participants who have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

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

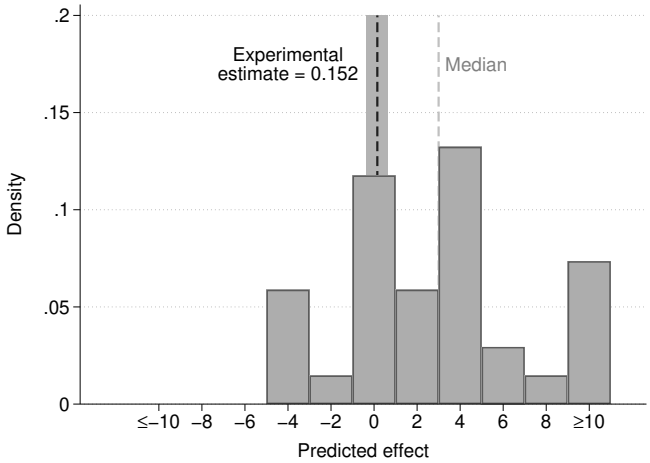
(a) Predictions: Poor-Households



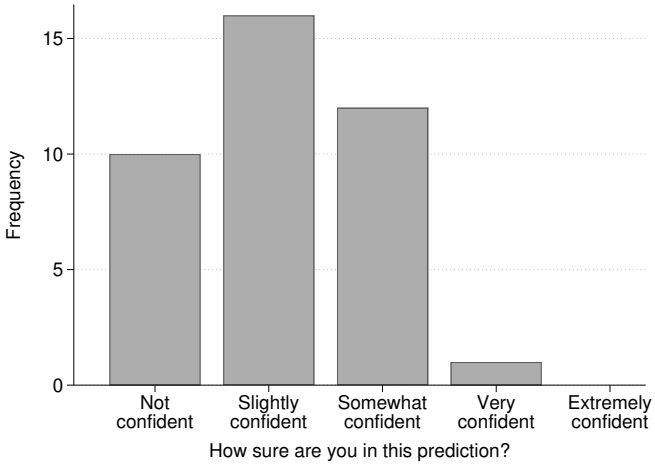
(b) Confidence: Poor-Households



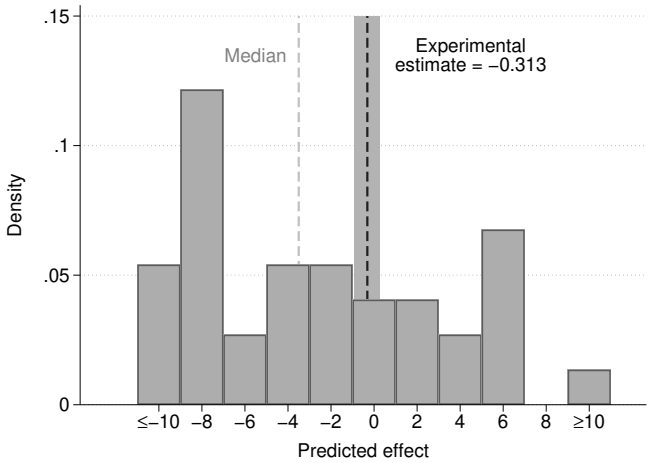
(c) Predictions: Middle-Households



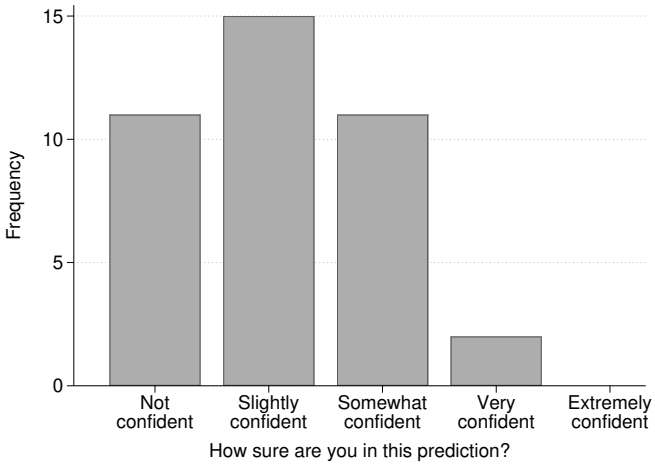
(d) Confidence: Middle-Households



(e) Predictions: Rich-Households



(f) Confidence: Rich-Households



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

L Sample Letters

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

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

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

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

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

Municipality of Tres de Febrero

General Services Tax

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

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

LOCATION:
[redacted]985

ACCOUNT: [redacted]

CADASTRAL NOMENCLATURE
Cir: 4 [redacted]1

RATE: 0.0121 **CATEGORY:** 1

VALUATION: $682.124

Monthly calculation

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

ANNUAL PAYMENT

TWO MONTHS DISCOUNT

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

2000001[redacted]30215

Scan the QR and pay through Mercado Pago

[redacted]

FIRST THREE MONTHS PAYMENTS

If you made the annual payment, waive the following fees

JANUARY

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

2023001[redacted]30215

[redacted]

FEBRUARY

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

2023002[redacted]30215

[redacted]

MARCH

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

2023003[redacted]30315

[redacted]

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

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

Municipality of Tres de Febrero

LESS TAXES, MORE WORK

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

FREE ABILITATIONS

For any sector or size.

Shops

SMEs and industries

Commercial vehicles

NO OBSTRUCTIONS OR BUREAUCRACY

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

WE HELP THOSE WHO GIVE WORK

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

SIMPLER FOR YOU

All your experience with the Municipality in one place

With your user you can:

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

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

FAIRER AND MORE EQUITABLE TSG

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

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

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

A 30% discount was applied to the 35% of properties with the lowest tax valuation.

No changes were made to the next 45% of the intermediate tax valuation

An average surcharge of 15% was made to the 20% of the highest valued properties

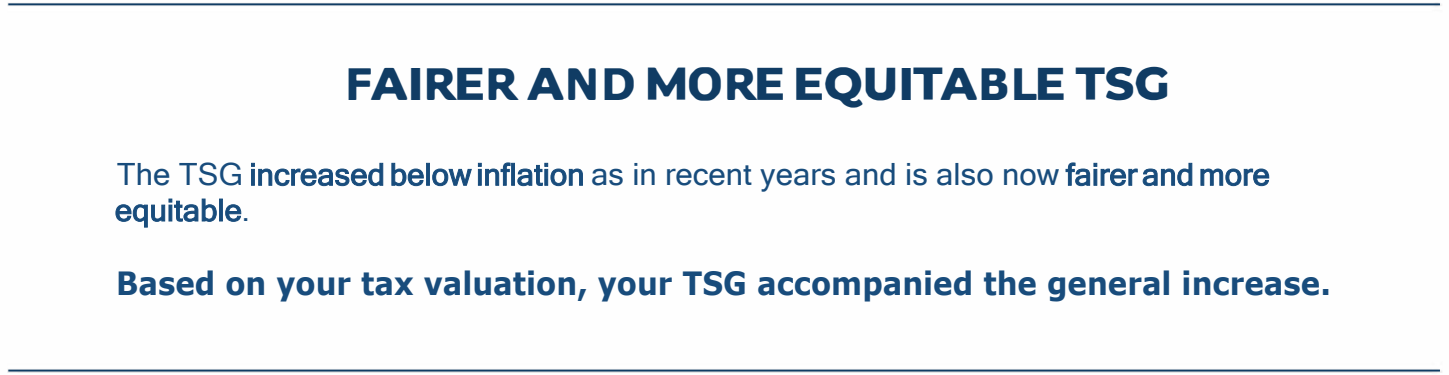
---

TELL US WHAT YOU THINK ABOUT OUR TAX POLICY

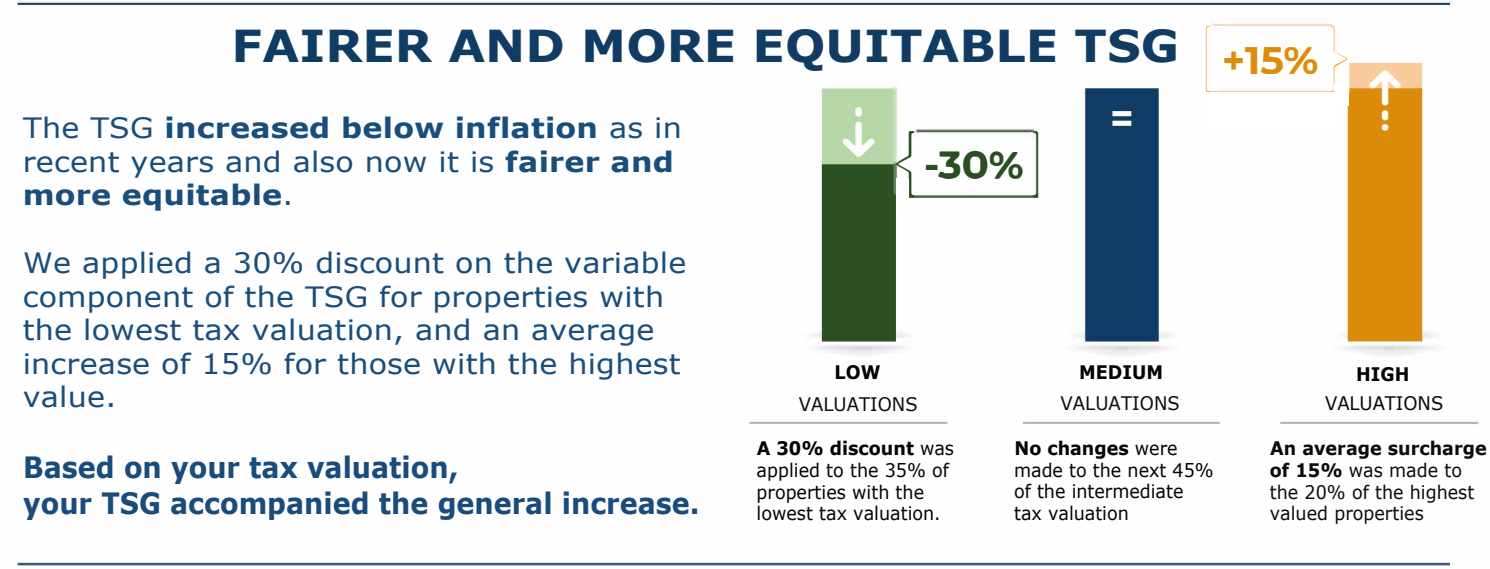
Knowing what you thought helps us improve.
Scan the QR or go to opinion.tresdefebrero.gov.ar/tasas2023

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

(a) Control Letter Message



(b) Treatment Letter Message

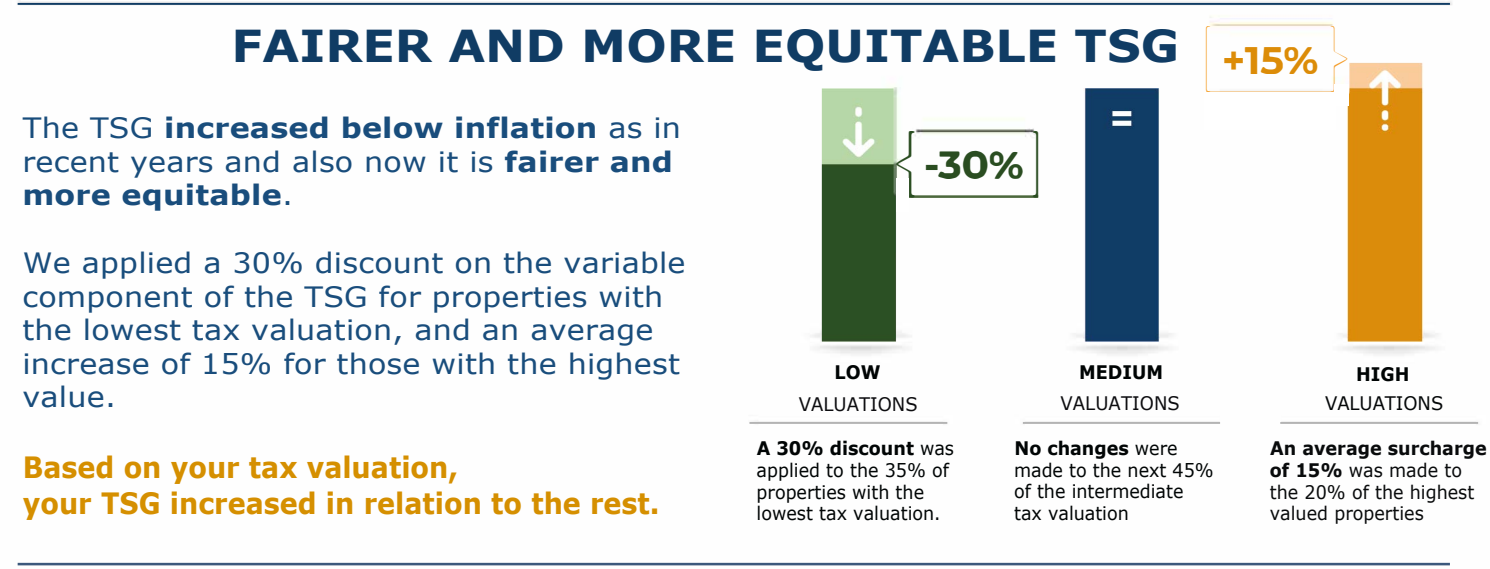


Note: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained on the second page of the mailer.

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

(a) Control Letter Message

(b) Treatment Letter Message



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

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

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

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

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

Municipality of
Tres de Febrero

Tasa por Servicios Generales

ACCOUNT:

MR/MRS:

IN CHARGE:

LOCATION:

ADDRESS:

CADASTRAL NOMENCLATURE

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

CATEGORY:

MODULE VALUE: **RATE:**

VALUATION:

Monthly calculation

MONTHLY TOTAL: $

Amount owed to date

PREVIOUS PERIODS:
CURRENT *MORATORIAS*:

TOTAL: $

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

Payment methods

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

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

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

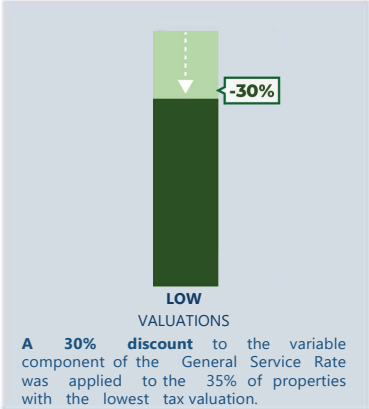
Due Date AMOUNT $

Municipality of
Tres de Febrero

TSG EVEN FAIRER AND EQUITABLE

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

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



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

SIMPLER FOR YOU

All your experience with the Municipality in one place

With your user you can:

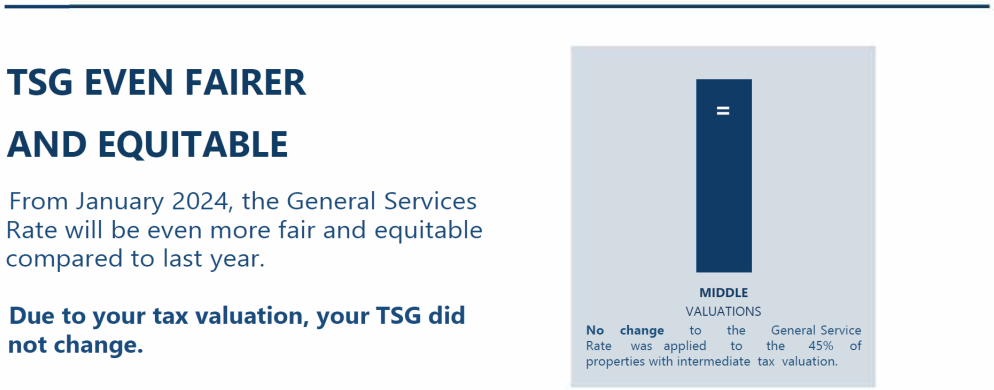
- **Pay your municipal taxes**
- **Carry out online procedures and registrations**
- **Load urban reports from your neighborhood**

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

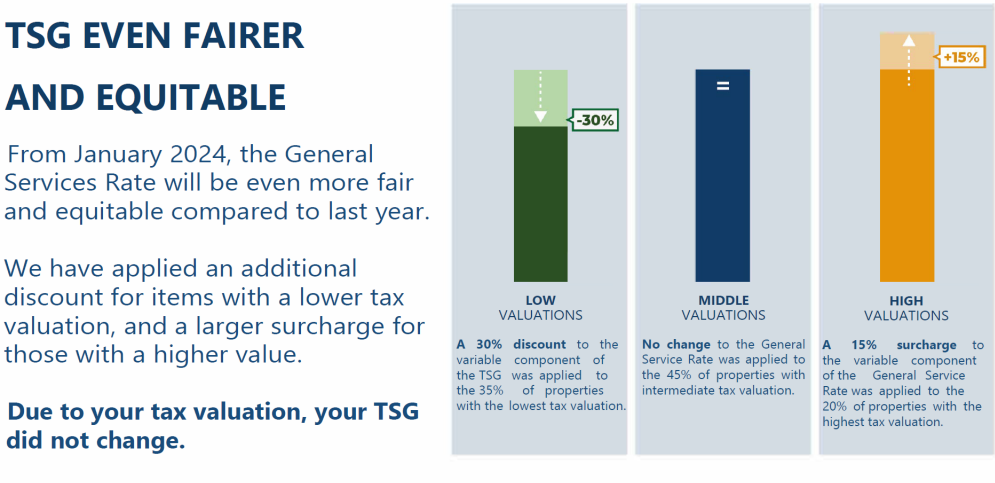
Remember that you can view all your **invoices** and join the **automatic debit** through **mi3f.tresdefebrero.gov.ar**

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

(a) Control Letter Message



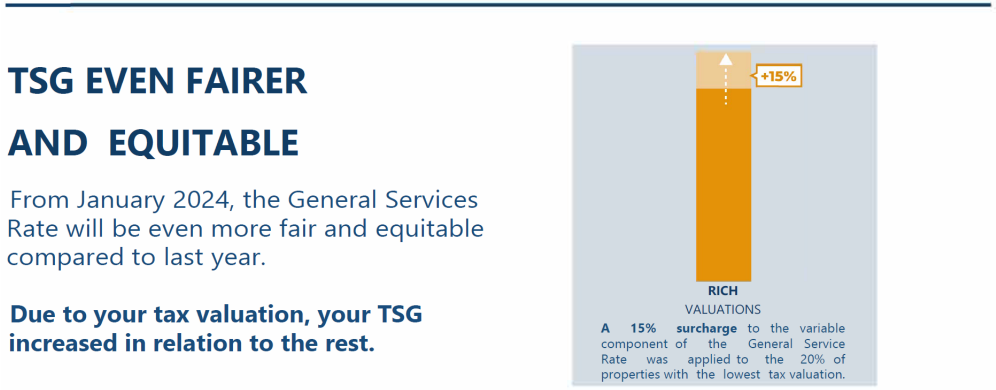
(b) Treatment Letter Message



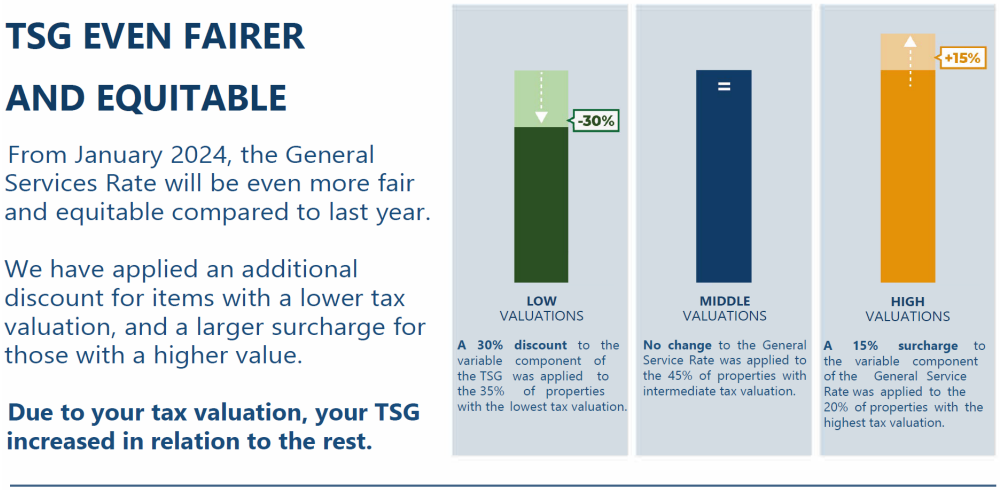
Note: English translation (from Spanish) of the main pieces of information from the mailers. This text was contained on the second page of the mailer.

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

(a) Control Letter Message



(b) Treatment Letter Message



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

M Survey Questionnaires

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

Gmail

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

Tres de Febrero <ANONYMIZED> To:
ANONYMIZED

JOIN THE MUNICIPALITY WHATSAPP

Hello ANONYMIZED!

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

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

CLICK HERE TO COMPLETE

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

Municipality of Tres de Febrero

LESS TAXES, MORE WORK

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

FREE ABILITATIONS
For any sector or size.

Shops | SMEs and industries | Commercial vehicles

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

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

FAIRER AND MORE EQUITABLE TSG

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

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

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

CLICK HERE AND SHARE THE INFORMATION

Unsubscribe Unsubscribe link

M.2 Complementary Taxpayer Survey (English Translation)

Welcome!

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

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

Additional Information - Control Poor

FAIRER AND MORE EQUITABLE TSG

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

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

Additional Information - Control Middle

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 accompanied the general increase.

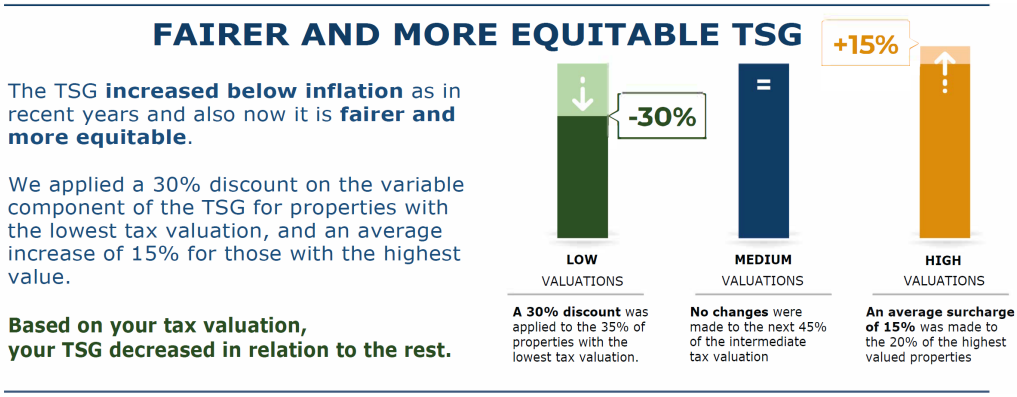
Additional Information- Control Rich

FAIRER AND MORE EQUITABLE TSG

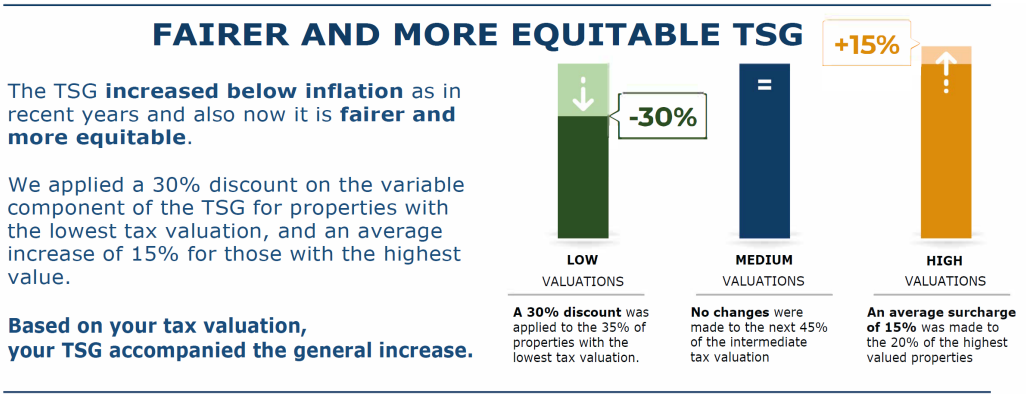
The TSG **increased below inflation** as in recent years and is also now **fairer and more equitable**.

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

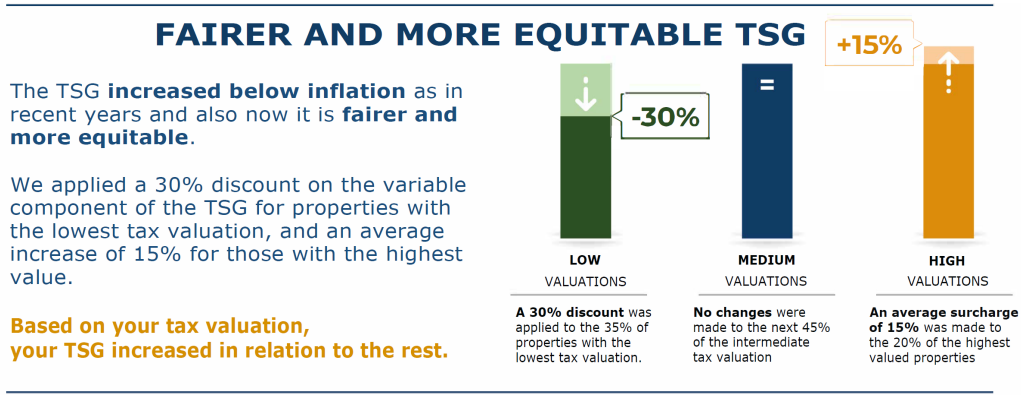
Additional Information - Treatment Poor



Additional Information - Treatment Middle



Additional Information - Treatment Rich



Do you live in Tres de Febrero?

- ◯ Yes
- ◯ No

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

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

Were you benefited by any of these policies?

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

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

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

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

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

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

How fair do you think the current distribution of municipal tax rates is between people with more and fewer resources—that is, how much each of these groups pays?

Very unfair — Very fair

0 1 2 3 4 5 6 7 8 9 10

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

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

Strongly disagree — Strongly agree

0 1 2 3 4 5 6 7 8 9 10

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

Strongly disagree — Strongly agree

0 1 2 3 4 5 6 7 8 9 10

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

Strongly disagree — Strongly agree

0 1 2 3 4 5 6 7 8 9 10

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

Very dissatisfied — Very satisfied

0 1 2 3 4 5 6 7 8 9 10

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

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

M.3 Expert Forecast Survey

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

I Agree

→

Introduction:

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

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

← →

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

The tax reform affected households in the following way:

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

← →

Subject Pool:

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

Outcome of Interest:

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

← →

Subject Pool:

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

Outcome of Interest:

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

← →

Experimental Design

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

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

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

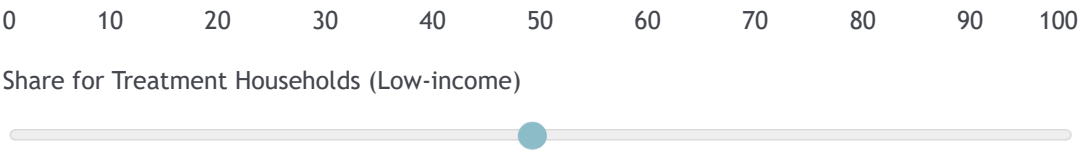
(2) Treatment message: in addition to the basic information about the recipient's own taxes, the message included an infographic with detailed information about the progressive nature of the reform – i.e., on how it affected the taxes of the three separate groups (low, middle and high property valuations).

Prediction 1. Low-income group.

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

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

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



How sure are you in this prediction?

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

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

Prediction 2. Middle-income group.

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

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

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

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

0 10 20 30 40 50 60 70 80 90 100

Share for Treatment Households (Middle-income)

How sure are you in this prediction?

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

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

Prediction 3. High-income group.

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

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

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

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

0 10 20 30 40 50 60 70 80 90 100

Share for Treatment Households (High-income)

How sure are you in this prediction?

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

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

← →

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

← →

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

Yes

No

← →

The University of Nottingham

Which of the following describes your current position?

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

← →

The University of Nottingham

Please select your discipline

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

← →

The University of Nottingham

Have you ever conducted research on preferences for redistribution?

Yes

No

Have you ever conducted research on taxation?

Yes

No

← →

This is the end of the survey.

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

← Submit