

Property Taxes as Market Signals: Evidence from Housing Markets with Information Frictions*

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Abstract

This paper estimates the causal effect of property tax liability on housing prices using georeferenced transaction and cadastral data from Santiago, Chile. Using a sharp regression discontinuity design (RDD) at the exemption threshold, we document a novel “tax premium”: becoming liable for property tax raises average housing prices by approximately 4 percent. We hypothesize that tax liability acts as a third-party certification that reduces information asymmetry, thereby overcoming the negative capitalization of tax costs. We evaluate this mechanism using three complementary tests. First, we systematically rule out alternative structural channels such as increased local public spending and social sorting. Second, we find no evidence that being liable to pay property taxes serves as a status signal or a coordination device for social sorting. Third, we show that the positive effect is driven by the extensive margin (being liable to taxes). By contrast, when we estimate the impact at the intensive margin using tax-rate changes at a higher threshold, we find the standard negative capitalization effect. Finally, by categorizing transactions by the magnitude of informational opacity derived from a hedonic price model, we find that the tax premium is negligible in transparent segments and is large and significant for properties with high informational friction. These findings suggest that in developing markets with informational gaps, fiscal tools can inadvertently serve as market signals, reversing standard incidence predictions.

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1 Introduction

Housing affordability has become a pressing policy concern as rapid house-price inflation intensifies financial strain on households and widens inequalities in access to homeownership. For example, Chile’s real prices have more than doubled in the past decade. Against this backdrop, recurrent property taxation emerges as a critical lever: it not only shapes user costs and, therefore, housing market dynamics, but also supplies a substantial share of local revenues—averaging 19 percent of municipal resources across the OECD (OECD, 2022), 27 percent of state-and-local tax receipts in the United States (Yushkov, 2025), and 61 percent of municipal income in Chile. Understanding how property taxes interact with housing prices is thus essential for designing policies to finance local public goods that distort the housing market as little as possible and decrease distributional imbalances.

This paper explores the causal relationship between property tax liability and residential housing prices. Drawing on the universe of transactions in Santiago between 2014 and 2017, we link each sale to georeferenced cadastral records containing floor area, construction date, cadastral value, and tax liability, resulting in 229,379 linked sales.

Our empirical strategy employs a sharp (RDD) that leverages a unique feature of the central government’s tax exemption rule. Every four years, the Internal Revenue Service (Servicio de Impuestos Internos, SII) massively reassesses the cadastral value of all properties in the country based on the neighborhood housing market appreciation, urbanization projects, and available infrastructure. Critically, the exempt threshold is adjusted proportionally to the national average change in residential property valuations and applies nationwide. Because reassessments differ across properties while the threshold shift applies to all, otherwise comparable dwellings fall just above or below the new cutoff, providing quasi-random variation in tax liability. Importantly, our identification does not rely on time-series variation. Instead, we focus on a single reassessment period and implement a cross-sectional RDD strategy within a four-year window.

Contrary to the predictions of standard capitalization theory, we find that becoming liable to property tax increases housing prices by approximately 4%. This counterintuitive “tax premium” challenges the conventional view that taxes act solely as a financial burden. We hypothesize that in markets with significant information asymmetry, the status of being subject to a positive tax liability serves as a credible signal of neighborhood market appreciation—effectively a government-backed “certification” of valuation—that outweighs the direct financial cost of the tax.

We employ a threefold strategy to support our hypothesis. First, we systematically rule out alternative structural channels. We show that the price premium is not driven by local public spending (by exploiting variation within districts sharing the same amenities) nor by social sorting

(by showing the effect persists even in high-taxpayer density areas).

Second, we estimate the impact on prices along the intensive margin of tax liability. For properties liable to taxes, there is a second threshold where rates increase. Using a regression kink design (RKD) at this second threshold, we find the standard capitalization effect: higher taxes depress prices. This crucial finding reconciles our results with standard theory. It demonstrates that while the financial cost of taxation does exert downward pressure on prices, it is completely overcome by the positive signaling value at the point of entry into the tax system.

Finally, we examine the heterogeneity of the impact of tax liability on prices with respect to informational frictions. Using residuals from a saturated hedonic price model as a proxy for valuation difficulty, we find striking heterogeneity: the tax premium is statistically negligible for properties with high transparency and rises substantially for properties with noisier information. This result backs that the tax signal is most valuable precisely when buyers face the highest opacity.

Our paper contributes to the literature on policies to reduce frictions in housing markets. Search costs and information frictions have been shown to adversely impact the mobility of lower-income households in the US (Bergman et al., 2024). Tandel et al. (2025) shows that a mandatory disclosure of the litigation status of housing projects in India led to a 4%–6% decline in the average price per square foot of litigated units relative to non-litigated units. By examining the interplay between property taxes, housing prices and market signals, we provide new insights into how information frictions shape housing prices.

We also add to the literature on property tax capitalization. Oates (1969) established the traditional view, arguing that higher property taxes decrease property values. Empirical studies have built on this framework, drawing on extensive evidence from developed economies to examine the magnitude of the price drop that accompanies property taxes. For instance, Oliviero and Scognamiglio (2019) found a decrease in prices following an increase in property tax rates in Italy. In contrast, Økland (2020) found no effects on prices in Norway, arguing that improved public services offset tax increases. Similarly, Elinder and Persson (2017) detected no significant price effects in Sweden. Spatial econometric studies further indicate a decrease in prices, but that it is lower than the increase in ownership costs (Giertz et al., 2021; Livy, 2018). We provide causal evidence that the signaling information channel substantially increases property prices, more than offsetting a potential negative effect due to capitalization.

We also add to the literature on inattention in the housing markets. Bradley (2017) provides evidence that homebuyers may not behave in a well-informed manner and that responses to property taxation are susceptible to inattention. Other studies confirm that informed and uninformed buyers behave differently (Li and Chau, 2024; Ihlanfeldt and Mayock, 2012), with non-local buyers often paying premiums due to information disadvantages. Developers usually rely on quality guarantees

and branding to mitigate asymmetric information (Kurlat and Stroebel, 2015). Our study adds to the literature by showing that buyers pay relatively more attention to a simple, salient tax-status signal than to more complex underlying information.

The remainder of the paper is structured as follows. Section 2 describes the institutional features of the Chilean property tax system. Section 3 details the data and outlines the empirical strategy. Section 4 presents the main findings, robustness checks, and further estimations. Section 5 reviews and disentangles the main channels identified in the literature, followed by Section 6, which conducts a heterogeneity analysis related to asymmetric information. Finally, Section 7 concludes with policy implications and directions for future research.

2 Background: Property tax in Chile

Our analysis focuses on the “Región Metropolitana de Chile” region, where Santiago is located. It has over 7.4 million inhabitants and 2.7 million dwellings according to the 2024 census. Our study period is 2014–2017, and we look exclusively at non-agricultural properties. The *Contribuciones de Bienes Raíces*, or *Impuesto Territorial*, referred to hereafter as the *property tax*, is a levy on individuals’ wealth held in the form of real estate ownership. The tax distinguishes between agricultural property and non-agricultural property, the latter further divided into residential and non-residential uses (Henríquez, 2014).

To determine the tax, each residential property i is categorized into three groups based on its cadastral value C_{it} , assessed at time t .

- The **first group** is fully exempt and includes properties with assessed values below the exemption threshold E_t , i.e., $C_{it} \leq E_t$.
- The **second group** pays property taxes on the portion of the value above the exemption threshold, $C_{it} - E_t$, at a rate τ_{t1} , as long as $C_{it} \leq B_t$. This group also pays a municipal sanitation tax (F_s) that is independent of the cadastral value.
- The **third group**, with values above the second threshold B_t , pays a higher rate τ_{t2} on the portion above B_t , and the lower rate τ_{t1} on the portion between E_t and B_t . This group also pays the fee F_s .

Formally, the property tax for each property T_{it} is defined as:

$$T_{it} = \begin{cases} 0 & \text{if } C_{it} \leq E_t, \\ (C_{it} - E_t) \cdot \tau_{t1} + F_s & \text{if } E_t < C_{it} \leq B_t, \\ (B_t - E_t) \cdot \tau_{t1} + (C_{it} - B_t) \cdot \tau_{t2} + F_s & \text{if } C_{it} > B_t. \end{cases}$$

This formulation ensures continuity in the tax schedule and avoids a discrete jump in the tax amount when the cadastral value crosses the second threshold B_t .

The cadastral value is updated every four years by the Internal Revenue Service (*Servicio de Impuestos Internos*, SII hereafter) through a mass reassessment process (*reavalúo*) that incorporates two criteria: neighborhood and building valuation. For neighborhood valuation, the SII considers factors such as city development, market demand, transaction prices, urbanization projects, and available infrastructure. The building valuation is based on building type, quality, age, technical specifications, and construction costs (SII, 2022).¹

We exploit a massive reassessment and a change in threshold values in 2014, as summarized in Table 1. Cadastral values and property prices are measured in \$UF. The Unidad de Fomento (UF) is a unit of account in Chile that is adjusted for inflation and, by the end of our study period, was equivalent to \$ US 44.

Table 1: Rates and brackets of the property Tax

		2006-2013	2014-2017
First bracket	Tax rate	1.00%	0.98%
	Exempt threshold value (E_t)	14,656	20,086
Second bracket	Tax rate	1.20%	1.143%
	Threshold value (B_t)	52,342	71,736

Notes: This table displays the tax brackets and corresponding rates applicable to non-agricultural properties in Chile after the 2006 and 2014 reassessment cycles. Threshold 1 refers to the rate applied to the portion of the taxable valuation within the first bracket, while Threshold 2 applies to taxable valuations exceeding the second threshold. The Exempt threshold represents the maximum valuation exempt from taxation, and the Second threshold marks the upper limit of the first bracket, after which the higher tax rate is applied. Values are expressed in 2014 million Chilean pesos (CLP).

Both the exemption and the second-bracket thresholds are adjusted proportionally to changes in the national average of residential property values. This adjustment mechanism provides a quasi-random source of variation, central to our empirical strategy. Specifically, we argue that a property's position relative to the threshold is effectively exogenous, enabling comparisons of properties near the cutoff to identify causal effects. To anticipate whether a property falls just above or below the threshold, a household would need to accurately predict its own dwelling's valuation change, the

¹According to the SII, the cadastral value generally reflects around 60% of the market value.

average change in its neighborhood, and the national average change that ultimately defines the threshold.

Following the 2014 reassessment, the SII announced that only 22% of non-agricultural properties would be subject to property tax (i.e., valued above the exemption threshold).²

3 Data and empirical strategy

For the main estimations, we pool microdata from the following sources for the period 2014–2017 into a single cross-section:

1. **Cadastral Value.** Semiannual data from the SII (Internal Revenue Service) that provides information for each property, including an identifier, location, total assessed value, taxable value, property tax liability, land area, and floor space. This information allows for identifying the tax liability for each property and its precise location.
2. **Home Sales.** Official records of every formal transaction. One of the country’s main real estate agencies (TOC-TOC) georeferenced the universe of transactions from the official real estate registry.³ This dataset includes the identifier code, location, transaction date, and final price. This data allows linking property tax status in the SII database to property-level prices.
3. **Spatial Data.** We use shapefiles for the municipalities’ boundaries and district polygons, as well as the locations of police stations, high schools, primary healthcare facilities, parks, and subway lines. These data are sourced from the national statistic agency SIEDU-INE⁴.

The resulting dataset is a pooled cross-sectional sample comprising 229,379 transactions from 2014 to 2017. Each transaction is linked to cadastral information to determine whether the property is subject to property taxes. To estimate the RDD we use the subsample of properties that are either exempt or fall in the first bracket. This is, we exclude properties whose cadastral value is above the second threshold B_t . We also exclude properties whose cadastral value is above the exemption threshold but do not pay taxes (e.g., low-income owners over 60 years old). In Section 4.6 we estimate a Fuzzy RD including these observations.

²We do not exploit other specific exemptions. Since 2014, property owners over 60 years old with an income below 13.5 UTA are exempt, while those earning between 13.5 and 30 UTA receive a 50% reduction. In 2022, over 15,000 seniors were exempted under this policy.

³The data were provided through a collaboration agreement between Toc-Toc and Pontificia Universidad Católica, and the data are protected by a confidentiality agreement.

⁴Data accessed via <https://www.ine.gob.cl/herramientas/portal-de-mapas/siedu> on December 2024.

Table A1 in the Appendix presents descriptive statistics for the RDD sample (204,828 transactions), categorized by tax payment status. Table 2 reports the same information but restricted to the optimal bandwidth sample used in the estimations, which includes 19,455 observations. It shows that paying property taxes is correlated with higher prices and greater built area.

Table 2: Descriptive statistics by property tax payment status for the selected bandwidth sample

	Total Properties		Exempted		Non-exempt	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Price per sq.m.	0.78	0.36	0.75	0.32	0.81	0.39
Cadastral value	19.99	1.32	18.90	0.64	21.19	0.66
Annual tax (including municipal fee)	0.02	0.03	0.00	0.00	0.05	0.02
Implied total rate	0.13	0.96	.	.	0.13	0.96
New	0.51	0.50	0.51	0.50	0.50	0.50
House	0.36	0.48	0.37	0.48	0.34	0.47
Age	24.52	21.09	25.07	20.78	23.93	21.41
Floorspace	61.85	28.77	61.74	28.03	61.98	29.56
Quality index	3.04	0.45	3.10	0.44	2.97	0.46
Observations	19,455		10,180		9,275	

Notes: Summary statistics are based on pooled cross-sectional data from 2014 to 2017, restricted to the selected bandwidth sample. The bandwidth of 2.286 is taken from the main regression, which estimates the optimal bandwidth using the method of Calonico et al. (2014). Price, cadastral value, and annual tax are expressed in millions of 2014 Chilean pesos. Columns report the mean and standard deviation (SD) for each variable across three groups: the total sample, properties that pay property tax, and those that do not. Variables include price per square meter (**Price per sq.m.**), which is also the dependent variable; the assessed cadastral value (**Cadastral value**); annual property tax (**Tax**), which is levied by the authority and includes a fixed cleaning charge determined at the municipal (*Comuna*) level; a dummy indicating whether the property is new (**New**); a dummy indicating whether the property is a house (1) or an apartment (0) (**House**); the building's age (**Age**); constructed area in square meters (**Floorspace**); and a quality index ranging from 1 to 5 (**Quality index**).

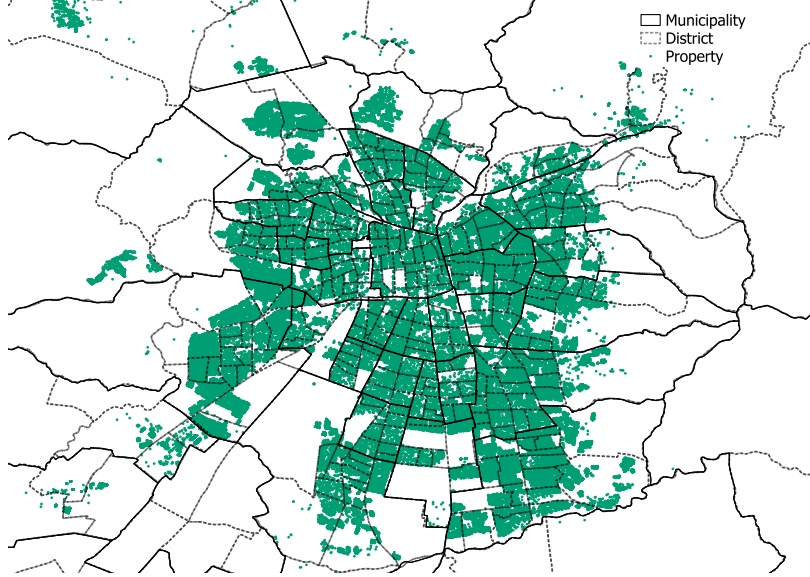
Recall that tax-liable properties are subject to two components: the property tax itself and a municipal sanitation fee (*tasa de aseo*), which varies across municipalities. The SII data reports the combined amount of these liabilities, making it difficult to disentangle the components. This combined report explains why the average tax rate reported in Table 2 differs from the rate applied to the cadastral value above the threshold (see Table 1). For the same reason, as shown in Figure 2 in the next section, properties with identical cadastral values may display different total liabilities.

For our estimations, this aggregation does not raise significant concerns. The combined liability—property tax plus municipal sanitation fee—is consistently applied across properties, and our identification strategy exploits relative variation around the exemption threshold rather than the precise decomposition of individual tax components.

Furthermore, Figure 1 presents the geographical distribution of dwellings in the dataset, along with the districts as subdivisions of the municipalities (*comunas*). Districts are subdivisions defined

by the census authority and represent the smallest aggregation level. On average, there are nine districts per municipality, 13,442 properties per district, and each district has an average area of 1.82 km². We include district fixed effects in the estimations to enhance precision by accounting for spatial heterogeneity.⁵

Figure 1: Spatial distribution of the dataset



Notes: This Figure illustrates the spatial distribution of the entire dataset of properties sold between 2014 and 2017. Districts within municipalities (*comunas*) are shown, serving as the spatial units of analysis.

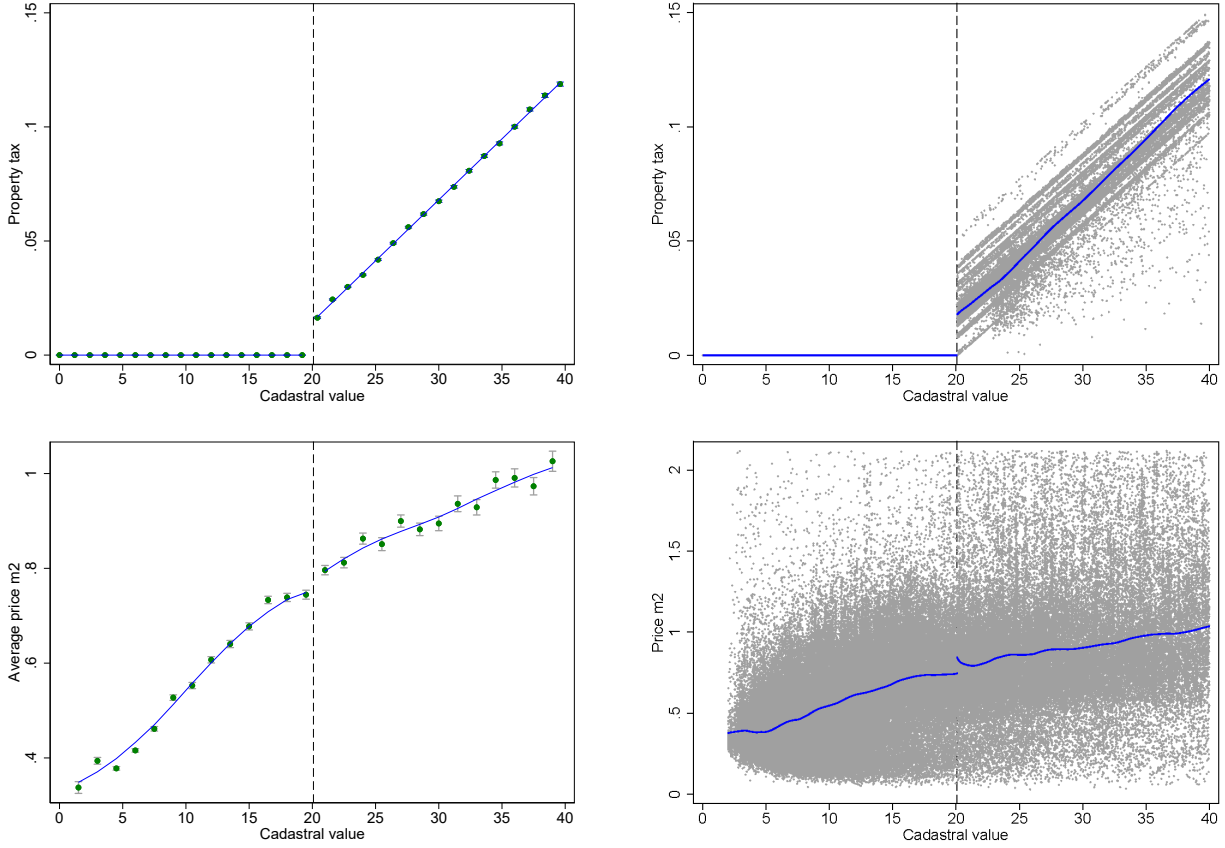
Figure 2 illustrates the relationship between cadastral value and property tax payments observed in the dataset. Panels A and B highlight the discontinuity in tax liability at the first cutoff, separating properties that do not pay tax from those that do. Panels C and D show the jump in prices per square meter around the first threshold. Although the graphical evidence suggests a clear jump in tax liability and prices at the threshold, it is insufficient to conclude on the policy’s overall impact. Still, it serves as a first step to validate our identification strategy.

3.1 Empirical strategy

To estimate the causal effect, we implement a sharp Regression Discontinuity Design (RDD) that exploits the discontinuity in effective property tax payments created by government regulations, which establish a non-taxable threshold. While similar approaches have been applied in the literature (see e.g., Alves, 2025), our design is sharp rather than fuzzy. The treatment—whether a property is liable to pay the property tax—depends on its cadastral value, which serves as the run-

⁵Figure A2 in the Appendix depicts the variation in property tax payment status within districts, focusing on observations selected around the cutoff.

Figure 2: Property Tax and Prices vs. Cadastral Value around the cutoff



Notes: Panels A and B show the relationship between cadastral value and property tax around the cutoff at 20.09 million 2014 Chilean pesos. Panels C and D show the corresponding relationship for property prices. In binned panels (A and C), each point represents the average within a bin of cadastral values, with vertical lines indicating 99% confidence intervals. Linear fits are shown separately for observations to the left and right of the cutoff point (dashed line). Raw panels (B and D) display individual observations.

ning variable in the estimation. Given the available data and the fact that cadastral adjustments occur every four years, we implement a cross-sectional RDD strategy within the period 2014–2017, a period with no changes in cadastral values, tax rates, or tax brackets.

Following Imbens and Lemieux (2008), who formalize RDD within the Rubin causal framework (Rubin, 1974), identification relies on the assumption that treatment assignment is quasi-random for properties near the threshold. This ensures that landlords just above and below the cutoff are comparable in all respects except for their tax payment status. Under this assumption, we estimate the average treatment effect at the cutoff by focusing on observations within a narrow bandwidth around the threshold, thereby isolating the causal effect while minimizing potential confounding factors.

We estimate the following specification:

$$P_i = \alpha_1 + \beta_1 D_i + \gamma_1 \cdot f(C_i) + \delta_1 X_i + \epsilon_i \quad (1)$$

where P_i is the price per sq. m. of the property i , $f(C_i)$ is the local polynomial used in the RDD with the cadastral value as the running variable, X_i is a vector of control variables, and ϵ_i is the error term. D_i is the treatment indicator (one if the property must pay taxes and zero otherwise), such that:

$$D_i = \begin{cases} 1 & \text{if } C_i \geq E \\ 0 & \text{if } C_i < E \end{cases}$$

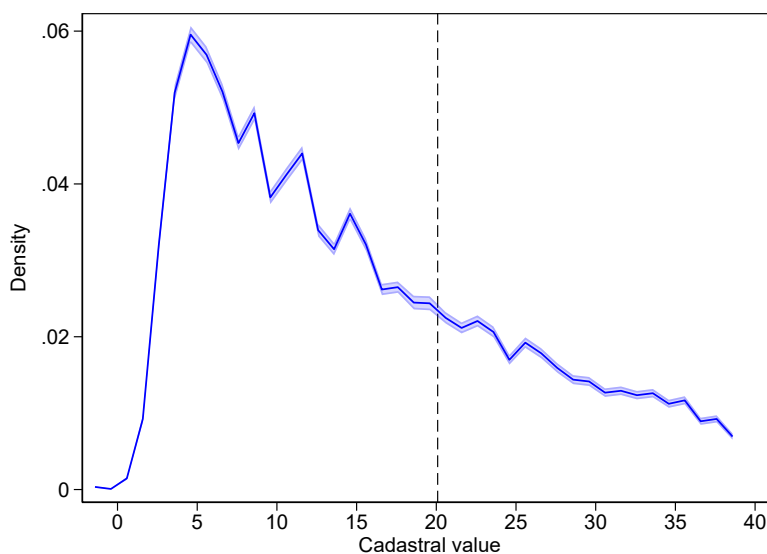
When the analysis is limited to only those observations near the cutoff point E , we have a quasi-random experiment to estimate the impact at the margin. Here, the treated and counterfactual groups are comparable, providing a valuable means of establishing a causal relationship between D_i and P_i close to the cutoff in a sharp RD design.

We performed the estimations using a non-parametric method, with the main approach being a local linear function. We follow Gelman and Imbens (2019), who argue that high-order polynomial regressions exhibit poor properties. Otherwise, the choice of bandwidth in RDD can significantly impact both the variance and potential bias in the regression results. Larger bandwidths generally reduce variance but may introduce bias because they include a broader range of observations. Conversely, smaller bandwidths reduce bias but can increase standard errors due to fewer observations. We apply the bandwidth methods proposed by Calonico et al. (2014), which aim to either minimize the mean squared error (MSE) or the coverage error rate (CER) around the cutoff point. Moreover, we use a triangular kernel for all estimations.

3.2 Identification assumptions

The identification assumptions are the validation of the discontinuity, the absence of non-bunching around the eligibility cutoffs, and local continuity in baseline covariates. To validate the first requirement, Figure 2 shows clearly the discontinuity at the first cutoff point. The second requirement is to ensure households cannot manipulate the variable that determines treatment allocation, i.e., the cadastral value threshold. This requirement is validated by thresholds that are adjusted every 4 years based on nationwide changes in cadastral values. Figure 3 presents histograms of the running variable (cadastral value) and demonstrates no abrupt changes near the cutoff, providing evidence of no manipulation.

Figure 3: McCrary Density Test: Bunching around threshold



Notes: The figure shows the density of the running variable (cadastral value) estimated around the cutoff point of 20.09 (vertical line). The shaded areas represent the 99% confidence intervals for the estimated density. Following McCrary (2008), the test shows no manipulation in the running variable near the cutoff, and the continuity hypothesis is not rejected. The same conclusion is reached when considering newer manipulation tests: the p-value of the Bugni and Canay (2021) test is 0.20.

Additionally, we perform two manipulation tests to assess the continuity of density near the cutoff points. These involve hypothesis tests based on the methods of McCrary (2008), Cattaneo et al. (2020), and Bugni and Canay (2021). We find no evidence of discontinuous changes in the density at the cutoff, reinforcing the validity of this study’s design. The tests proposed by Cattaneo et al. (2018) yield weaker evidence due to sensitivity to the polynomial order and bandwidth selection. Small changes in these parameters can slightly affect the test’s significance (see Figure A3 in the Appendix). Nonetheless, as discussed above, adjusting the cutoffs every four years based on the average variation across all properties’ cadastral values makes manipulation of the cutoffs

highly unlikely.

The third requirement concerns covariate balance between treated and non-treated units. In a valid RDD, covariates should be similar in expectation near the cutoff: treatment assignment is as good as random locally, so it is not systematically correlated with observed covariates (Imbens and Lemieux, 2008). To assess this requirement, we re-estimate the local linear RDD using each covariate as the dependent variable. As reported in Table 3, the estimated discontinuities are small and statistically indistinguishable from zero for almost all covariates.

The only exception is *Floorspace*, which exhibits a negative and statistically significant discontinuity. This sign indicates that properties subject to the tax are, on average, approximately one square meter smaller than exempt properties in a narrow neighborhood of the threshold. This very small difference could generate concerns, since smaller properties could mechanically exhibit higher prices per square meter. As a robustness check, we re-estimate the specification using the *price level* (not price per sq. m.) as the dependent variable. The discontinuity remains positive and statistically significant, confirming that the floorspace imbalance does not drive our main result.

Table 3: Covariate balance around the cutoff: RDD estimates

	(1) Floorspace	(2) High quality	(3) House	(4) Age	(5) Used	(6) Year sold
RD estimate	-1.386**	0.000	0.016	-1.179*	-0.000***	0.000*
Standard error	(0.458)	(0.000)	(0.009)	(0.541)	(0.000)	(0.000)
Bandwidth	2.779	0.731	2.653	3.699	2.158	0.817
Total observations	204,828	204,828	204,828	122,723	122,723	204,828
Obs. left of cutoff (BW)	12,378	3,057	11,805	8,746	4,674	3,334
Obs. right of cutoff (BW)	11,081	3,195	10,574	7,296	4,327	3,495

Notes: This table reports covariate balance tests using local linear regressions estimated via a regression discontinuity design (RDD). Each column corresponds to a separate regression, with the covariate listed in the column header as the dependent variable. The running variable is the property’s cadastral value, and the cutoff is set at 20.09 million constant 2014 Chilean pesos, corresponding to the tax exemption threshold.

All regressions use a triangular kernel and a first-order polynomial (local linear specification). Bandwidths are selected separately for each covariate using the MSE-optimal procedure proposed by Calonico et al. (2014). The models include the same fixed effects and controls used in the main specification: time fixed effects (year and month of transaction), district-by-year interactions, building characteristics, and spatial controls, except for the covariate used as a dependent variable. Standard errors are clustered at the district level.

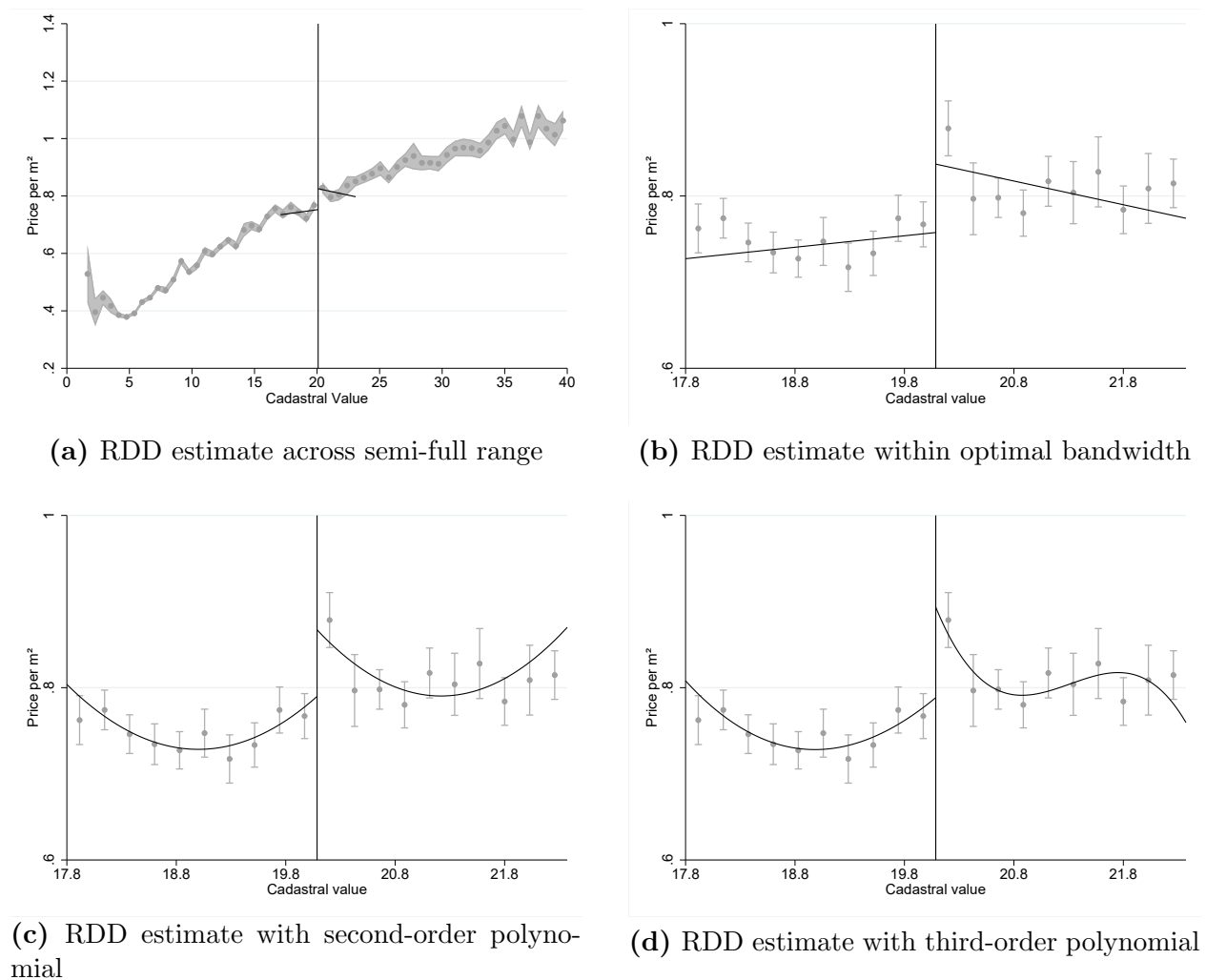
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4 Results

This section presents the results of estimating the effect of paying property taxes on housing prices using the Regression Discontinuity Design (RDD) around the exemption threshold.

4.1 Impact of property tax payments on housing prices

Figure 4: Estimated impact of property taxation on housing prices



Notes: Panels (a)–(d) display the estimated effect of being subject to property taxation on housing prices per square meter, based on a regression discontinuity design (RDD). The running variable is the cadastral value in constant 2014 pesos, with a cutoff set at 20.09 million pesos. All estimations use a triangular kernel and cluster errors. Panel (a) shows the full range, Panel (b) focuses on the optimal bandwidth with a first-degree polynomial, and Panels (c) and (d) replicate the estimation using second and third-order polynomials for robustness. Confidence intervals (99%) are shown as shaded areas. Extreme values of the cadastral value on panel (a) were excluded from the sample for visualization purposes.

Figure 4 provides graphical evidence illustrating the effect of property tax payments on housing prices, following the optimal data-driven regression discontinuity plots proposed by Calonico et al. (2015). Panel (a) shows the main RDD estimate across the full range of the running variable, while Panel (b) focuses exclusively on the optimal bandwidth, highlighting the local discontinuity at the cutoff. Panels (c) and (d) present robustness checks using second and third-order polynomials

within the same optimal bandwidth. Across all specifications, the Figure reveals a significant increase in housing prices for properties that pay the tax. To improve visual accuracy, we trimmed the extreme values by excluding the top and bottom 10% of observations from the distribution tails. Additional graphs for the full sample and alternative specifications are presented in the Appendix, and the conclusion remains unchanged.

Table 4: Main results: Estimated impact of paying tax on prices

	(1)	(2)	(3)	(4)	(5)
RD estimate	0.0794***	0.0633***	0.0593***	0.0431***	0.0435***
Standard Error	0.0178	0.0117	0.00827	0.00718	0.00714
Bandwidth	2.286	2.286	2.286	2.286	2.286
Pre-cutoff Level	0.758	0.690	1.115	0.917	0.930
Post-cutoff Level	0.837	0.753	1.174	0.960	0.974
Total Obs.	204,828	204,828	204,828	204,828	204,828
Obs. Left of Cutoff (BW)	10,180	10,180	10,180	10,180	10,180
Obs. Right of Cutoff (BW)	9,275	9,275	9,275	9,275	9,275
FE Municipality		✓			
FE District			✓	✓	✓
FE Time			✓	✓	✓
Building covs.				✓	✓
Spatial covs.					✓
N	204,828	204,828	204,828	204,828	204,828

Note: RD estimates correspond to housing prices per sq. m., expressed in millions of constant 2014 Chilean pesos. All columns use the MSE-optimal bandwidth proposed by Calonico et al. (2014) determined in our preferred specification in column (5). The running variable is the cadastral value, and the threshold corresponds to the exemption bracket cutoff. Time-fixed effects include year and month separately. Covariates include the district-year interaction.

Building covariates: quality index, building age, and indicators for new construction and house type. Spatial covariates: distance to the nearest police station, high school, primary health facility, park, and subway line.

All specifications use a first-order polynomial and a triangular kernel. Standard errors are clustered at the district level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The results of the main estimations are presented in Table 4, using local linear regressions with the optimal bandwidth proposed by Calonico et al. (2014). Robust standard errors are clustered at the district level. As shown, the results complement the graphical evidence and are statistically significant at the 0.1% level across all specifications.

Column (1) shows the estimation without covariates. In column (2), the estimation includes municipality fixed effects. However, the precision improves when we use district fixed effects in column (3) (noting that there are multiple districts within a municipality). In column (4), we include additional property-level covariates, such as the quality index, building age, usage, and whether the property is an apartment. In column (5), we add spatial covariates, including distance to the nearest police station, high school, primary healthcare facility, park, and subway line. The

optimal bandwidth was obtained using this specification.

Our estimated effect is approximately 0.04 million pesos, meaning that properties paying taxes are, on average, more expensive. The pre-cutoff average price level at the threshold in the preferred specification is 0.93 million pesos per sq. m., indicating that prices are approximately 4% higher for properties that pay taxes. This increase implies that property taxes partially contribute to higher housing prices in Chile. Furthermore, this finding highlights the significant impact of tax policies on local housing markets, as property taxes are associated with higher transaction prices. It also raises important questions about the broader implications of such policies, particularly in regions where housing affordability is a pressing concern.

These results do not align with empirical evidence from other contexts when considering only the capitalization approach. It is important to emphasize that this analysis compares units around the cutoff within the same district, thereby excluding potential effects of differences in amenities resulting from increased public expenditure. To better understand these results, Sections 5 and 6 test potential channels and study heterogeneous effects.

4.2 Robustness checks: Threshold location and polynomial specification

This section shows that our preferred specification’s results are robust and stable to two exercises. The first exercise implements a falsification test by shifting the cutoff location. In contrast, the second examines the sensitivity of the estimates to the polynomial order used to approximate the conditional expectation functions. Together, these checks ensure that the main results are not driven by arbitrary threshold placement or functional-form assumptions.

Falsification test: alternative thresholds. We first conduct a placebo exercise in which the cutoff is artificially shifted away from the actual exemption threshold and the RDD is re-estimated at these alternative locations. If the estimated discontinuity at the accurate cutoff reflects the causal effect of tax liability, no systematic discontinuities should arise at other points of the running variable. Table 5 reports the results of this exercise. Column (1) shifts the cutoff to the left by -3 million Chilean pesos, while column (3) shifts it to the right by $+3$ million pesos. Column (2) corresponds to the actual exemption threshold.

Robustness to polynomial order. Next, we assess the sensitivity of the RDD estimates to the order of the local polynomial used to approximate the conditional expectation functions. While the local linear specification ($p = 1$) is standard in RDD applications due to its favorable bias–variance tradeoff, higher-order polynomials may overfit local noise and yield unstable estimates. Table 6 presents RD estimates obtained using local polynomials of order $p = 1$ through $p = 4$.

Table 5: Robustness: Falsification exercise by changing the threshold

	(1) −3M	(2) +0	(3) +3M
RD estimate	−0.00873	0.0435***	0.00750
Standard error	0.00748	0.00714	0.00778
Bandwidth	1.880	2.286	3.457
Pre-cutoff level	1.151	0.930	0.998
Post-cutoff level	1.142	0.974	1.005
Total observations	204,828	204,828	204,828
Obs. left of cutoff (BW)	11,349	10,180	13,589
Obs. right of cutoff (BW)	8,720	9,275	12,065

Notes: This table reports a falsification test in which the cutoff is artificially shifted along the running variable. Column (2) corresponds to the true exemption threshold, while columns (1) and (3) report placebo estimates at alternative cutoffs. In each case, the optimal bandwidth is re-estimated using the MSE-optimal procedure of Calonico et al. (2014). The estimated discontinuities at the placebo thresholds are small and not statistically significant, whereas a sizeable and statistically significant effect is observed at the true cutoff. All specifications include property characteristics, spatial controls, and district fixed effects. Standard errors are clustered at the district level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: Robustness to polynomial order

Polynomial order	(1) $p = 1$	(2) $p = 2$	(3) $p = 3$	(4) $p = 4$
RD estimate	0.0435***	0.0477***	0.0368***	0.0176
t -statistic	(6.10)	(5.37)	(3.70)	(1.58)
Standard error	0.00714	0.00888	0.00995	0.0112
Bandwidth	2.286	4.277	5.177	5.127
Pre-cutoff level	0.930	0.929	0.945	0.964
Post-cutoff level	0.974	0.976	0.982	0.982
Total observations	204,828	204,828	204,828	204,828
Obs. left of cutoff (BW)	10,180	20,657	26,428	26,104
Obs. right of cutoff (BW)	9,275	16,696	19,788	19,637

Notes: This table reports RD estimates of the effect of property tax liability on price per sq. m. using local polynomial regressions of increasing order. Column (1) reports the baseline local linear specification. Columns (2)–(4) employ quadratic, cubic, and quartic polynomials, respectively. The estimated discontinuity remains positive and statistically significant for polynomial orders up to $p = 3$. Still, it becomes smaller and statistically insignificant at $p = 4$, consistent with concerns regarding overfitting at high polynomial orders. All specifications include property and spatial covariates as well as district fixed effects. Standard errors are clustered at the district level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4.3 RDD estimates using total prices

While the previous specifications focused on prices per square meter to account for differences in property size, it is also informative to examine whether the discontinuity at the exemption threshold translates into differences in total transaction prices. This exercise addresses the concern that any effect observed in unit prices could be mechanically driven by differences in floor area rather than by the hypothesized signaling mechanism. By estimating the same regression discontinuity design using total prices as the dependent variable, we provide a complementary perspective on the magnitude and robustness of the discontinuity. If the signaling hypothesis holds, one would expect to observe a similar pattern for total prices, albeit potentially attenuated relative to prices per square meter.

Consistent with the baseline results, we find that properties subject to the property tax exhibit higher transaction prices. The preferred specification yields an estimated discontinuity of approximately 1.3 million Chilean pesos, corresponding to an increase of about 2.5 percent relative to the mean pre-threshold price.

Table 7: Regression discontinuity estimates using total property prices

	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	0.924	1.430*	1.163**	1.288***	1.163***	1.295***
Standard error	0.733	0.640	0.384	0.302	0.342	0.304
Bandwidth	3.252	2.919	3.417	3.272	2.286	2.987
Pre-cutoff level	42.38	31.41	68.55	37.61	50.29	52.19
Post-cutoff level	43.30	32.84	69.72	38.89	51.45	53.48
Total observations	204,828	204,828	204,828	204,828	204,828	204,828
Obs. left of cutoff (BW)	14,945	13,124	15,826	15,081	10,180	13,462
Obs. right of cutoff (BW)	12,880	11,630	13,452	12,971	9,275	11,854
Comuna fixed effects			✓			
District fixed effects				✓	✓	✓
Time fixed effects					✓	✓
Building covariates					✓	✓
Spatial covariates						✓
Floor area (m ²)		✓	✓	✓	✓	✓

Notes: Entries report RD estimates of the effect of property tax liability on total transaction prices, expressed in millions of constant 2014 Chilean pesos. The running variable is the cadastral value, and the cutoff corresponds to the property tax exemption threshold. All specifications employ a local linear regression with a triangular kernel.

Time fixed effects include the year and month of the transaction. Building covariates comprise the quality index, building age, and indicators for new construction and house type. Spatial covariates include distances to the nearest police station, high school, primary health facility, park, and subway line. Standard errors are clustered at the district level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4.4 Addressing composition concerns around the threshold

A potential concern in regression discontinuity designs is that the estimated treatment effect may reflect average differences in observable characteristics between treated and control units, rather than a sharp discontinuity at the cutoff. In our context, this would imply that the indicator for paying property taxes could be capturing structural differences in property valuation—even after conditioning on the running variable—rather than the causal effect of losing the exemption.

Table 8: Robustness: varying the bandwidth size around the RDD cutoff

	(1) 2×BW	(2) 1×BW	(3) 0.5×BW	(4) 0.25×BW	(5) 0.1×BW
RD estimate	0.0424***	0.0435***	0.0202*	0.00296	0.0412**
Standard error	0.00607	0.00714	0.00828	0.0117	0.0150
Bandwidth	4.572	2.286	1.143	0.572	0.229
Pre-cutoff level	0.918	0.930	0.968	1.255	1.250
Post-cutoff level	0.961	0.974	0.988	1.258	1.291
Total observations	204,828	204,828	204,828	204,828	204,828
Obs. left of cutoff (BW)	22,328	10,180	4,877	2,183	706
Obs. right of cutoff (BW)	17,833	9,275	4,825	2,634	1,046
Lower CI	0.0305	0.0295	0.00398	-0.0199	0.0119
Upper CI	0.0543	0.0575	0.0364	0.0258	0.0705

Notes: This table reports regression discontinuity estimates of the effect of property tax liability on housing prices per square meter using alternative bandwidths around the cutoff. Column (2) employs the baseline MSE-optimal bandwidth (2.286). Column (1) doubles the baseline bandwidth, while Columns (3)–(5) progressively reduce it to 0.5, 0.25, and 0.1 times the baseline. All specifications use a local linear regression with a triangular kernel and include the full set of covariates and fixed effects. Confidence intervals are reported at the 90% level. Standard errors are clustered at the district level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

To assess whether compositional differences drive our results, we conduct a robustness exercise that varies the bandwidth around the cutoff. The idea is straightforward: if the estimated effect reflects a genuine local discontinuity, it should remain reasonably stable across a range of bandwidth choices, though precision may deteriorate as the window narrows. Table 8 reports the results. Column (2) corresponds to the baseline MSE-optimal bandwidth (2.286). Columns (1) and (3) respectively double and halve this bandwidth, while Columns (4) and (5) further restrict the window to 0.25 and 0.1 times the baseline bandwidth.

Overall, the estimates display stability across bandwidth choices. When the window is reduced to one-half and one-quarter of the baseline (Columns 3 and 4), the point estimate declines and precision worsens, which likely reflects increased sampling variability and greater sensitivity to local heterogeneity. Importantly, this attenuation does not persist as the window is further narrowed. In Column (5), where the bandwidth is extremely narrow, and observations are tightly concentrated around the threshold, the estimated effect is comparable to the baseline and is statistically signif-

icant. This pattern is consistent with a genuine discontinuity at the cutoff rather than differences driven by composition away from it.

Taken together, these results suggest that the estimated discontinuity is indeed operating at the exemption threshold rather than being driven by compositional differences between exempt and non-exempt properties. The temporary attenuation observed at intermediate bandwidths likely reflects a trade-off between bias and variance. At the same time, the recovery of the effect in the narrowest window reinforces the interpretation of a genuine local treatment effect. These findings underscore the importance of careful bandwidth selection and complement our broader set of robustness checks.

4.5 Estimation using a donut RDD

To further assess the robustness of the regression discontinuity design (RDD) estimates, we implement a donut RDD by symmetrically excluding observations within a fixed distance from the cutoff. This approach addresses potential concerns about manipulation, heaping, or measurement error in the running variable near the threshold, which could bias the estimated treatment effect. By removing observations nearest to the cutoff, the donut design ensures that the discontinuity is not driven by local irregularities or endogenous sorting. At the same time, it provides a stringent test of whether the estimated effect persists when identification relies on observations slightly farther from the threshold.

Table 9 reports the results for alternative donut sizes. Each column excludes observations within an increasingly wide interval around the cutoff, ranging from 0.1 to 1.5 million Chilean pesos on each side, and re-estimates the RDD using a newly selected optimal bandwidth. For relatively small donut sizes (0.1–0.5 million pesos), the estimated discontinuity remains positive and statistically significant, with magnitudes close to the baseline estimates. As the excluded window widens, both the precision and statistical significance of the estimates decline. This pattern is consistent with the RDD setup, as identification relies on observations close to the cutoff and naturally weakens when local information is removed.

Overall, the persistence of the estimated effect for small donut sizes supports the validity of the baseline specification, while the attenuation observed for larger exclusion windows underscores the importance of local variation around the threshold for credible identification.

Table 9: Donut RDD estimates: robustness to excluding observations near the cutoff

	(1) 0.1M	(2) 0.25M	(3) 0.5M	(4) 1M	(5) 1.5M
RD estimate	0.0484***	0.0482***	0.0620***	0.0522*	0.0347
Standard error	0.00784	0.00954	0.0109	0.0213	0.0249
Bandwidth	2.889	3.015	2.401	2.861	3.377
Pre-cutoff level	0.910	0.876	0.841	1.118	1.112
Post-cutoff level	0.959	0.925	0.903	1.170	1.147
Total observations	204,217	202,864	200,720	196,277	192,118
Obs. left of cutoff (BW)	12,766	12,778	8,724	8,514	9,154
Obs. right of cutoff (BW)	11,178	10,776	7,494	7,135	7,071

Notes: This table reports estimates from a donut regression discontinuity design (RDD) in which observations within a symmetric interval around the cutoff are excluded. The cutoff corresponds to a cadastral value of 20.086 million 2014 Chilean pesos. Each column reports results for a different donut size, ranging from 0.1 to 1.5 million pesos on each side of the cutoff. For each specification, the optimal bandwidth is re-estimated using the MSE-optimal procedure of Calonico et al. (2014). All regressions use a local linear specification with a triangular kernel and include property-level covariates, spatial controls, and district fixed effects. Standard errors are clustered at the district level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4.6 Fuzzy RDD

The baseline design in this paper is a sharp regression discontinuity (RD) around the cadastral-value exemption threshold, where tax liability changes discontinuously. In the main sample used for the sharp RD, we previously excluded a small set of transactions that were *off-norm* with respect to the statutory rule—i.e., observations for which the recorded payment status did not match what the threshold rule would imply (properties below the cutoff that pay, or above the cutoff that do not pay). These cases may arise from administrative timing, reporting issues, enforcement frictions, or special legal conditions, and could mechanically complicate sharp assignment in the estimation window.

As an additional exercise, we reintroduce these off-norm observations and re-estimate the effect using a fuzzy RD framework. The fuzzy design treats tax payment as the endogenous treatment, $D_i \in \{0, 1\}$, and uses the indicator for being above the cutoff, $Z_i = \mathbb{1}\{X_i \geq c\}$, as an instrument, where X_i is the cadastral value (in constant prices), and c is the exemption threshold. This approach estimates the local average treatment effect (LATE) for those transactions whose probability of paying the tax changes at the cutoff:

$$\tau^{\text{LATE}} = \frac{\lim_{x \downarrow c} \mathbb{E}[Y_i | X_i = x] - \lim_{x \uparrow c} \mathbb{E}[Y_i | X_i = x]}{\lim_{x \downarrow c} \mathbb{E}[D_i | X_i = x] - \lim_{x \uparrow c} \mathbb{E}[D_i | X_i = x]}, \quad (1)$$

where Y_i denotes the price per sq. m. (in constant prices). Estimation follows the same local polynomial RD procedure as in the baseline results: a triangular kernel, local linear fits ($p = 1$), covariate adjustment, and district-clustered inference. This type of complementary fuzzy RD implementation is commonly used as a robustness device in applied urban and local public finance settings when assignment may be imperfect in the raw administrative data (Alves, 2025).

Table 10 reports the first stage and the implied fuzzy RD estimate. The first-stage discontinuity is significant and precisely estimated, indicating strong instrument relevance. Importantly, the fuzzy RD LATE is very close in magnitude to our preferred sharp RD estimate reported earlier (Table 4), implying that reinstating the off-norm observations and allowing for imperfect compliance does not materially change the main conclusion. Taken together, these results strengthen the interpretation that the observed price discontinuity at the exemption threshold reflects the causal effect of tax liability, rather than a byproduct of sample cleaning or deterministic assignment imposed by construction.

Table 10: Fuzzy RDD (LATE)

	(1)	(2)
	First stage (jump in D)	Fuzzy RD (LATE)
Coefficient	0.404***	0.0408***
Standard Error	0.0137	0.0101
Bandwidth	0.486	1.615
Pre-cutoff Level	0.214	0.814
Post-cutoff Level	0.618	0.843
Total Obs.	205,959	205,959
Obs. Left of Cutoff (BW)	2,125	7,171
Obs. Right of Cutoff (BW)	2,200	6,936

Description: Column (1) reports the first-stage jump in the probability of paying the tax at the cutoff (instrument relevance). Column (2) reports the fuzzy RDD local average treatment effect (LATE) of paying the tax on prices per m². The upper panel shows point estimates with t -statistics in parentheses; the lower panel lists associated scalars: standard errors, bandwidths, estimated conditional means at each side of the cutoff, and effective sample sizes within the bandwidth.

Notes. FS = first stage; LATE = local average treatment effect. Robust standard errors clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

5 Disentangling Potential Channels

To interpret the counterintuitive positive effect of tax liability on housing prices, we must first assess and rule out standard structural mechanisms that typically govern property capitalization. We systematically examine three potential alternative drivers: Tiebout-style public spending effects, preference-based segregation, and unobserved spatial heterogeneity.

5.1 Local Public Spending and Amenities

The standard capitalization framework suggests that property taxes depress prices unless offset by the value of local public goods. A potential concern is that tax revenue translates into localized amenities—such as improved security or park maintenance—that are capitalized into prices.

However, this mechanism is unlikely to drive our results for several reasons regarding our identification strategy:

- **Intra-municipal variation:** We exploit variation across properties within the same municipality during a period of stable revenue, rather than comparing changes in revenue over time or across jurisdictions.
- **Spatial granularity:** Our specification includes district fixed effects. Since districts are small

spatial units (average area: 1.82 km^2), we effectively compare neighbors who share the same public goods.

- Revenue recycling: The variation driving our estimates stems from property-specific cadastral values, not municipal budgetary shocks. There is no institutional mechanism in Chile that recycles tax revenue from a specific property back into its immediate block.

5.2 Segregation Hypothesis

We also test the hypothesis that paying property taxes serves as a status signal or a coordination device for social sorting. If homeowners prefer neighbors with similar socioeconomic status, the tax liability could act as a proxy for neighborhood affluence.

We test this by calculating the tax-liable population density in each district. If segregation preferences drive the premium, the signaling value should diminish in districts where tax liability is ubiquitous (i.e., the signal becomes redundant).

We split the sample into two groups based on the district-level share of taxpayers (below vs. above the median). As shown in Table 11, the estimated effect remains positive and statistically significant in both groups, with similar magnitudes. The absence of attenuation in high-taxpayer districts indicates that social segregation preferences are not the primary channel.

Table 11: RDD estimates by district taxpayer density (median split)

	(1) Below median	(2) Above median
RD estimate	0.0389**	0.0429**
Standard error	0.0148	0.0138
Bandwidth	2.684	2.748
Pre-cutoff level	0.577	1.056
Post-cutoff level	0.616	1.099
Total observations	103,170	101,658
Obs. left of cutoff (BW)	5,272	6,810
Obs. right of cutoff (BW)	2,685	8,220

Notes: The table reports RDD estimates of the effect of property tax liability on price per sq. m. (`precio_cte_m2`), expressed in millions of constant 2014 Chilean pesos. The sample is split by district-level tax-paying property share (below- vs. above-median). All specifications use a local linear regression with a triangular kernel, MSE-optimal bandwidth selection (Calonico et al., 2014), and standard errors clustered at the district level; t -statistics are shown in parentheses. Regressions include the same covariates and fixed effects as the baseline (property characteristics, spatial controls, and district/time fixed effects).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

5.3 Spatial Composition and Zoning

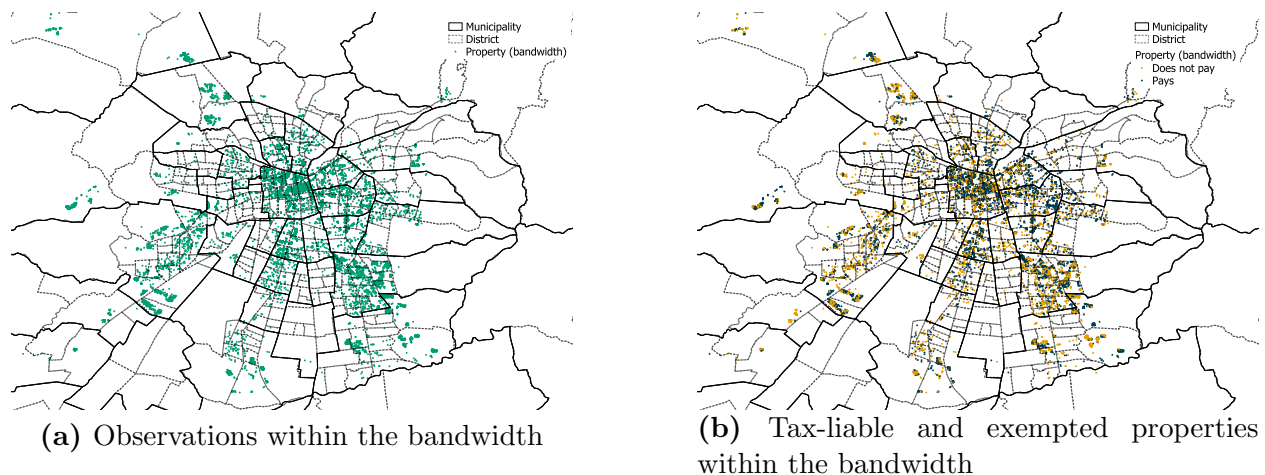
Another potential concern is that the empirical strategy may focus the analysis on specific geographic areas. If these areas—whether municipalities or districts—share particular characteristics correlated with socioeconomic status, education, or proximity to certain parts of the city, the results could reflect these local attributes rather than the mechanism under study.

To ensure that our results are not an artifact of unobserved spatial characteristics or zoning regulations specific to the bandwidth sample, we examine the geographic distribution of treated and control units.

Figure 5 shows that observations within the optimal bandwidth are broadly dispersed throughout the Greater Santiago metropolitan area rather than concentrated in specific enclaves. Moreover, both tax-liable and exempted properties are present across the city, indicating that identification does not rely on localized spatial clustering.

This spatial balance supports the interpretation that the estimated effect reflects general market dynamics rather than zoning-specific or neighborhood-level artifacts.

Figure 5: Spatial distribution of properties within the optimal bandwidth



Notes: The left panel displays the geographic distribution of properties that fall within the MSE-optimal bandwidth used in the RDD. Observations are dispersed throughout Greater Santiago rather than concentrated in a few districts. The right panel plots properties within the same bandwidth, distinguishing tax-liable and exempted units. The broad spatial presence of both groups indicates that the empirical results are not driven by zoning-specific or neighborhood-level spatial clustering. Municipal and district boundaries are included for reference.

5.4 Heterogeneity by property type

Finally, we assess whether specific property classes drive the estimated effect. Developers of new units or apartments may face pricing dynamics that differ from those relevant for sellers of older properties or single-family homes.

We stratify the sample by property type (houses versus apartments) and construction age and find that the positive price effect persists across all subsamples (Table 12). Point estimates are somewhat larger for older units and for apartments, whereas for houses the estimate is positive but imprecisely estimated, likely reflecting the smaller sample size. Overall, all four coefficients are positive and of similar order of magnitude, suggesting that the underlying mechanism is not tied to a single asset type or market segment but instead reflects a more general informational friction affecting housing transactions.

Table 12: RDD estimates by property type and age

	(1)	(2)	(3)	(4)
RD estimate	0.0194***	0.0437***	0.0196	0.0549***
Standard error	0.00518	0.0114	0.0101	0.00931
Bandwidth	0.884	3.769	2.704	2.761
Pre-cutoff level	1.164	0.750	1.145	1.167
Post-cutoff level	1.183	0.794	1.164	1.222
Total observations	82,104	122,724	93,123	111,705
Obs. left of cutoff (BW)	1,822	8,935	4,629	7,536
Obs. right of cutoff (BW)	1,928	7,412	3,664	7,257

Notes: This table reports regression discontinuity estimates of the effect of property tax liability on housing prices per square meter (in constant 2014 pesos) across subsamples defined by property type and construction age. Columns (1)–(4) correspond to: (1) new properties, (2) older properties, (3) houses, and (4) apartments. All regressions use a local linear specification with a triangular kernel. Bandwidths are selected using the MSE-optimal procedure of Calonico et al. (2014). Standard errors are clustered at the district level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6 Mechanism: taxation as a signal under asymmetric information

Having ruled out public-goods provision, sorting, and other standard channels, we propose an information-based mechanism in which property tax liability serves as a credible signal of positive local housing market dynamics. While buyers can readily observe structural characteristics and neighborhood amenities, underlying market conditions—such as local demand pressures and price trends—are difficult and costly to infer. Property tax liabilities reflect reassessments by the tax authority that incorporate recent transactions and aggregate market information beyond observable

dwelling attributes. As these reassessments are imposed by a third party and translate into real fiscal costs, they convey credible information about local market dynamics, shaping buyers' beliefs and affecting transaction prices independently of amenities or public services.

6.1 Information vs. capitalization

Housing markets are characterized by substantial information asymmetry regarding neighborhood-level market dynamics and future valuation trends. While observable attributes such as amenities and structural quality are readily verifiable, changes in local demand, price momentum, and appreciation expectations are costly for buyers to infer. In standard models, property taxes lower prices through capitalization of future tax liabilities (see Figure 6, Panel a, for a graphical depiction). We argue instead that entry into the tax system conveys positive information about underlying market dynamics, generating a countervailing signaling effect (Panel b).

Two opposing mechanisms, therefore, govern equilibrium housing prices under property taxation. The *capitalization effect* predicts that higher taxes are discounted into lower prices as buyers account for future fiscal costs. In contrast, the *signaling effect* arises because tax liability reflects reassessments by the tax authority that incorporate recent transactions and aggregate market trends beyond fixed amenities or dwelling characteristics. As a result, being taxable signals stronger underlying demand conditions.

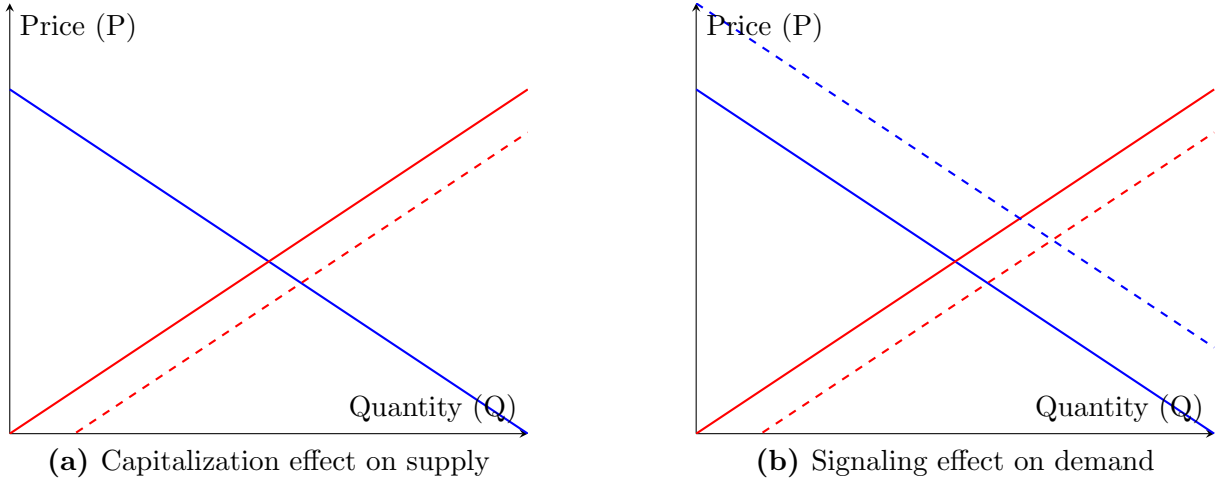
These mechanisms operate simultaneously. While higher tax payments impose a direct cost that tends to reduce prices (Panel A of Figure 6), the information conveyed by reassessment can shift demand outward by updating buyers' beliefs about local market conditions (Panel B). The net effect is therefore theoretically ambiguous and depends on the relative strength of capitalization and signaling.

6.2 Capitalization along the intensive margin

To empirically disentangle signaling from capitalization, we exploit a key feature of the Chilean property tax schedule: a second threshold at which the tax rate increases without a change in taxpayer status. Prior results show that properties crossing the exemption cutoff experience higher prices, consistent with signaling dominating capitalization at the extensive margin. We now examine whether this price premium persists when taxation increases along the intensive margin, where there is no variation in tax status, and variation operates solely through higher taxes.

The identifying intuition is straightforward. At the first threshold, entry into the tax system generates a signaling effect that dominates the associated tax cost. At the second threshold, the informational content of tax status is already revealed, so increases in the tax rate primarily operate

Figure 6: Supply and demand equilibrium



through capitalization.

Table 13 and Figure 7 report Regression Kink Design (RKD) estimates at the second threshold, using the properties that are not exempted. We find a negative, statistically significant kink in prices, indicating that higher tax rates reduce housing prices, holding other factors constant. This result is consistent with the presence of standard tax capitalization in Chile. It also supports our hypothesis that capitalization is dominated by signaling at the point of entry into taxation.

Table 13 presents the RKD estimates. Column (1) reports the baseline specification with covariates and fixed effects, while columns (2) and (3) report placebo thresholds. Only the actual threshold exhibits a significant kink, reinforcing the interpretation of a causal tax effect.

This estimate suggests that a one-unit increase in the effective tax rate is associated with a decline in property prices per square meter. The magnitude should be interpreted cautiously, as it reflects a narrow bandwidth and slight tax variation. The key result is a negative, significant effect, consistent with capitalization at the second threshold rather than signaling.

6.3 Testing the signaling hypothesis

If the price premium associated with property tax liability reflects an information signal, its effect should be strongest when market participants face greater informational frictions. To test this prediction, we construct a property-level measure of informational asymmetry based on the unexplained component of each transaction price from a saturated hedonic model:

$$P_{id} = \beta_0 + \alpha_d + \mathbf{X}_i' \boldsymbol{\beta} + \gamma \cdot \text{CadastralValue}_i + \varepsilon_{id},$$

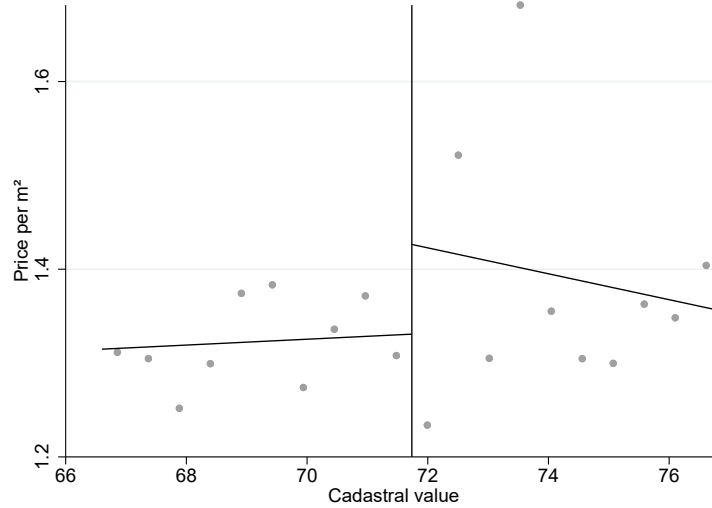
Table 13: RKD Estimates at Second Threshold

	(1)	(2)	(3)
RKD Estimate	-0.0186***	-0.00343	0.000790
Standard Error	0.00407	0.00522	0.00175
Bandwidth	7.907	5.503	16.47
Pre-cutoff Level	0.00502	-0.00309	-0.00234
Post-cutoff Level	-0.0136	-0.00653	-0.00155
Total Obs.	106932	106932	106932
Obs. Left of Cutoff (BW)	4765	4581	8434
Obs. Right of Cutoff (BW)	3788	3638	5054

Notes: This table reports RKD estimates of the effect of property taxation on housing prices per square meter (in constant 2014 pesos) at the second threshold. Columns (2)–(3) correspond to different bandwidth choices, showing robustness of the estimates. All regressions use a triangular kernel, a first-degree polynomial, and robust standard errors clustered at the district level. Covariates include property characteristics (quality index, building type, age, condition, apartment status), spatial controls (distance to police stations, schools, health facilities, parks, subway lines), and district fixed effects. Bandwidths are selected using the MSE-optimal procedure (Calonico et al., 2014).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 7: Unconditional RKD estimation. The kink at the threshold reflects the change in slope induced by the higher tax rate.



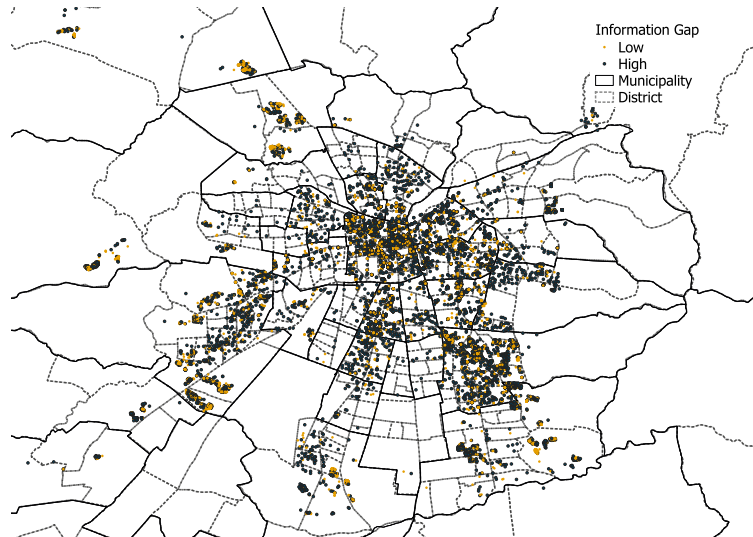
where P_{id} denotes the transaction price of property i in district d , α_d are district fixed effects, and $\mathbf{X}_i$ includes detailed property characteristics (age, type, condition, quality indices) and fine spatial controls (distances to amenities, schools, and transit). We explicitly control for the cadastral value to mitigate omitted-variable concerns related to neighborhood fundamentals.⁶

The absolute residual from this regression, $|\hat{\varepsilon}_{id}|$, captures the portion of the property's price that is not explained by the rich set of observable attributes. We interpret larger residuals as indicating transactions in which prices are harder to predict from publicly observable information, reflecting greater informational frictions faced by market participants regarding local market dynamics. To obtain a scale-free measure, we normalize the residual by the property's transaction price:

$$\text{InformationGap}_{id} \equiv \frac{|\hat{\varepsilon}_{id}|}{P_{id}}.$$

We sort properties according to the size of $\text{InformationGap}_{id}$; we construct quartiles and octiles, where belonging to a higher quartile or octile is indicative of transactions occurring in contexts with a larger information gap about neighborhood quality. We then estimate the same regression discontinuity design (RDD) within each subsample.

Figure 8: Spatial distribution of the information gap



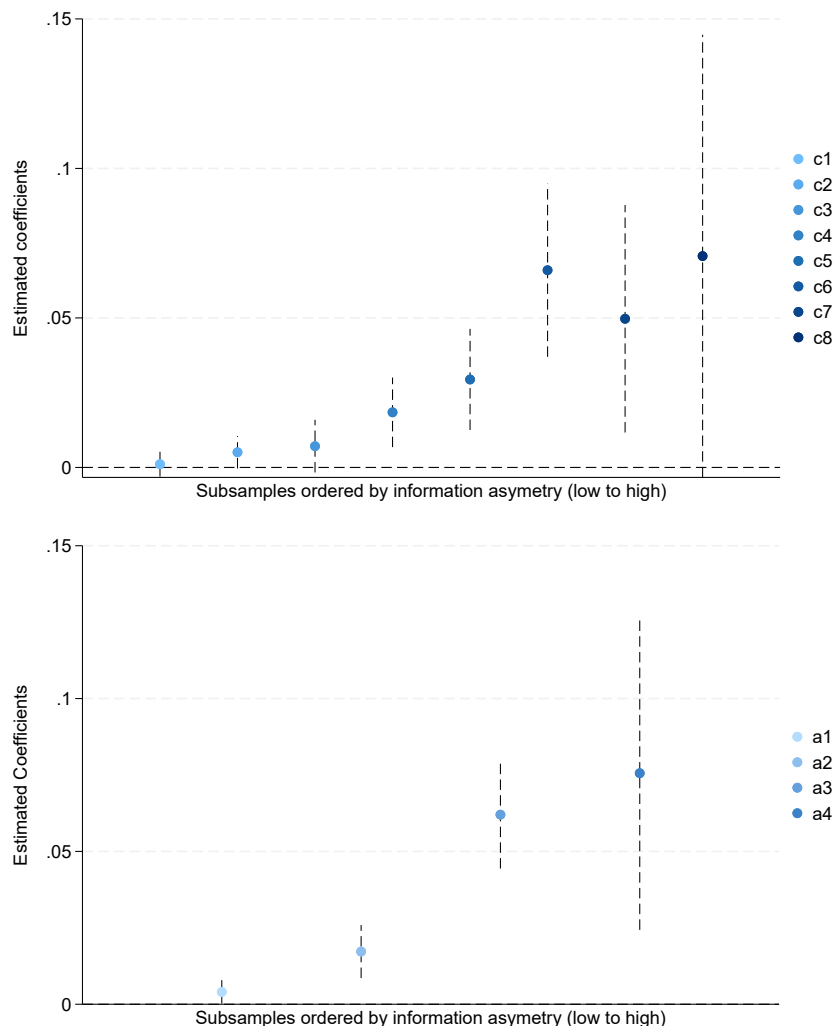
Notes: This figure displays the spatial distribution of units classified around the sample median into two groups: high information gap and low information gap. The classification uses 20,557 observations within the optimal bandwidth.

Figure 9 and Table 14 show a clear monotonic pattern. When prices are well explained by observables, the estimated treatment effect of tax liability is small and statistically insignificant.

⁶A potential concern is that, if omitted, the cadastral value may correlate with the error term and bias the estimated effects. We address this by including the cadastral value as a regressor. Incorporating it strengthens the credibility of our estimates by mitigating this source of bias. Estimation results are reported in Table A2.

As informational frictions increase, the price effect rises sharply.

Figure 9: Heterogeneous effects of property taxation by neighborhood information asymmetry



Notes: RD estimates segmented by the neighborhood information asymmetry proxy. Top: eight octiles. Bottom: quartiles. Estimates increase with asymmetry, consistent with the signaling hypothesis.

These results are consistent with property tax liabilities—arising from administrative reassessments that aggregate local market information—serving as a credible signal when prices are otherwise difficult to predict using observable characteristics. Where informational frictions are most severe at the property level, this signal is most valuable and has the most significant impact on transaction prices.

This heterogeneity provides strong causal evidence: the tax liability serves as a “certification” that is highly valued only when the market lacks other reliable quality indicators.

Table 14: RD Estimates by levels of neighborhood information asymmetry

Panel A: Asymmetry octiles (1=Low, 8=High)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	0.00109	0.00507	0.00712	0.0184**	0.0294***	0.0660***	0.0497*	0.0707
Standard Error	0.00212	0.00276	0.00449	0.00594	0.00863	0.0148	0.0194	0.0378
Bandwidth	2.213	2.565	3.285	3.210	2.766	2.893	3.294	2.984
Pre-cutoff Level	1.059	1.116	1.036	1.086	1.088	1.122	1.203	0.826
Post-cutoff Level	1.060	1.121	1.044	1.104	1.118	1.188	1.252	0.897
Total Obs.	24,579	24,579	24,580	24,579	24,579	24,580	24,579	32,773
Obs. Left of Cutoff (BW)	1,314	1,558	2,070	2,003	1,491	1,344	1,405	1,876
Obs. Right of Cutoff (BW)	1,272	1,359	1,862	1,751	1,254	1,380	1,249	1,376

Panel B: Asymmetry quartiles				
	Q1 (Low)	Q2	Q3	Q4 (High)
RD Estimate	0.00404*	0.0172***	0.0620***	0.0756**
Standard Error	0.00196	0.00442	0.00900	0.0261
Bandwidth	2.132	2.346	2.436	3.439
Pre-cutoff Level	1.090	1.126	1.121	0.821
Post-cutoff Level	1.094	1.143	1.183	0.896
Total Obs.	51,207	51,207	51,207	51,207
Obs. Left of Cutoff (BW)	2,685	2,931	2,457	3,232
Obs. Right of Cutoff (BW)	2,505	2,800	2,359	2,453

Notes: The signaling effect of property taxation is concentrated in high-asymmetry neighborhoods. Specifications as in Table A2. Bandwidth selection via MSE-optimal procedure (Calonico et al., 2014). Robust standard errors clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6.4 Discussion on the Information Channel

Why does tax liability carry information if cadastral values are public? The key distinction is between *availability* and *binding salience*. Cadastral values are observable but often stale, are poorly attended by buyers/sellers, and are more costly to verify. By contrast, tax liability makes the assessment *binding*: crossing the exemption threshold implies that the SII has recently reassessed the property and deemed its value above the taxable cutoff, aggregating current market signals from transactions and local dynamics.

This binding status increases the credibility and salience of the assessment at the point of sale. Once a property is taxable, the assessed value carries legal and fiscal consequences, is more likely to be verified or audited, and serves as a focal point in negotiation and due diligence. These factors reduce uncertainty about valuation accuracy and exposure to future liabilities, thereby tightening buyers' beliefs about underlying market conditions relevant to the asset's price.

We illustrate this with a hedonic regression estimated within a narrow bandwidth around the exemption threshold, including both the cadastral value and an indicator for tax liability (Table 15).

Both coefficients are precisely estimated, but the tax-liability indicator is more than twice as

Table 15: Hedonic regression around the exemption threshold

	Price per m ²	Standard Error
Cadastral value (C)	-0.021***	(0.003)
Pays property tax (dummy)	0.052***	(0.008)
Observations	19,455	

Robust standard errors in parentheses. Additional controls omitted for brevity.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

large as the cadastral coefficient. This result suggests buyers place greater weight on the *binding* nature of the reassessment (taxable status) than on the raw assessment level. In short, tax liability functions as a certification device that converts diffuse, low-salience public information into a credible, transaction-relevant signal, operating alongside (and sometimes offsetting) standard capitalization.

7 Final remarks

This study advances our understanding of property tax capitalization by uncovering its dual role in Chile’s housing market: as a financial burden that influences ownership costs and as a signal that mitigates information asymmetry. The empirical findings challenge traditional views by demonstrating that property tax payments can elevate housing prices in contexts of high price uncertainty, acting as a form of third-party certification that fosters buyer confidence. These insights highlight the relationship between tax policies, market transparency, and housing affordability.

The policy implications of these findings are substantial. Policymakers could leverage the signaling effect of property taxes to address market inefficiencies, particularly in regions where affordability pressures and information gaps are acute. For instance, targeted tax incentives or certifications linked to tax liability could enhance the role of property taxes in fostering trust and reducing uncertainty, while balancing equity and revenue-generation objectives. However, such measures must be carefully designed to prevent exacerbating disparities or affordability challenges.

Future research could extend these insights in several ways. First, cross-contextual studies are needed to examine whether the signaling effect of property taxes is consistent in housing markets with differing institutional frameworks, tax structures, and socioeconomic dynamics. Second, longitudinal analyses could examine how changes in tax policies affect buyer behavior, market stability, and the long-term incorporation of taxes into housing prices. Third, exploring the broader externalities of property taxes—such as their impact on urban planning, segregation, and public service financing—could provide a more comprehensive view of their role in shaping housing markets.

Finally, further investigations could investigate the feedback mechanisms between property taxes and market dynamics. For instance, how do rising housing prices driven by the signaling effect influence future tax policies or public perceptions of tax fairness? By addressing these questions, future research can offer deeper insights into the design of property tax systems that balance revenue generation, affordability, and market transparency.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process.

Statement: During the preparation of this work, the authors used Gemini and ChatGPT to identify typos, detect articles that were cited but not in the reference list and vice versa, and improve grammar. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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A Appendix

A.1 Descriptive statistics: complete dataset

Table A1: Descriptive statistics by property tax payment status

	Total Properties		Exempted		Non-exempt	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Price per sq.m.	0,75	0,48	0,56	0,37	1,02	0,48
Cadastral value	21,34	16,22	10,43	4,70	37,15	13,77
Annual Tax	0,08	0,13	0,00	0,00	0,20	0,13
New	0,40	0,49	0,36	0,48	0,47	0,50
House	0,45	0,50	0,56	0,50	0,31	0,46
Age	26,25	18,68	28,05	17,42	23,10	20,32
Floorspace	63,02	30,27	53,55	21,92	76,75	35,05
Quality index	3,24	0,73	3,62	0,58	2,69	0,56
Observations	204,828		121,175		83,653	

Notes: The summary statistics are based on pooled cross-sectional data from 2014 to 2017. Prices and cadastral values are expressed in constant Chilean pesos (2014). Columns report the mean and standard deviation (SD) for each variable across three groups: the total sample, properties exempt from property tax, and non-exempt properties. Variables include price per sq. m. (**Price per sq.m.**), which is also the dependent variable; the assessed cadastral value (**Cadastral value**); total annual property tax (**Tax**); a dummy variable indicating whether the property is new (**New**); a dummy indicating whether the property is a house (1) or an apartment (0) (**House**); the building's age (**Age**); constructed area in square meters (**Floorspace**); and a quality index ranging from 1 to 5 (**Quality index**).

Table A2: Hedonic regression using price per sq. m. as the dependent variable

Variable	Coefficient (SE)	
Cadastral value	−0.002***	(0.000)
Age	0.003***	(0.001)
House (vs. apartment)	0.107***	(0.009)
New unit	0.033***	(0.009)
Construction quality: Medium-high	−0.042***	(0.012)
Construction quality: Medium	−0.199***	(0.014)
Construction quality: Medium-low	−0.326***	(0.018)
Construction quality: Low	−0.209***	(0.022)
Transaction year: 2015	0.184	(0.317)
Transaction year: 2016	0.359	(0.630)
Transaction year: 2017	0.507	(0.947)
Month: February	0.008*	(0.004)
Month: March	0.008*	(0.004)
Month: April	0.010*	(0.004)
Month: May	0.014***	(0.004)
Month: June	0.015***	(0.004)
Month: July	0.013***	(0.004)
Month: August	0.002	(0.004)
Month: September	0.017***	(0.004)
Month: October	0.024***	(0.004)
Month: November	0.032***	(0.004)
Month: December	0.022***	(0.004)
Distance to metro (m)	−0.000	(0.000)
Distance to parks (m)	0.000***	(0.000)
Distance to police station (m)	0.001	(0.003)
Distance to health center (m)	0.027***	(0.003)
Distance to primary school (m)	0.046***	(0.004)
Group (year × district)	−0.000	(0.001)
Constant	1.311***	(0.022)
Observations	204,828	

Notes: Robust standard errors in parentheses. The regression includes controls for district, omitted for brevity.

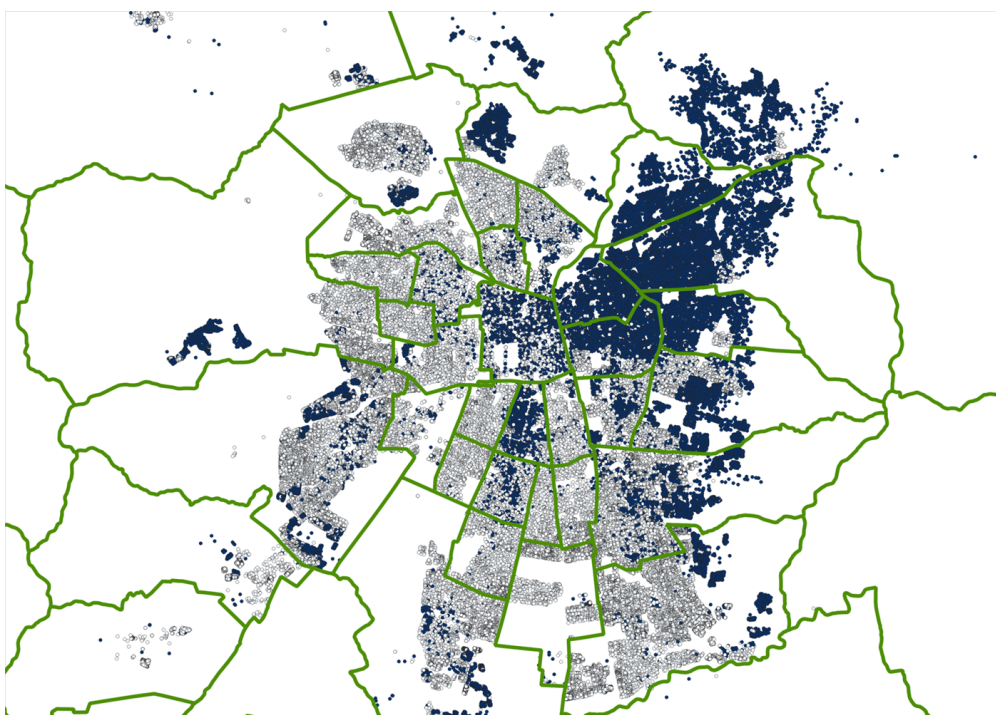
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A1: Spatial distribution of home prices per sq. m. (2014–2017)



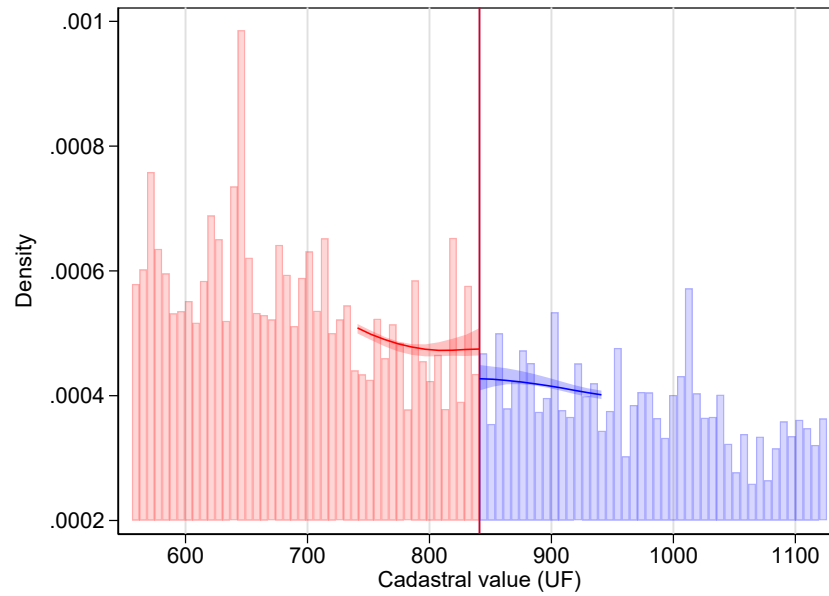
Notes: Each dot represents a home purchased from 2014 to 2017, with color intensity increasing as the purchase price rises. Darker shades indicate higher prices, and orange lines represent county boundaries.

Figure A2: Distribution of homes by property tax payment status (2014–2017)



Notes: Each dot represents one home purchased from 2014 to 2017, with a total of 233,057 homes. The orange lines indicate county boundaries. Blue dots represent properties that pay property tax, while white dots represent those that do not.

Figure A3: Cattaneo et al. (2018) density test: Bunching around threshold



Notes: The figure shows the density of the running variable (cadastral value) estimated around the cutoff point (vertical line). Following Cattaneo et al. (2018), the test shows some evidence of manipulation in the running variable near the cutoff. However, the test is sensitive to the selection of bandwidth and the polynomial order.