Value-Added Tax Incidence in Housing Markets*

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Abstract

We provide the first estimates of VAT incidence in the housing market, exploiting a 2016 reform in Chile that introduced a 19% tax on sales by developers while exempting resales by individual homeowners. Using rich administrative data covering nearly the universe of residential transactions, we estimate a difference-in-differences design that leverages the differential tax treatment across seller types. Apartment prices per square meter rose by approximately 11.8% within three years, with consumers bearing 75% of the effective tax rate. This increase combines two margins: an 8.5% rise in nominal prices and a 3.2% decline in unit sizes, a form of *housing shrinkflation* visible in construction permits before any price response. Despite the large tax, transaction volumes did not decline. Purchase contracts surged by 33% before implementation and fell by a comparable amount immediately after, indicating pure re-timing rather than a permanent demand response. The tax raised revenue without reducing market activity, but at the cost of higher prices and smaller apartments for buyers.

JEL Classification: H22, H25, R21, R31

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1 Introduction

Housing is the single largest expenditure item for most households and their primary store of wealth, yet governments differ sharply in how they treat it under the Value-Added Tax[1]. Some countries tax new housing at the standard rate, others apply reduced rates, and many exempt it entirely.[2] These choices are contested and actively revisited: Greece recently suspended VAT on new construction (Investa Real Estate, 2024), Hungary reduced its rate to 5% (KCG Partners Law Firm, 2024), Ireland cut VAT on new apartments in 2025, and Romania raised its standard rate as part of fiscal consolidation (Vertex Inc., 2025). The Value-Added Tax is among the most widely adopted tax instruments, present in over 170 countries, raising 21% of total tax revenue on average across the OECD and nearly 40% in Chile (OECD, 2024b). Yet despite the importance of both, there is no causal evidence on how VAT affects housing markets. Whether extending the VAT base to new housing is an effective revenue instrument, and who ultimately bears the burden of the tax, are open empirical questions.

In this paper, we provide the first estimates of VAT incidence in the housing market exploiting Chile's 2014 reform, which introduced a 19% VAT on sales of new properties by developers, effective January 2016, while leaving homeowner resales untaxed. The reform is well suited for estimating tax incidence and behavioral anticipatory effects. We document two set of results.

First, despite a 15.7% effective tax increase, the number of new-housing title trans-

[1] In OECD countries, housing-related expenditures averaged 22.5% of final household consumption in 2022 (OECD, 2024a). Housing accounts for more than half of total net worth for middle-class households across the OECD and exceeding 80% in countries such as Chile, Greece, and Latvia (OECD, 2022b)

[2] Denmark and the Netherlands tax new housing at the standard rate. Belgium, Ireland, and Spain apply reduced rates. Austria, Sweden, Finland, Portugal, and Germany generally exempt new housing. See Cnossen (2011) for an overview. Canada applies a reduced rate.

fers rose from 37,463 per year on average before the reform to 51,117 in 2016–2019. There was no market collapse. We also show descriptively that construction permits rose in anticipation of the tax. We conclude that the transaction anticipation is a pure re-timing response concentrated in the months surrounding the implementation date. Using monthly purchase-contract data and following the bunching methodology of Chetty et al. (2011) and Kleven and Waseem (2013), we estimate that buyers pulled forward contracts equivalent to approximately three months of normal transaction volume, an excess of 33.5% above the counterfactual (2.14% per percentage point of VAT). The estimated missing mass after equals the excess mass before it, confirming pure re-timing with no net demand destruction. The response is nonetheless modest relative to comparable settings: Fritzsche and Vandrei (2019) estimates 43% more transactions before an increase of one percentage point of the german real estate transfer tax. Additionally, our estimate corresponds to a timing elasticity of 0.84 weeks per percentage point of VAT, compared to approximately three weeks per percentage point for UK stamp duty changes (Best and Kleven, 2018a) and a monthly sales increase of 2.5% per percentage point in the month preceding a EU VAT changes (Buettner and Madzharova, 2021) .

Second, the differential treatment of developer sales and homeowner resales creates comparable groups of transactions which we exploit in a dynamic difference-in-differences design. We compare the evolution of transaction prices, property sizes, and prices per square meter, in narrow geographical areas using administrative microdata from the Chilean Internal Revenue Service covering nearly the universe of real-estate transactions from 2010 to 2022. Apartment prices per square meter rose by approximately 11.8% within three years of the reform relative to the resale control group, against a full pass-through benchmark of 15.7%.[3] Consumers therefore

[3]Because the VAT applies only to the construction component of the sale price, the effective rate is

bore approximately 75% of the effective tax burden during the three years after the implementation. Most pre-reform coefficients are non-significant and close to zero validating the parallel-trends assumption. We also verify that spillovers from treated to control transactions are absent: resale prices in census tracts with high exposure to new construction show no differential post-reform dynamics relative to low-exposure tracts.

The 11.8% increase reflects two distinct margins of adjustment. Unit prices rose by approximately 8.5%, accounting for about three-quarters of the total effect. The remaining quarter is explained by a 3.2% decline in average apartment sizes, equivalent to roughly 2 square meters. Consumers thus paid higher prices for smaller apartments. This is not a demand-side response: construction permit data show that the median approved unit size began declining immediately since 2014. It is possible that developers reduced unit sizes to limit the nominal price increase in sale prices. This behavior is analogous to shrinkflation in consumer goods markets (Janssen and Kasinger, 2026; Kim, 2024) and implies that price-based pass-through estimates understate the true burden on consumers. We also explore whether incidence varies with local supply conditions but do not find statistically significant differences across groups.

This paper makes three contributions. The first is to the empirical literature on housing taxation (Dachis et al., 2012; Besley et al., 2014b; Kopczuk and Munroe, 2015; Slemrod et al., 2017; Best and Kleven, 2018a; Eerola et al., 2021; Hilber and Lyytikäinen, 2017). Prior work has focused on transfer taxes and stamp duties, whereas we estimate the effect of a VAT which previous literature has documented is less prone to evasion (Pomeranz, 2015) and applies only to new housing devel-

19% times one minus the land-value share: approximately 15.7% for apartments which are our main focus.

opments. We show that buyers and sellers react to the announcement of a new tax by pulling forward construction and transactions. This policy generates behavioral distortions in timing but not in total market activity. Moreover, the Chilean VAT applies the construction value of new properties with no price threshold, in contrast to previous studies where the incidence is measured local to a price notch. Our results contrast previous findings of large to full incidence on sellers Besley et al. (2014b); Dachis et al. (2011); Kopczuk and Munroe (2015). While transfer taxes apply also to resale transactions involving individual household sellers negotiating bilaterally with buyers, our setting involves competitive developers setting prices in a new-construction market highlighting that the market structure matters for tax incidence. Finally, previous literature conjectured that developers may adjust property characteristics in response to housing taxes, noting weaker price distortions for newly built properties (Kopczuk and Munroe, 2015); we provide the first direct evidence and measure its magnitude with respect to unit price changes, which ultimately are leading the response.

The second is the literature on VAT pass-through to consumer prices (Carbonnier, 2007; Crossley et al., 2014; Kosonen, 2015; Benzarti and Carloni, 2019; Benzarti et al., 2020; Gaarder, 2019; Fuest et al., 2025; Buettner and Madzharova, 2021; Benzarti et al., 2024). We extend this literature to housing, the largest household consumption category. Existing evidence comes exclusively from the retail and services sectors, housing differs from those settings as transactions are infrequent and involve high stakes and extensive search, and supply adjusts slowly. Evidence from VAT increases is more limited than decreases but points towards higher pass-through for increases than decreases Benzarti et al. (2020). Our estimate of 75% for a VAT increase is consistent with this up- per range compared to changes in other settings and sectors. Furthermore, our transaction-level data reveal that developers

adjusted unit sizes alongside prices as theorized in (Chalioti and Serfes, 2024). Once size adjustment is accounted for, pass-through is higher, suggesting that price-based estimates may understate the true consumer burden.

Finally, we contribute to the empirical literature on pre-announced changes in consumption taxation Buettner and Madzharova (2021); Cashin and Unayama (2016); Crossley et al. (2014); Baker et al. (2021); Cashin (2017). This literature shows that announced tax increases induce consumers to shift expenditures forward, with the magnitude depending on goods storability, transaction costs, and the length of the announcement window. Our setting provides a test of these mechanisms for the largest single purchase most households ever make: a 15.7 effective percentage point VAT increase announced 15 months in advance, with liability determined by contract date rather than delivery. Our evidence is consistent with the re-timming of purchases, however, differs from other settings due to the size of the response. Our results highlight that besides the size of the tax, market frictions are an important determinant of the response by limiting the intertemporal substitution response to the pre-announced tax consistent with Cashin and Unayama (2016) in the durables market.

The remainder of this paper is organized as follows. Section 2 describes the institutional background and data. Section 3 outlines the tax incidence model and measurement framework. Section 4 details the empirical strategy and the main results. Section 5 show an heterogeneities analysis. And section 6 concludes.

2 Institutional Background and Data

2.1 VAT in Chile and TaxReform

VAT is the primary consumption tax in Chile and, by 2019, accounted for approximately 40% of total tax revenues. The standard VAT rate in Chile is 19% (OECD, 2022a), applied as a percentage of the selling price of goods and services net of input costs.

In September 2014, the Chilean government approved a fiscal reform aimed at increasing tax revenues by 3 percentage points of GDP, improving the equity of the tax system, and reducing tax evasion and avoidance (Ministerio de Hacienda, 2014). Prior to the reform, Chile was facing an economic slowdown driven by declining international demand for commodities.[4] Among its several components[5], the reform targeted the sales from the real estate sector, which had long enjoyed a VAT exemption that effectively subsidized new housing consumption relative to other goods and services.

This paper focuses on one key element of the reform: the introduction of VAT on residential real estate sales. The government projected that the extension of VAT to real estate would generate additional revenues equivalent to 0.36% of GDP (Ministerio de Hacienda, 2014). Figure 1 shows yearly revenues collected under the tax. During the first two years of implementation, the tax accounted for more than 0.2% of total VAT revenue; once transition exemptions expired, this share rose to as much as 0.8%, confirming that the measure had substantial fiscal consequences.

[4]Chile is the world's largest producer of copper, accounting for approximately one-quarter of global supply. The government expected the reform to help finance a significant expansion in social expenditures (Ministerio de Hacienda, 2014).

[5]The reform also included changes to income taxes, consumption taxes on goods harmful to health, and taxes on pollutant emissions.

For real estate transactions, the tax base is calculated as 19% of the difference between the sale price of the property and the assessed land value (Biblioteca del Congreso Nacional de Chile, 2014). Land is excluded from the tax base because it is not a produced good and its value does not reflect construction activity. The effective tax rate therefore depends on the land-value share of the total property value and varies across property types and locations. For apartments, where land accounts for a relatively small fraction of total value, the effective rate is approximately 15.7%. For houses, where land represents a larger share of total value, the effective rate is lower and more variable. This distinction motivates our primary focus on apartment sales, which face a higher and more uniform effective tax rate and therefore provide the cleanest test of VAT incidence.[6]

The statutory incidence of the tax falls on producers, that is, the legal obligation to remit the tax rests with the seller. Whether the economic burden is ultimately borne by sellers or passed forward to buyers depends on market structure, the relative elasticities of supply and demand, the direction of the tax change, and tax salience and compliance is the central empirical question of this paper.

Before the reform, all residential property transactions to final consumers were exempt from VAT[7]. The reform imposed VAT exclusively on sales by firms or individuals engaged in the business of selling residential properties, referred to here

[6] In this context apartments are residential properties within a building sharing common spaces with other units, whereas, houses are generally detached constructions on own lot.

[7] In Chile, residential construction generally involves two distinct firm types: construction companies (empresas constructoras), which built properties and sold them to real-estate developers (empresas inmobiliarias), who then resold them to final consumers. Construction companies charged a VAT on their sales to developers, but since developers' sales to consumers were exempt, this input VAT became a non-recoverable cost embedded in the final sale price. To partially offset this burden, construction companies could deduct 65% of their VAT debit from income tax obligations (Crédito Especial Empresas Constructoras, CEEC), subject to a per-unit cap and an eligibility threshold progressively reduced from 4,500 UF to 2,000 UF between 2015 and 2017. The 2014 reform made developers liable for VAT on final sales, converting the previously irrecoverable input VAT into a recoverable credit.

as frequent sellers, a category that encompasses real estate developers and professional property investors[8]. Sales by non-frequent sellers, e.g. households selling their own residential property who are not engaged in the real estate business, remained exempt from VAT (Biblioteca del Congreso Nacional de Chile, 2014; Ministerio de Hacienda, 2014). This differential treatment is the foundation of our empirical strategy: we compare the evolution of prices and transaction volumes over time for properties sold by frequent sellers (treated) and non-frequent sellers (control) using a difference-in-differences design. Under the assumption that both groups would have followed parallel trends in the absence of the reform, any divergence in outcomes after January 2016 identifies the causal effect of the VAT.

Figure 2 illustrates the implementation timeline. The reform was approved by Congress in September 2014, establishing a 15-month anticipation window before the January 2016 effective date, a feature that is central to our analysis of pre-reform timing behavior. The government also introduced several transition exemptions that shaped the pattern of implementation. For frequent sellers, properties with construction permits issued before the reform were exempt from VAT if sold before the end of 2016; from 2017 onward, all properties sold by frequent sellers became subject to VAT regardless of permit date. Sales contracts signed before January 2016 were also exempt from VAT, and properties purchased with a housing voucher were not subject to VAT irrespective of seller status. Together, these transition rules imply that 2016 was a period of partial implementation, as many new-housing transactions completed that year remained covered by pre-reform permits or contracts. Full implementation effectively began in 2017, which is reflected in the time pattern of our estimated price effects.

[8] An individual can be presumed to be a frequent seller if they sell a property for profit within 365 days of its purchase

Consistent with this incentive, anecdotal evidence suggests that in the months leading up to implementation, real estate developers launched extensive advertising campaigns urging potential buyers to sign before December 31, 2015. Figure A.1 presents two representative examples: an online advertisement reading "Last Months: Take advantage and buy VAT free," and a construction-site banner stating "Buy today VAT free." These campaigns reflect a supply-side awareness of the tax incentive and an active effort to translate it into accelerated demand. Complementing this supply-side evidence, online search data confirm that public awareness of the reform was widespread on the demand side as well. Figure A.2 shows the evolution of search interest related to the VAT policy change. Search activity remained low following the September 2014 announcement but increased sharply in the months immediately preceding implementation, indicating that potential buyers were actively seeking information about the financial implications of the reform. Together, the advertising evidence and the search data establish that the reform was both salient and anticipated.

2.2 Data

Our analysis draws on four administrative and institutional datasets that together cover nearly the universe of real estate transactions in Chile between 2010 and 2022.

SII Cadaster: The Internal Revenue Service (SII)[9] maintains a property-level cadaster of the entire stock of real estate in Chile, updated semi-annually for property tax assessment purposes. Each record contains a unique national property identifier, the property's address, and a rich set of physical and administrative characteristics including property size, construction materials, property type (residential or com-

[9] In Spanish: *Servicio de Impuestos Internos* (SII).

mercial), year of construction, land area, property tax liability, and appraised value. We use cadaster records from 2009 to 2022, which we match to transaction data using the unique property identifier to obtain time-invariant property characteristics at the time of each sale. As of the second half of 2022, the registered stock of residential properties in Chile comprised 5,735,002 units.

Property Sale and Registration Declaration (Form 2890): Our primary source of transaction-level data is Form 2890, collected by the SII. This form is filed at the time of deed signing and records the transaction value, the exact transaction date, covering nearly the universe of real estate transactions in Chile from 2010 to 2022.[10] Transaction values in Form 2890 are reported in Chilean Pesos (CLP), Unidades de Fomento (UF), or both. The UF is an inflation-indexed unit of account widely used in Chilean real estate and financial markets.[11] When UF values are unavailable, we convert from CLP using the Central Bank of Chile's daily exchange rate (Banco Central de Chile, 2025). All primary outcome variables are expressed in UF, so that estimated price changes are interpreted in real terms.

A limitation of our data is the absence of direct information on seller characteristics. As discussed in Section 2.1, VAT liability applies exclusively to sales by frequent sellers while resales by private households remain exempt. Since seller status is not directly recorded in Form 2890, we construct a proxy for treatment assignment based on observable transaction and property characteristics. Our classification exploits an empirical regularity in the Chilean housing market: newly constructed units are

[10]The transaction date recorded in Form 2890 corresponds to the signing of the final deed (*escritura*). For new construction, buyers and sellers typically agree on price and terms in a preliminary purchase agreement (*promesa de compraventa*) signed months or even years earlier. Once the deed is formalized, it must be registered with the Conservator of Real Estate (*Conservador de Bienes Ra'ices*, CBR), which reports transaction details to the SII.

[11]The UF is published daily by the Central Bank of Chile and adjusts with the consumer price index (CPI). In 2014, the year the reform was announced, the average UF value was approximately 24,000 CLP.

mostly sold by real estate developers, who by definition qualify as frequent sellers, while second-hand properties are predominantly transacted by private households who do not meet the frequent seller criteria. To classify transactions, we follow the methodology of the Central Bank of Chile for constructing the national housing price index (Banco Central de Chile, 2024), which distinguishes between new and used properties based on transaction and property registry records. Specifically, we tag a transaction as a *new* (treated) sale if it constitutes the first recorded sale of a given property identifier and occurs in the same year as construction or within two years thereafter; all subsequent involving the same property and remaining transactions are tagged as *resales* (non-treated) . This taxonomy maps directly onto the VAT treatment structure of the reform: new sales correspond to developer transactions subject to VAT, while resales correspond to homeowner transactions that remain exempt.

Construction Permits: This dataset provides annual counts of the number of units approved for construction per project, along with total construction size, construction type, and location. It is provided by the National Institute of Statistics of Chile (INE). We focus on residential building permits, which record the number of apartment units approved for construction.

Residential Purchase Contracts: This dataset records annual new-unit purchase contracts for apartments nationwide, reported by the Chilean Chamber of Construction (*Camara Chilena de la Construccion*, CChC).

Administrative Boundary Shapefiles: We use two sets of geographic boundary files to assign properties to administrative units. The first contains municipality (*comuna*) boundaries for all 346 Chilean municipalities. The second contains census tract (*distrito censal*) boundaries delineated for the 2017 Chilean Population and Housing

Census. Census tracts are the finest territorial subdivision used by the National Institute of Statistics (INE) for census data collection purposes; they are defined below the municipality level and designed to contain a roughly uniform number of dwellings, facilitating comparisons across areas with different population densities.[12] To assign each property to its corresponding census tract, we geocode the property addresses recorded in the SII cadaster using the geographic coordinates contained in the cadaster records, and spatially match each property to the census tract polygon within which it falls.

2.3 Sample Selection and Descriptive Statistics

Our main estimation sample consists of transaction-level observations of apartment sales drawn from Form 2890 for the period 2010–2022 matched to property level characteristics from the cadaster. We apply sample restrictions in four steps. First, we apply standard data quality filters following Banco Central de Chile (2024). We retain only transactions with positive recorded prices and exclude: transactions associated with social housing projects, which are subject to separate market prices; transactions below 100 UF or above 100,000 UF, which likely reflect data entry errors; properties smaller than 20 m^2 or larger than 500 m^2, which fall outside the range of standard residential apartments; and transactions where the recorded sale date precedes the property's construction year, which are inconsistent and likely reflect administrative errors. We also remove transactions with multiple records within the same month for the same property to eliminate likely duplicates, and exclude properties with more than four recorded transactions over the sample period.

Second, we address a contamination concern specific to our difference-in-differences

[12] The 2017 census delineated 2,771 census tracts nationally, classified as urban, rural, or mixed.

design. Because treatment is absorbing, resales of post-reform new properties may have capitalized the VAT into their resale price. Including such resales in the non-treated control group would bias our estimates of the VAT effect downward by contaminating the control group with VAT-inclusive prices. We therefore exclude from the control group all resales of properties built after January 2016 that were resold after the tax took effect.

Third, we improve the comparability of treated and control transactions. New apartments sold by developers tend to be recently built, while the stock of resale properties spans a much wider range of construction vintages. To ensure that the control group reflects properties of similar age and quality as the treated group, we restrict resales to properties built after 2005. We also exclude transactions from rural census tracts.

Fourth, we trim the price distribution to address the skewness of apartment sales values. We remove transactions where the price per square meter, the total price, or the property size falls below the 1st or above the 99th percentile of the within-treatment-group distribution. This trimming is applied separately for new and resale transactions to avoid mechanical differences in the support of the two distributions.

Our final sample contains 693,145 transactions spanning 2010–2022, located across 740 distinct census tracts. Table 2 reports summary statistics for the final sample separately for new and resale transactions. New apartments sell at a slightly higher price per square meter than resales, with mean log price per m^2 of 3.56 versus 3.51, while mean log sale prices are marginally lower for new units (7.55 versus 7.57), reflecting the fact that new apartments are on average smaller. Specifically, new apartments have a mean size of 57.6 m^2 compared to 62.3 m^2 for resales. Appraised

values per square meter are higher for new units (log value 2.99 versus 2.91), while total appraised values are similar across groups (log value 6.97 versus 6.98). By construction time, new properties are on average less than one year old at the time of sale (0.77 years since construction), while resale apartments average 4.1 years since construction, reflecting our sample restriction to resales of properties built after 2005. All differences in means are statistically significant at conventional levels, as shown by the p-values from two-sample t-tests reported in the last column. Our empirical strategy addresses these baseline differences by exploiting within-census-tract.

3 Tax Incidence Model and Measurement

We use the canonical partial equilibrium incidence model of Kotlikoff and Summers (1987) for an ad-valorem tax as a framework to analyze the effect of the VAT on housing prices. Let p_s be the pre-tax price of property h and p_c the tax-inclusive price of the same property, so that $p_c = p_s(1+t)$. In the absence of taxation, $p_c = p_s = p$, which is the price that clears the market for good h; the pre-tax equilibrium therefore satisfies:

$$D_h(p) = S_h(p) \tag{1}$$

Following the introduction of an ad-valorem tax, the tax creates a wedge between the price paid by consumers and the price received by producers. The new equilibrium must satisfy:

$$D_h(p_c) = S_h(p_s) \tag{2}$$

By analyzing the change in equilibrium induced by the introduction of the tax, we can determine which side of the market bears the tax burden. Implicit differentiation of equation 2 yields:

$$\frac{dp_s}{dt} = \frac{p_s D'(p_c)}{S'(p_s) - D'(p_c)(1+t)} = -\frac{p_s \varepsilon_{d,p}}{\varepsilon_{s,p} + \varepsilon_{d,p}(1+t)} \tag{3}$$

$$\frac{dp_c}{dt} = \frac{p_s \varepsilon_{s,p}}{\varepsilon_{s,p} + \varepsilon_{d,p}(1+t)} \tag{4}$$

Equations 3 and 4 describe the incidence on producers and consumers, respectively, when the ad-valorem tax rate changes. Both equations predict that the price received by producers will fall or remain unchanged, while the price paid by consumers moves in the opposite direction. As in the case of a unit tax, the more price-elastic a side of the market, the larger the change in the price it faces. If supply is very inelastic, prices remain relatively stable and producers bear a larger share of the tax burden.

The empirical literature typically estimates the share of the tax borne by consumers by comparing the observed change in prices to the theoretical change implied by full pass-through. In our case, starting from an initial situation with no VAT, the consumer share of the tax burden (ρ) can be expressed as:

$$\rho = \frac{\Delta p_c / p_c}{\tau_i} \tag{5}$$

where $\Delta p_c / p_c$ is the average observed proportional change in consumer prices and τ_i is the effective VAT rate. Empirically, we measure this ratio as the estimated change in size-adjusted prices divided by the effective VAT rate. Full pass-through,

a situation in which the entire tax burden falls on consumers, corresponds to a price increase equal in magnitude to the tax, so that the share equals one. Zero pass-through, a situation in which the burden falls entirely on producers corresponds to no change in consumer prices, so that the share equals zero.

4 Empirical Analysis and Results

This section presents our main empirical findings. We proceed in three steps. We first examine whether the reform altered the timing and volume of housing transactions (Section 4.1). We then estimate the pass-through of the VAT into transaction prices per square meter, unit prices, and property size (Sections 4.2.2–4.2.3). Throughout, we focus on apartment transactions, where the effect is likely to be larger as their land value share is smaller compared to houses.

4.1 Transaction Volumes and Behavioral Re-Timing

The reform could have affected transaction volumes through two distinct channels. First, if the VAT is passed through to buyers in the form of higher prices, demand for new housing may decline permanently. Second, the January 2016 implementation date creates a time notch: buyers who signed a purchase contract (*promesa de compraventa*) before that date were fully exempt from the tax, regardless of when title transfer subsequently occurred. This notch creates a strong incentive for buyers to accelerate their purchase decision in order to lock in VAT-free status. We examine each channel in turn.

4.1.1 Descriptive Evidence on Transaction Volumes

Figure 3 plots the annual number of apartment transactions for three series: new-unit title transfers, new-unit purchase contracts, and resale title transfers. Two patterns emerge.

First, new-unit title transfers followed a sustained upward trend throughout the sample period and continued to grow after the VAT took effect: the average annual volume rose from 37,463 transactions in the pre-reform years to 51,117 in 2016–2019. There was no market collapse. Second, purchase contracts tell a different story. Contracts peaked at 62,267 in 2015, the year immediately preceding implementation, before falling sharply to 39,339 in 2016, a decline of 37%. Because the lag between contract signing and title transfer ranges from 12 to 36 months in the new-construction market, the surge in 2015 contracts and the subsequent collapse in 2016 contracts translated into a smooth increase in title transfers with no visible discontinuity. This pattern is consistent with buyers pulling forward their signings to avoid the January 2016 VAT, rather than a permanent reduction in demand.

On the supply side, developers appear to have responded to the reform as well. Panel (a) of 5 plots the annual number of new residential units approved through construction permits. Approved apartment units nearly tripled in the two years surrounding the announcement: from 25,775 in 2013 to 60,989 in 2014 and 72,907 in 2015. Panel (b) of figure 5 shows that median approved unit sizes declined over the same period, from 84 to 79 square meters between 2013 to 2019. We interpret this as developers bringing forward construction approvals and beginning to adjust unit characteristics in anticipation of the reform, a pattern we examine more formally in Section 4.2.3.

4.1.2 Bunching Estimation

The descriptive evidence is consistent with buyers accelerating contract signings before the notch, but it cannot rule out that the 2015 peak partly reflects an underlying upward trend in demand that would have occurred absent the reform. To quantify the timing response and separate the notch-induced spike from the trend, we estimate a counterfactual distribution of monthly contracts in the absence of the time notch, following Chetty et al. (2011) and Kleven and Waseem (2013).

We use monthly purchase contract counts from the Chilean Construction Chamber (*Cámara Chilena de la Construcción*, CChC). Because VAT liability is determined by the date of the *promesa de compraventa* rather than the deed, this series captures the relevant margin of adjustment. We group contracts into monthly bins and estimate the following regression:

$$c_m = \sum_{j=0}^{p} \beta_j z_m^j + \sum_{k=\text{lb}}^{\text{ub}} \gamma_k \mathbf{1}[m = k] + \varepsilon_m \tag{6}$$

where c_m is the number of purchase contracts in month m, z_m is the distance in months from January 2016, p is the polynomial degree, $[\text{lb}, \text{ub}]$ is the excluded window around the notch, and the indicators $\mathbf{1}[m = k]$ are set to one for months within the excluded range. The regression is estimated on observations outside the window and the fitted values of the polynomial create counterfactual $\hat{c}_m$: the number of contracts that would have been signed absent the time notch.

The excess bunching mass is:

$$\hat{B} = \sum_{m=\text{lb}}^{\text{Dec15}} (c_m - \hat{c}_m)$$

and measures the number of purchase contracts accelerated in anticipation of the tax. The missing mass is:

$$\hat{M} = \sum_{m=\text{Jan16}}^{\text{ub}} (\hat{c}_m - c_m)$$

and measures the amount contracts signed in anticipation to the tax that would have been signed after January 2016 in the absence of a time notch. Following Kleven and Waseem (2013), we set the lower bound lb visually and determine the upper bound ub recursively by requiring $\hat{B} = \hat{M}$. This condition imposes that every contract pulled forward from after the notch appears as excess mass before it, leaving total transaction volume unchanged consistent with pure re-timing rather than destruction of demand. Standard errors are estimated using the residual bootstrap of Chetty et al. (2011): we draw from the estimated residuals of equation (6) with replacement, construct a synthetic contract series, re-estimate the model, and repeat 500 times; the standard error of $\hat{B}$ is the standard deviation of the resulting distribution.

4.1.3 Bunching Results

Our preferred specification uses a third-degree polynomial and sets lb $= 9$ months, yielding an endogenous upper bound of 21 months. Figure 4 presents the observed and counterfactual monthly contract series. There is a pronounced excess mass in the months preceding January 2016 and a corresponding shortfall immediately after, providing visual confirmation of bunching. We normalize the excess and missing mass by the average monthly counterfactual volume:

$$\hat{b} = \frac{\hat{B}}{\sum_{m=\text{lb}}^{\text{Dec15}} \hat{c}_m \;/\; |\text{lb}|}$$

$$\hat{m} = \frac{\hat{M}}{\sum_{m=\text{lb}}^{\text{Dec15}} \hat{c}_m \;/\; |\text{lb}|}$$

In our preferred specification, $\hat{b} = 3.03$ (SE $= 0.34$), implying that the total excess of contracts signed in anticipation of the tax was equivalent to approximately three months of normal counterfactual transaction volume. The missing mass estimate $\hat{m} = 3.07$ (SE $= 1.01$) is statistically indistinguishable from $\hat{b}$, consistent with the convergence condition and with the interpretation that observed bunching reflects pure re-timing. This contrasts with settings where the tax causes transactions to be lost altogether: Kopczuk and Munroe (2015), studying the price notch created by the Mansion Tax in New York and New Jersey, find that missing mass exceeds bunching mass, leading them to conclude that the tax caused market unraveling rather than pure re-timing. One caveat to the interpretation of our result is that we argue that sales were not lower with respect to the transaction levels pre-reform, it could be the case that in the absence of the tax, sales could have been higher, however, having a national reform prevent us to form a control group that takes the advantage of geographical differences in the implementation of the tax allowing us to test formally if the average number of transactions with and without the the tax evolved differently.

To express the magnitude of the timing response in terms comparable to the prior literature, we compute two elasticity measures following Kancherla and Tazhitdinova (2023). The first elasticity (ε_1) measures the proportional excess of contracts relative to the counterfactual within the bunching window per percentage point of the effective VAT rate:

$$\varepsilon_1 = \frac{\hat{B}}{\sum_{m=\text{lb}}^{\text{Dec15}} \hat{c}_m} \cdot \frac{1}{\tau_{\text{eff}}} \tag{7}$$

where $\tau_{\text{eff}} = 15.7\%$ is the effective VAT rate for apartments, net of the land value share. ε_1 measures the proportional excess of contracts per proportional change in the net-of-tax rate. In our preferred specification, $\varepsilon_1 = 2.14$ (SE $= 0.24$). This is directly comparable to Kancherla and Tazhitdinova (2023), who report values of 0.44–0.55 for capital gains realizations around the one-year short-term/long-term threshold in the United States,a setting where the tax differential is permanent and fixed rather than arising from a one-time announced reform. This difference may point out that announcements of tax increases in advance of the implementation of the policy can generate larger behavioral effects than thresholds that are fixed over time. Our estimate is also comparable in total magnitude to Buettner and Madzharova (2021), who find that a one percentage point higher VAT rate triggered a 2.5% increase in consumer durable sales in the single month preceding implementation, however, our 2.14% effect accumulates over nine months.

The second elasticity measures average re-timing in months of counterfactual volume per percentage point of VAT:

$$\varepsilon_t = \frac{\hat{b}}{\tau_{\text{eff}}} \tag{8}$$

This is a semi-elasticity in months per percentage point, can be compared to the estimated changes in the UK housing market in anticipation to the increase of the transfer tax in Best and Kleven (2018b). In our preferred specification, $\varepsilon_t = 0.193$ months per percentage point, equivalent to 0.84 weeks per percentage point. Best

and Kleven (2018b) find that the expected increase of the UK stamp duty induced an excess mass equivalent to approximately three weeks of sales per percentage point of the tax before its implementation. Our estimate of 0.84 weeks per percentage point is roughly four times smaller. The limited re-timing we estimate likely also reflects frictions specific to the new-housing market that would attenuate the behavioral responses. First, search and matching frictions limit how quickly buyers can locate, evaluate, and commit to a specific unit that may not yet be built (Genesove and Han, 2012; Best and Kleven, 2018b). Second, average down-payments of 20% in the Chilean market may constrain buyers' ability to free up the liquidity required to accelerate a purchase. Finally, tax inattention may have prevented full behavioral adjustment: Bradley (2017) shows that homebuyers systematically underreact to the tax implications of housing purchases in the context of higher future property taxes.

4.1.4 Robustness

The excess bunching, missing mass, and elasticity estimates depend on three specification choices: the polynomial degree p, the lower bound lb of the bunching window, and the convergence tolerance. Table 3 reports estimates obtained by varying one parameter at a time while holding all others at their preferred values.

Three conclusions emerge. First, the normalized excess bunching $\hat{b}$ and missing mass $\hat{m}$ are stable across all specifications, ranging from 2.93 to 3.29 and 2.82 to 3.20 respectively, and are statistically indistinguishable from each other within every column, consistent with pure re-timing in all cases. Second, the timing semi-elasticity ε_2 is similarly stable, ranging narrowly from 0.187 to 0.210, with no pairwise difference statistically significant at conventional levels. Third, ε_1 shows more variation, ranging from 1.56 to 2.33. This is mechanical: when the bunching window is extended from 9 to 12 months, additional counterfactual volume enters the de-

nominator of ε_1 without a proportional increase in the numerator, since the months furthest from the notch contribute relatively little excess mass.

4.2 VAT Pass-Through Estimation

4.2.1 Empirical Strategy

We estimate the effects of the VAT on housing outcomes using an event-study difference-in-differences design that compares new-unit transactions (treated) to resale transactions (control) before and after the reform. Our estimating equation is:

$$\log(y_{ict}) = \alpha\, T_i + \sum_{j \neq 2014} \beta_j \left(T_i \times \mathbf{1}[t = j]\right) + \delta_t + \phi_c + \gamma X_i + \varepsilon_{ict} \tag{9}$$

where $\log(y_{ict})$ is the logarithm of the outcome for transaction i, in census tract[13] c in year t. Our main outcomes are the transaction price (in inflation-adjusted UF), the property size in square meters, and the price per square meter. T_i is an indicator equal to one for new-unit (treated) transactions and zero for resale (control) transactions. Year fixed effects δ_t absorb common shocks such as demand and credit shocks. Census tract fixed effects ϕ_c control for time-invariant differences in location-specific amenities, regulatory constraints, and income levels (Saiz, 2010). X_i is a vector of time-invariant property-level controls: indicators for units smaller than 50 square meters, whether the property exceeds the 75th percentile of the census-tract ap-

[13]Census tracts (*distritos censales*) are the territorial subdivisions defined by the Chilean National Statistics Institute (INE) below the municipality level. Each tract constitutes the basic unit for statistical operations, with boundaries established by the INE that carry no legal status. Nationally, there are 2,771 census tracts, classified as urban, rural, or mixed; urban tracts are delimited primarily by dwelling count (Instituto Nacional de Estadísticas (INE), 2017). We restrict the sample to urban census tracts; the average tract in our estimation sample contains approximately 6,400 dwellings and records 108 apartment transactions per year. We exclude rural census tracts.

praisal distribution, whether it is located in a multi-unit complex, and whether it includes a parking space, storage unit, or commercial area. Standard errors are clustered at the census tract level to account for both serial correlation (some units transact multiple times) and spatial correlation within tracts.

The coefficients of interest are β_j in equation 9. We use 2014 as the reference year because the reform was announced late during the year; the pre-2014 coefficients β_{2010} through β_{2013} serve as a placebo test for differential pre-reform trends (Steigerwald et al., 2021). The coefficient β_{2015} captures anticipatory price effects. Coefficients β_{2016} through β_{2019} capture the short- and medium-run effects of the tax during the period of full implementation. We include post-2019 coefficients for completeness but do not attribute them to the VAT, as mortgage rates rose sharply during the COVID-19 period and may have differentially affected new and resale transactions.[14]

To benchmark our estimates against the theoretical incidence, we compare the estimated price change to the full pass-through prediction. A key distinction between the Chilean VAT and the transfer taxes studied in most prior work is the tax base: whereas transfer taxes typically apply to the full sale price (Besley et al., 2014a; Best and Kleven, 2018a; Kopczuk and Munroe, 2015), the Chilean VAT applies only to the construction component of the property value, excluding land. The theoretical full pass-through for apartments is therefore:

$$\Delta^{\text{FP}} = 0.19 \times (1 - \bar{l}) = 15.7\% \tag{10}$$

[14]The literature documents several COVID-19 shocks to housing markets that could confound our estimates in the post-2019 period, including the price effects of pandemic-related lockdowns (Anundsen et al., 2023), the relative appreciation of suburban housing (Gupta et al., 2022), and the erosion of the urban job-access premium following work-from-home adoption (Brueckner et al., 2023).

where $\bar{l} = 17.5\%$ is the estimated average land share for apartments from Balsa and Vásquez (2023).[15]

Our estimates are likely conservative for two reasons. First, we cannot directly observe whether any individual seller is a habitual developer subject to VAT or a private seller who is not; any misclassification introduces attenuation bias under classical measurement error. Second, if new and resale properties are close substitutes, full pass-through in the new-unit segment would make resale properties relatively cheaper, potentially raising resale prices and attenuating the estimated differential. We test for within-tract spillovers in Section 4.2.4 and find no evidence of them, suggesting that attenuation from the substitution channel is limited.

4.2.2 Pass-Through to Prices per Square Meter

We begin by documenting the time pattern of price adjustment. Figure 7 plots the estimated $\hat{\beta}_j$ coefficients from equation (9) with price per square meter as the outcome, separately for apartments and houses. The figure includes the full-pass-through benchmark of 15.7% as a horizontal reference line.

Four findings stand out. First, the pre-reform coefficients from 2010 to 2013 are small in magnitude and statistically indistinguishable from zero, supporting the parallel-trends assumption underlying our identification strategy. Second, the 2015 coefficient, capturing anticipatory effects following the September 2014 announcement, is small and insignificant, suggesting that the surge in purchase contracts observed in that year was not accompanied by a differential price increase. This is consistent with supply responding to the announcement: as documented in Section 4.1, construction approvals nearly tripled between 2013 and 2015, plausibly offsetting

[15] Specifically, $\Delta^{\text{FP}} = 0.19 \times (1 - 0.175) = 15.7\%$. For houses, the average land share is 50.6%, yielding a full pass-through benchmark of 9.5%.

upward price pressure in the anticipation period.

Third, in 2016, the year of partial implementation, during which many projects approved before the reform retained their exemptions, the estimated effect is 3.8%, well below the full pass-through benchmark. Fourth, and most importantly, from 2017 onward we observe a sustained differential increase in apartment prices: the point estimates are 11.8% in 2017, 9.3% in 2018, and 12.3% in 2019. The pooled 2017–2019 estimate[16] of 11.8% is statistically different from zero at conventional levels but also statistically below the full pass-through benchmark of 15.7%.[17]. Only the coefficient for 2019 (12.6%) is not statistically different from 15.7% at the 5% level. The post-COVID estimates of 15.9% (2020) and 14.7% (2021) while in line with the full pass-through prediction, they cannot be cleanly attributed to the VAT alone.

Our pooled 2017–2019 estimate of approximately 11.8% for price per square meter implies a pass-through rate of roughly 75% relative to the full pass-through benchmark of 15.7%, rising to approximately 78% in 2019 when temporary exemptions had largely expired. This estimate is within the range documented in the broader VAT pass-through literature. Most of the evidence on VAT pass-through comes from studies of VAT reductions, with estimated pass-through to consumers ranging from 25% for French restaurants (Benzarti and Carloni, 2019), roughly 50% for Finnish hairdressers (Kosonen, 2015) and car sales in France (Carbonnier, 2007), 70% for German supermarkets (Fuest et al., 2025) and housing repair services in France (Carbonnier, 2007), up to near-complete pass-through for food products in Norway (Gaarder, 2019). Evidence on VAT increases is more limited but consistently points to higher pass-through than for decreases (Benzarti et al., 2020, 2024). Ben-

[16]This estimate comes from a pooled regression aggregating the entire pre-reform period as baseline and separate dummies for the periods 2015, 2016, 2017–2019 and 2020–2022

[17]The p-value of the two-sided t-test of has a p-value of 0.006

zarti et al. (2020) document that prices respond more strongly to VAT increases than to equivalent decreases across European countries, and Benzarti et al. (2024) find pass-through rates of 50% for a VAT cut but 90% for its subsequent repeal in the same setting. Our estimate of 75% for a VAT increase is consistent with this upper range and with the asymmetry result of Benzarti et al. (2020).

While no other paper specifically estimates the incidence of VAT on housing, transfer or stamp duty taxes are a common form of transaction tax in the housing market. The transfer tax literature offers a direct benchmark for our incidence results. In contrast to our result, Dachis et al. (2011) find that a transfer tax increase in Toronto was almost entirely capitalized into lower seller prices, with incidence falling on sellers rather than buyers. Besley et al. (2014b) find that only 40% of the stamp duty burden fell on buyers, with sellers absorbing the remaining 60% through lower pre-tax prices. In contrast, we find that buyers bear more than 70% of the VAT burden. Finally, Kopczuk and Munroe (2015) documents that the incidence of transaction taxes falls fully on sellers. Two features of those settings may explain the difference. First, transfer taxes apply also to resale transactions involving individual household sellers negotiating bilaterally with buyers, whereas our setting involves competitive developers setting prices in a new-construction market. Second, the stamp duty schedules in Besley et al. (2014b) and Kopczuk and Munroe (2015) create price notches that give sellers a direct incentive to reduce transaction prices below the threshold, and in Dachis et al. (2011) buyers had the option to relocate outside the taxed Toronto area. In both, there were lower incentives to pass the tax to buyers. Notably, Kopczuk and Munroe (2015) find that the distortion is weaker for newly built properties than for resales, and attribute this to possible real adjustments to property characteristics, a margin we document directly below.

4.2.3 Do Prices or Unit Sizes Adjust?

The estimated pass-through to price per square meter combines two distinct adjustment margins: higher unit prices and smaller unit sizes at a given price. To decompose these effects, we re-estimate equation (9) separately using the log unit price and the log property size (in square meters) as outcomes.

Figure 11 shows that nominal unit prices for new apartments rose relative to resales after the reform. The pooled 2017–2019 estimate is 8.5%, accounting for approximately three-quarters of the observed price-per-square-meter effect. The remainder is explained by a differential reduction in unit size: Figure **??** shows that new apartments transacted after the reform were smaller than comparable resale units relative to the pre-reform baseline, with a pooled 2017–2019 effect of -3.2%.[18] This mechanism was theorized in previous literature such as Kopczuk and Munroe (2015) but we can prove it empirically and measure its magnitude. Together, the two margins account for the full price-per-square-meter response.

The size reduction is consistent with two mechanisms. On the demand side, buyers facing higher effective prices may substitute toward smaller units to preserve homeownership status. On the supply side, developers may respond strategically by reducing unit size, a less salient attribute than the nominal price, if buyers are less sensitive to changes to the size than along the price margin (Chetty et al., 2009). We interpret the supply-side channel as primary. Construction permit data provide corroborating evidence: the median approved unit size declined from 84 square meters in 2013 to 79 square meters by 2019 (Figure 5), a trend that began at the time of the reform announcement and pre-dates any price response in transaction

[18] To put this in perspective, the average new apartment size in the pre-treatment period was 57.6 square meters (approximately 620 square feet). A 3.2% reduction corresponds to roughly 1.8 square meters, approximately the area of a small bathroom.

data. This pattern is consistent with developers proactively reducing approved unit sizes in anticipation of a tax-induced cost increase (Chalioti and Serfes, 2024). The broader phenomenon, firms absorbing cost shocks by shrinking product quantity rather than raising list prices, has been documented in retail markets (Janssen and Kasinger, 2026; Kim, 2024), and our findings extend this evidence to the housing sector.

4.2.4 Robustness Checks

Spillover Effects: Our identification strategy assumes the absence of spillovers to control units (resales) within census tracts. This assumption may be violated if existing properties are close substitutes for new units. In that case, the VAT-induced increase in new housing prices could shift demand toward resale properties, thereby increasing resale prices. If such spillovers are present, our baseline estimates would represent a lower bound of the true treatment effect. However, we do not find evidence consistent with spillovers, namely, differential price increases in areas with greater exposure to new property sales.

In this section, we formally assess the extent of spillovers on resale prices. Specifically, we test whether resale prices exhibit differential post-treatment dynamics depending on their proximity to new property sales. To do so, we restrict the sample to control transactions and partition them into two groups based on the local intensity of new property sales. We define high-spillover census tracts as those in which the share of new property sales during the pre-treatment period exceeds 75%, and low-dose census tracts as those in which this share is below 75%. We estimate spillover effects on resale transactions using the following event-study specification:

$$\log(y_{icmt}) = \alpha H_i + \sum_{j \neq 2013} \beta_j (H_i \times \mathbf{1}[t = j]) + \delta_t + \phi_m + \gamma X_i + \varepsilon_{ibmt} \tag{11}$$

Where y_{icmt} denotes the price per square meter of transaction i in census tract c, municipality m, and year t. The indicator H_i equals one for resale transactions located in high-spillover census tracts (as defined using pre-treatment data from 2010–2013). The specification includes year fixed effects (δ_t), municipality fixed effects (ϕ_m), and a vector of property-level controls (X_i), including indicators for units smaller than 50 square meters, properties above the 75th percentile of the census-tract appraisal distribution, location in a multi-unit complex, and the presence of a parking space, storage unit, or commercial area. Standard errors are clustered at the municipality level. We consider just control transactions i.e. resales of apartments built after 2005. The identification comes from differential exposure to new property sales across census tracts within the same municipality, holding constant common time shocks and municipality-level factors. Census tracts are classified using only pre-treatment information, ensuring that treatment intensity is not endogenous to post-reform price dynamics.

Figure 12 illustrates the empirical strategy. The map displays high-spillover (blue) and low-spillover (yellow) census tracts in the Metropolitan Region of Santiago for the period 2010–2013, with thicker lines denoting municipality boundaries. The identification relies on comparing the evolution of resale prices across high- and low-spillover areas within the same municipality over time. Implicitly, this approach assumes that spillovers are spatially localized and increase with exposure to nearby new construction activity, as in González-Pampillón (2022). Figure 13 presents the estimated coefficients β_j from equation 11. We find no evidence of differential price dynamics between high- and low-spillover areas over the sample

period. The estimated coefficients are negative throughout the sample period and statistically indistinguishable from zero. We interpret these results as evidence consistent with the absence of spillover effects from treated (new) to control (resale) transactions.

Specification: Tables B.1 , B.2 and B.3 report the event-study coefficients for our main outcomes across four variants of the main specification. First, we remove the controls to understand the sensitivity of the results to their introduction. Second, we report the results with broader municipality cluster and fixed effects to take into account larger housing markets. Finally, we include time by municipality fixed effects to allow for differential municipality trends in the outcomes. The results are broadly stable across specifications, supporting the validity of the baseline estimates.

The placebo coefficients β_{2010}–β_{2013} are small and statistically indistinguishable from zero in the baseline across all three outcomes, consistent with parallel pre-trends between new and resale properties prior to the reform. Notable exceptions include our main specification where is the size outcome is positive and significant. This pattern does not persist once controls are removed or using broader fixed effects. Anticipation effects are small and insignificant across all specifications and outcomes, ranging from 0.004 to 0.017 log points. This is consistent with the response both from demand and supply prior to VAT implementation. The post-reform coefficients are stable and precisely estimated across specifications, except controlling for broader municipality fixed effect which can result in less precise comparisons. For prices per square meter, β_{2017}–β_{2019} range between 0.095 and 0.126 log points in the baseline using narrow fixed effects. For unit prices, the pattern is similar, with estimates between 0.07 and 0.125 log points over the same period using narrow fixed effects. The size outcome displays a contrasting pattern, with β_{2017}–β_{2019} negative and signifi-

cant in three of four specifications, consistent with housing shrinkflation, estimates from the municipality trend specification are smaller but directionally consistent.

Sample: Tables B.4, B.5, and B.6 report the event-study coefficients for our main outcomes across four variants of the sample construction. All columns use the baseline specification of equation 9 throughout. First, we include observations in the top and bottom 1% tails of the price, price per square meter, and size distributions, which are excluded from the baseline to limit the influence of outliers. Second, we retain resales of properties whose first sale occurred under VAT after implementation, which the baseline excludes to avoid contaminating the control group with units that may have partially capitalized the tax at first sale. Third, we extend the sample to properties built after 1999, relaxing the baseline restriction to post-2005 construction and allowing a comparison with older control units.

The results are broadly stable across all sample variants, supporting the validity of the baseline sample restrictions. Most of the placebo coefficients β_{2010}–β_{2013} are small and statistically indistinguishable from zero across all three outcomes and all columns, consistent with parallel pre-trends between new and resale properties prior to the reform, the exception being a positive and significant β_{2011} for the size outcome across all sample variants. Anticipation effects remain small and insignificant across all variants and outcomes. The post-reform price effects are stable. For prices per square meter, β_{2017}–β_{2019} range between 0.090 and 0.123 log points across the four sample variants, with the attenuations when post-VAT resales or older properties are included. This attenuation is expected: including resales of treated units in the control group biases the differential downward, so the smaller coefficients should be interpreted as a lower bound on the true pass-through. Same with including older properties which can be less comparable to new properties. For unit

prices, β_{2017}–β_{2019} range between 0.072 and 0.098 log points, displaying a similar pattern of attenuation when post-VAT resales or older units are retained. The size outcome is negative and significant in β_{2017}–β_{2019} across all four sample variants, ranging from -0.018 to -0.027 log points, supporting the real effect results.

5 Heterogeneous Pass-Through Across Local Market Conditions

The canonical incidence model predicts that pass-through to consumers is higher in markets with relatively inelastic supply: when developers cannot easily expand the housing stock in response to a cost increase, a larger share of the tax burden is transmitted forward to buyers through higher prices. To test this prediction, we examine whether VAT pass-through varies across municipalities with different local market conditions. We classify municipalities along three dimensions, all measured in the pre-reform period to avoid endogeneity with respect to the reform. First, we classify by the median appraised value of the residential property stock, computed from the SII cadaster, which we assume is positively correlated with lower demand elasticities. Second, we classify by construction intensity, defined as the ratio of newly approved residential units to the total property stock in 2009, measured using construction permit data from 2010–2013, which proxies for the pace of new supply expansion. Third, we classify by housing density, defined as the ratio of total residential properties to municipal area in square kilometers, which proxies for land scarcity. For each dimension, we estimate equation (9) separately for municipalities above and below the 75th percentile of the relevant distribution.

Across all three dimensions of local market heterogeneity, the point estimates of

VAT pass-through range narrowly between 74% and 91% of the full pass-through benchmark, and no pairwise difference is statistically significant, which its at odds with the theoretical prediction based on the relative supply and demand elasticities.

Housing density: Figure 14 shows that high-density municipalities have a pooled 2017–2019 estimate of 12.0%, corresponding to a pass-through rate of 76%. Contrary to the theoretical prediction, low-density municipalities show a larger point estimate of 14.2%, corresponding to a pass-through rate of 91%, though the two estimates are not statistically distinguishable from each other. Notably, low-density municipalities are the only subgroup for which the 2015 anticipatory coefficient is statistically significant of around 5%, suggesting that price pressure emerged earlier in less dense markets.

Construction intensity: Figure 15 shows that low construction intensity municipalities have a pooled 2017–2019 estimate of 12.6%, corresponding to a pass-through rate of 80%. High construction intensity municipalities show a slightly larger point estimate of 13.2%, corresponding to a pass-through rate of 84%, the opposite of the theoretical prediction. As with the other dimensions, the difference between groups is not statistically significant.

Appraisal values: Figure 16 presents the estimated event-study coefficients by property appraissal or valuation. High-valuation municipalities have a pooled 2017–2019 pass-through of 12.3% , corresponding to a pass-through rate of 78% relative to the full benchmark of 15.7%. Low-valuation municipalities show a marginally smaller estimate of 11.6% , corresponding to a pass-through rate of 74%. The difference of 0.7 percentage points between the two groups is not statistically significant. Neither group shows a statistically significant anticipatory price effect in 2015.

6 Conclusion

This paper estimates the incidence of a 19% VAT on new housing in Chile, one of the largest tax changes studied in the housing literature and the first application of a VAT to the residential market for which causal evidence exists. Using administrative data covering nearly the universe of real-estate transactions, we find that apartment prices per square meter rose by approximately 11% within three years of the reform, with consumers bearing about 75% of the effective tax rate. The increase operated through two margins: higher nominal prices (8.5%) and smaller apartment sizes (3.2%). Despite the large tax, transaction volumes did not decline, though we observe pure re-timing of purchase contracts around the implementation date, around 2.4% increase per percentage point of the effective tax rate of purchase contracts in anticipation of the tax.

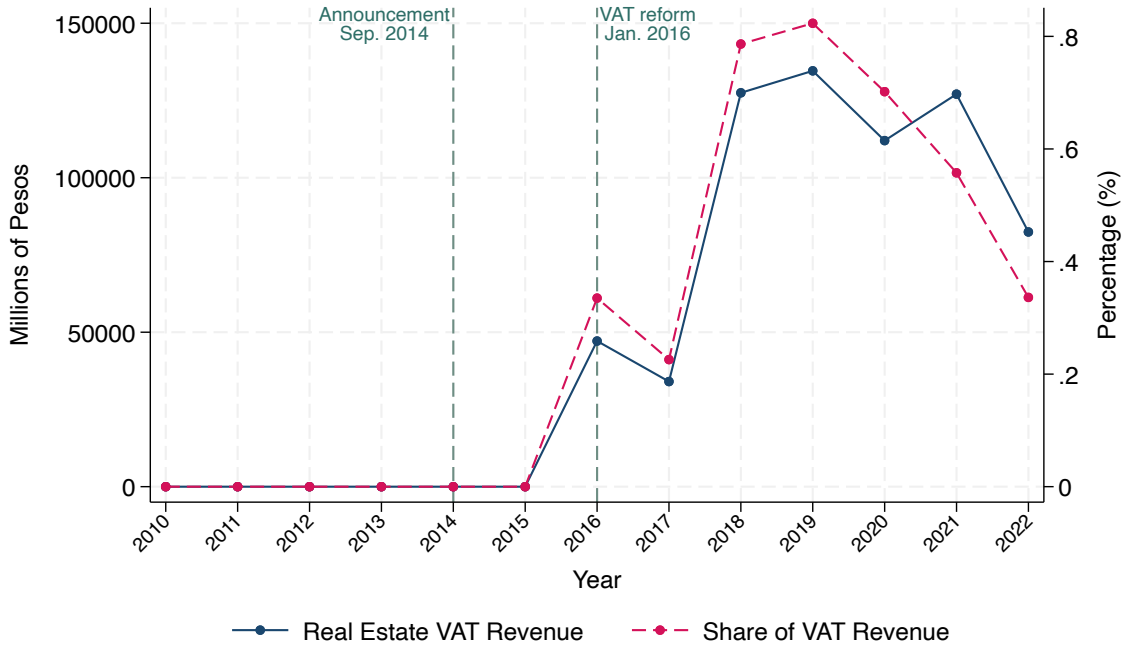
Three aspects of these results have broader implications. First, the finding that developers adjusted unit sizes in response to the tax, rather than passing through the entire cost via unit prices, suggests that incidence analysis focused exclusively on prices may understate the burden on consumers. This size adjustment margin is likely relevant in other settings where firms can modify product characteristics. Second, the stability of transaction volumes indicates that the tax raised revenue without generating volume reductions documented for transfer taxes in other countries. This distinction matters for tax design: unlike transfer taxes that discourage mobility by taxing every transaction, a VAT on new construction applies once and does not penalize subsequent resales. Third, the modest anticipatory re-timing we estimate, despite a 15-month announcement window, highlights the role of market frictions in limiting behavioral responses. In new-housing markets, where transactions involve long lead times and substantial downpayments, the scope for tax

avoidance through timing is constrained.

From a policy perspective, these findings speak to a question faced by governments worldwide: whether to include new housing in the VAT base. Our results suggest that such a tax is an effective revenue instrument, in the sense that it does not collapse the market. However, a relevant part of the burden falls in the consumers. For countries considering this policy, two implications follow. The tax is likely to exacerbate affordability pressures. The incidence may not be fully visible in unit prices, since part of the adjustment can take the form of smaller units, a margin that standard price indices do not capture.

7 Figures

Figure 1: Total VAT revenue from Real Estate Transactions



Note: The figure shows yearly aggregates of VAT revenue declared to the SII from form F29 in current millions of pesos (blue-solid) and as a share of the total VAT revenue (red-dotted) reported by the the Chilean Central Bank.

Figure 2: Tax Reform Timeline

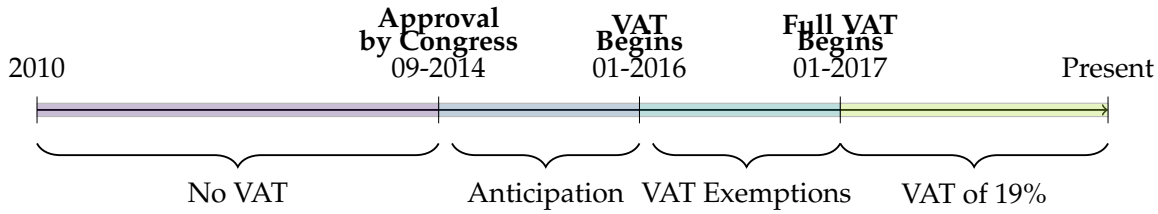
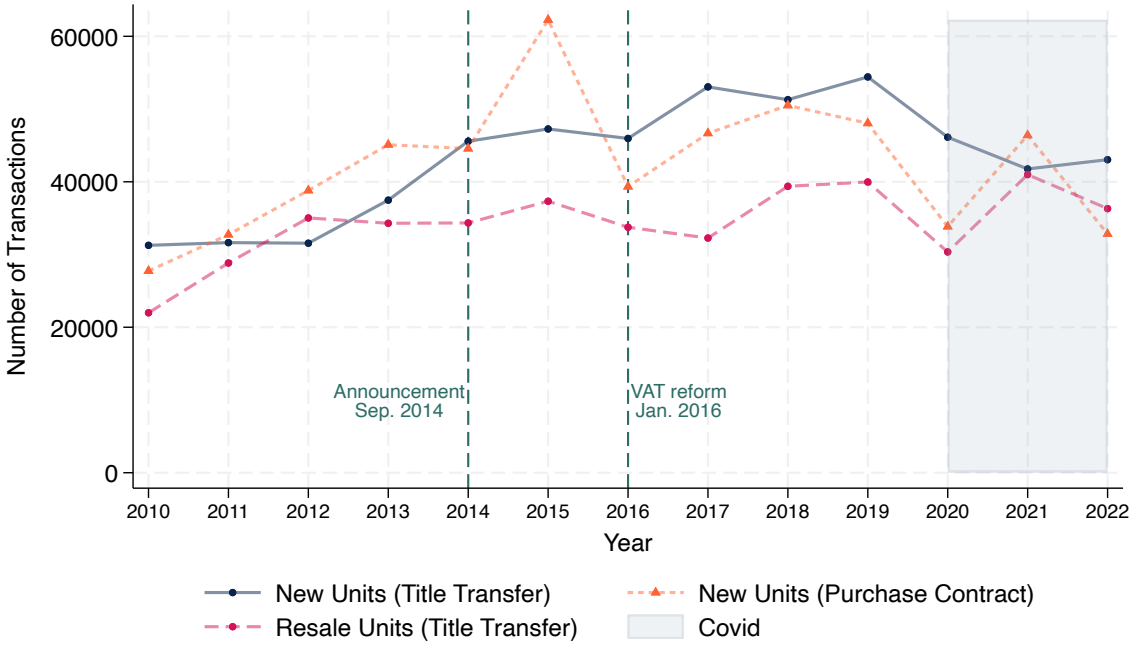
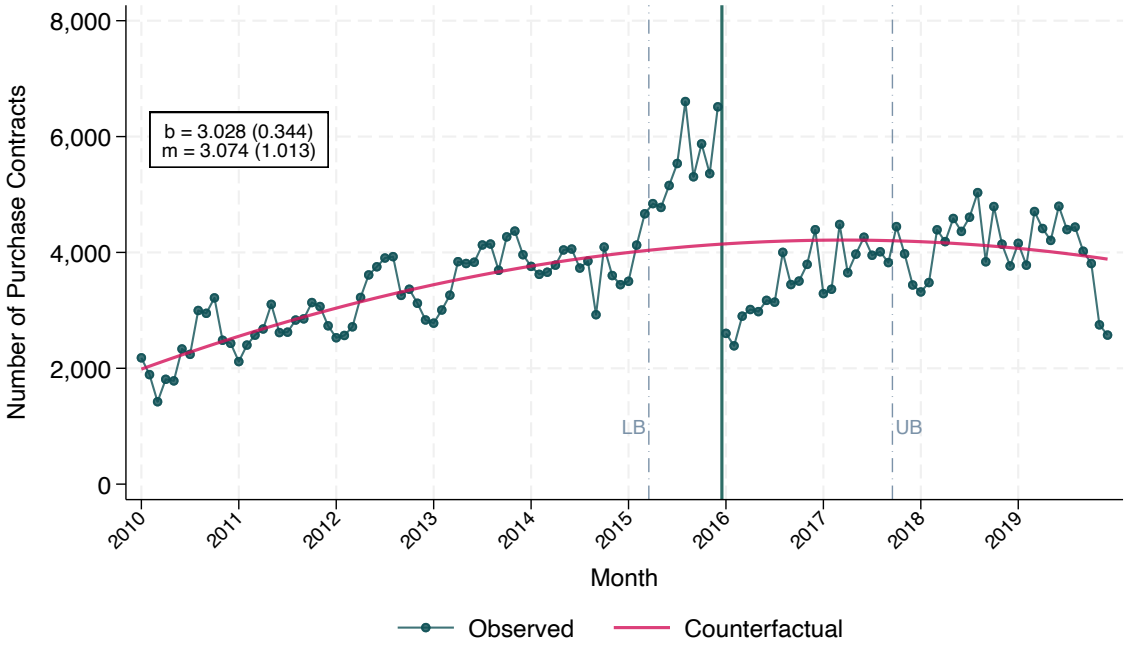


Figure 3: Number of Transactions and Sale Contracts



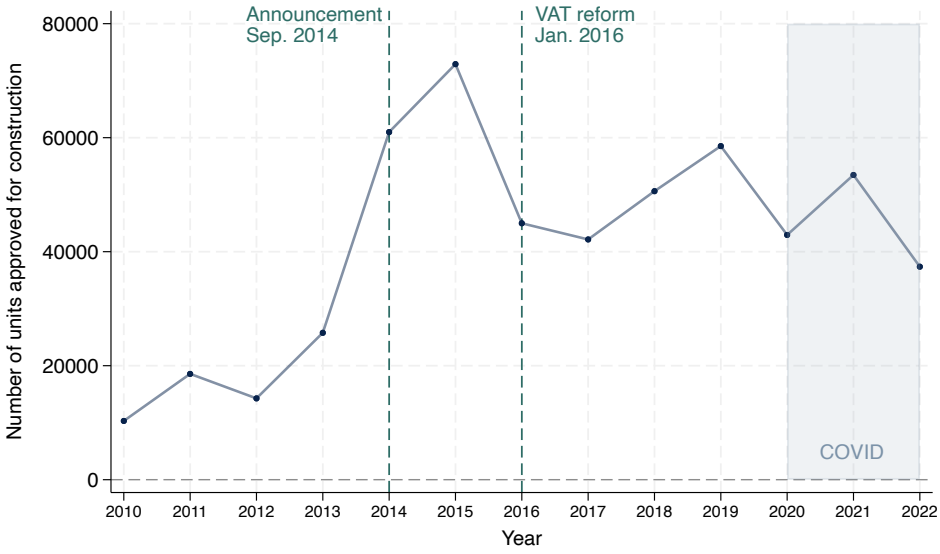
Note: The figure shows the annual number of apartment transactions by type: new units (title transfers), new units (purchase contracts), and resale units (title transfers). The title transfer series are constructed from the administrative transaction data used in this paper and include all observations regardless of missing prices or extreme values. The series for new unit purchase contracts corresponds to national-level counts reported by the Cámara Chilena de la Construcción. Vertical dashed lines indicate the September 2014 announcement of the VAT reform and its implementation in January 2016. The shaded region denotes the COVID-19 period.

Figure 4: Number of Transactions and Sale Contracts

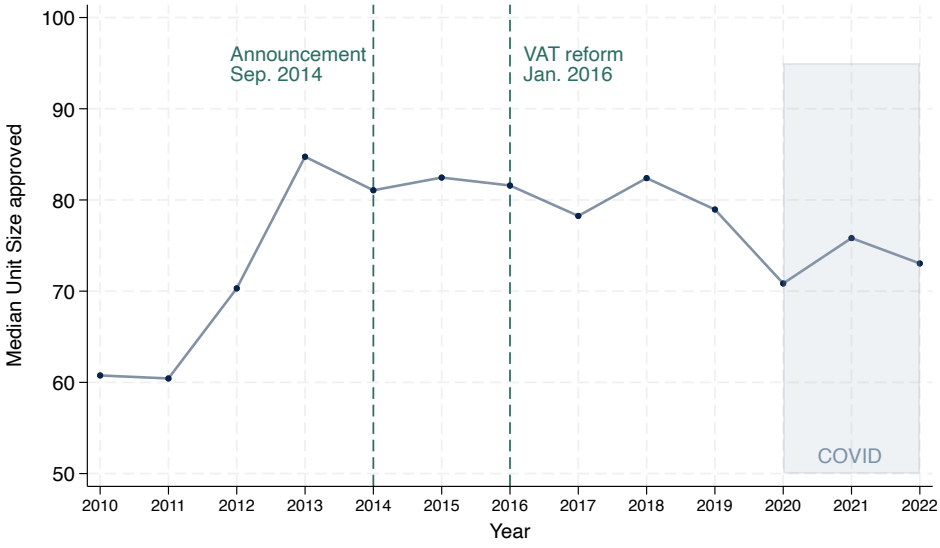


Note: The figure plots monthly new apartment purchase contracts (observed series) and the estimated counterfactual (dashed line) around the January 2016 VAT notch, constructed by fitting a third-degree polynomial to the observed series excluding the bunching window $[\text{LB}, 0] = [-9, 0]$ months and the missing mass region $[1, 21]$ months relative to the notch. The counterfactual is extrapolated into both excluded regions using the estimated polynomial coefficients. The upper bound of the missing region (UB = 21 months) is determined recursively such that $|\text{excess} - \text{missing}|/\text{excess} < 0.05$. The excess mass b measures the total number of contracts bunched before the notch expressed in units of the average monthly counterfactual volume. The missing mass m measures the corresponding shortfall of contracts in the post-notch period using the same normalization. Bootstrap standard errors (500 replications) follow the residual resampling procedure of Chetty et al. (2011), in which residuals from outside-window observations are resampled with replacement, added to the fitted counterfactual to construct a synthetic series, and the estimator is re-applied to each synthetic draw; the inside-window observations are held fixed across replications to preserve the bunching signal.

Figure 5: New Housing Permits and Average Approved Unit Size



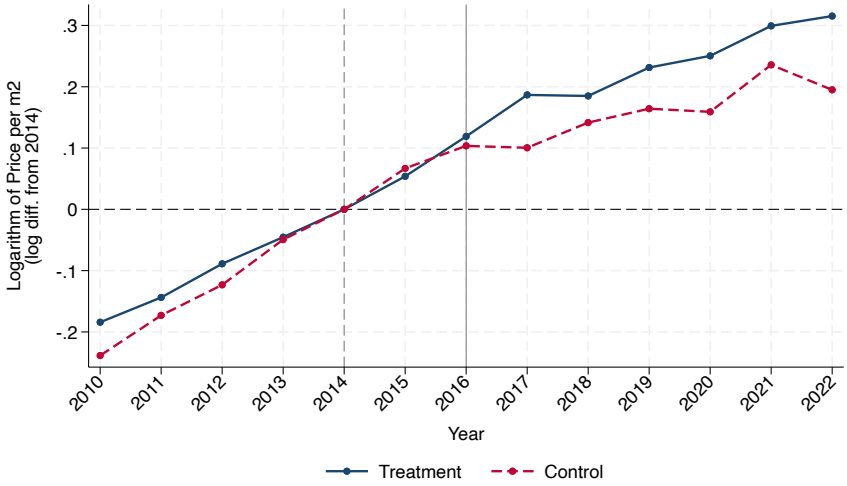
(a) Number of New Approved Apartments



(b) Median Size of Approved Housing Units

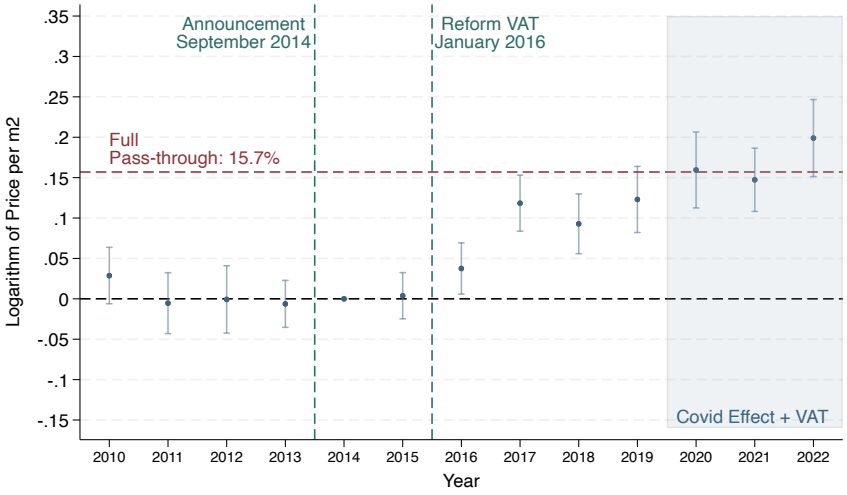
Note: The figure plots the annual number of apartment units approved through construction permits and the median approved unit size. The number of approved units is computed as the total number of housing units reported in residential construction permits corresponding to apartment buildings. Unit size is calculated as the total permitted floor area divided by the number of units within each permit, and the figure reports the median of this measure across municipalities and permits. The sample includes permits classified as residential and containing apartment buildings.

Figure 6: Descriptive Trends on Transaction Prices per Square Meter



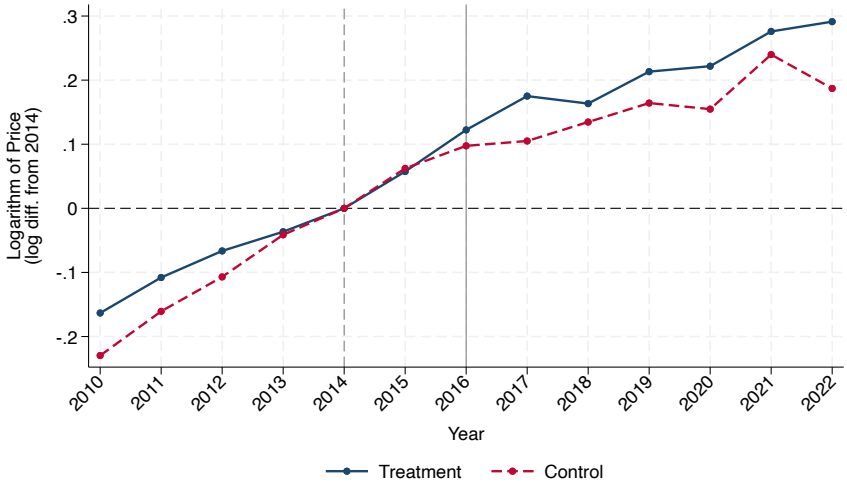
Note: The figure displays the conditional means of the logarithm of sale price per square meter by treatment status controlling for census tract and time effects. It also controls for dummy variables for properties under 50 square meters, dummy variables indicating that the property's assessment is on the top quartile of the census tract distribution, if the property is located in a multi-unit complex and whether the unit includes a parking, storage, commercial or office space. Each data-point display the difference between the conditional mean and its value during 2014. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. We exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, as well as, transactions in rural census tracts and transactions for which first sale included VAT and resold after the VAT was implemented.

Figure 7: VAT Pass-Through on Transaction Prices per square meter



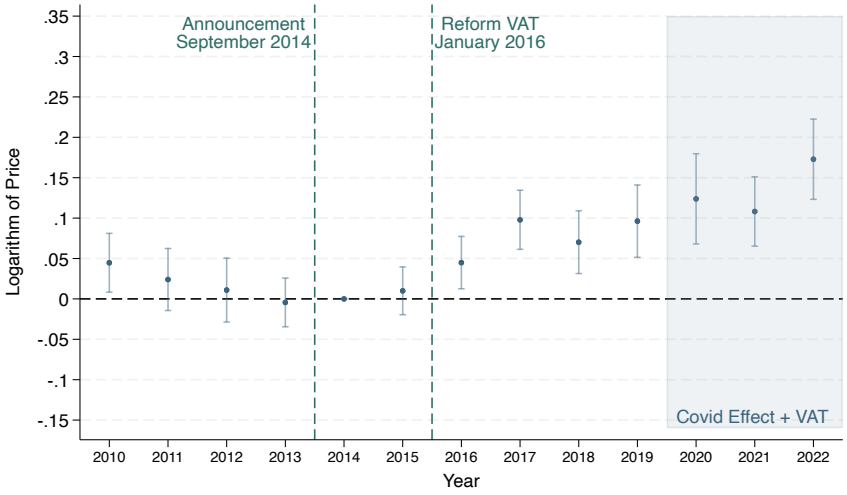
Note: The figure shows the coefficients β_j estimated from specification 9 where the outcome is the logarithm of transaction price per square meter. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. The regression includes census tract fixed effects, dummy variables for properties under 50 square meters, dummy variables indicating that the property's assessment is on the top quartile of the census tract distribution, if the property is located in a multi-unit complex and whether the unit includes a parking, storage, commercial or office space. We exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, as well as, transactions in rural census tracts and transactions for which first sale included VAT and resold after the VAT was implemented. Standard errors are clustered at the census tract level and lines represent 95% confidence intervals.

Figure 8: Descriptive Trends on Transaction Prices



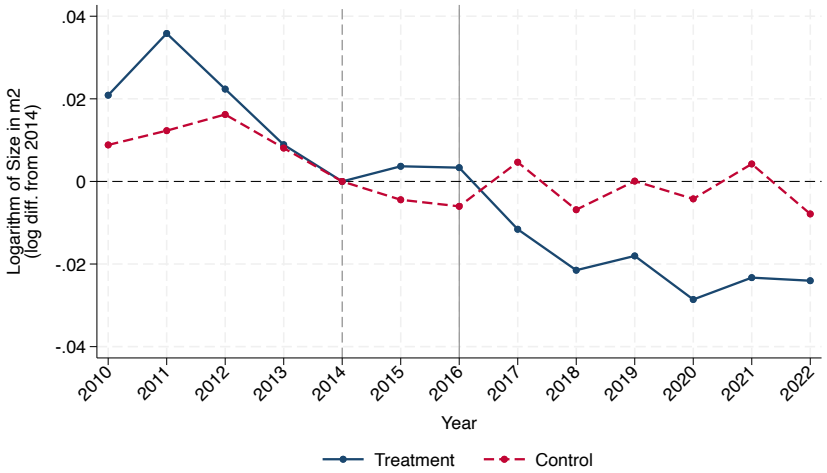
Note: The figure displays the conditional means of the logarithm of sale price by treatment status controlling for census tract and time effects. It also controls for dummy variables for properties under 50 square meters, dummy variables indicating that the property's assessment is on the top quartile of the census tract distribution, if the property is located in a multi-unit complex and whether the unit includes a parking, storage, commercial or office space. Each data-point display the difference between the conditional mean and its value during 2014. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. We exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, as well as, transactions in rural census tracts and transactions for which first sale included VAT and resold after the VAT was implemented.

Figure 9: VAT Pass-Through on Transaction Prices



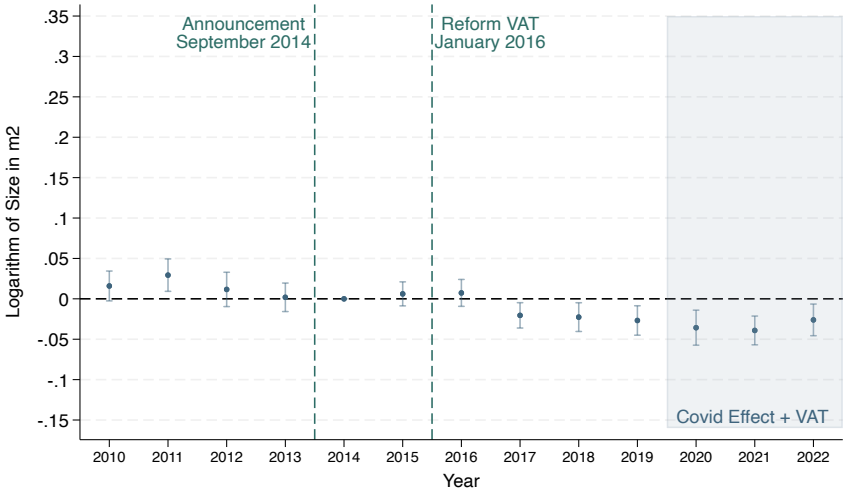
Note: The figure shows the coefficients β_j estimated from specification 9 where the outcome is the logarithm of transaction price. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. The regression includes census tract fixed effects, dummy variables for properties under 50 square meters, dummy variables indicating that the property's assessment is on the top quartile of the census tract distribution, if the property is located in a multi-unit complex and whether the unit includes a parking, storage, commercial or office space. We exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, as well as, transactions in rural census tracts and transactions for which first sale included VAT and resold after the VAT was implemented. Standard errors are clustered at the census tract level and lines represent 95% confidence intervals.

Figure 10: Descriptive Trends on Property Size



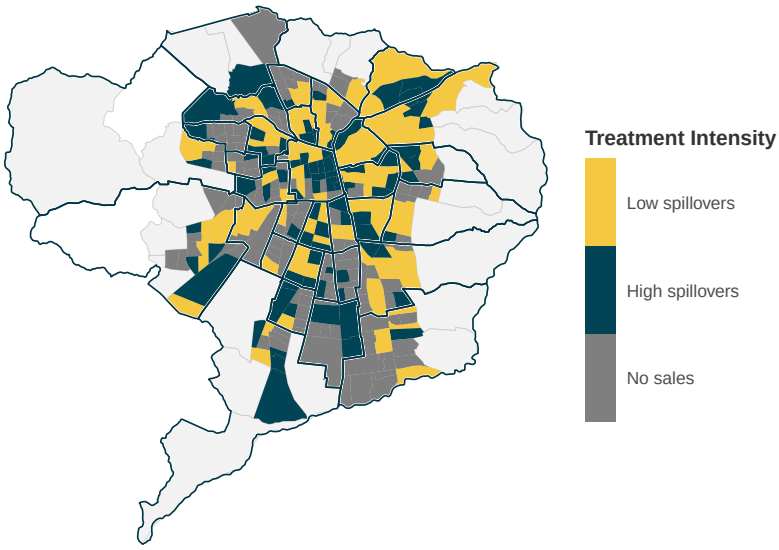
Note: The figure displays the conditional means of the size in square meters of the residential are of a property by treatment status controlling for census tract and time effects. It also controls for dummy variables for properties under 50 square meters, dummy variables indicating that the property's assessment is on the top quartile of the census tract distribution, if the property is located in a multi-unit complex and whether the unit includes a parking, storage, commercial or office space. Each data-point display the difference between the conditional mean and its value during 2014. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. We exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, as well as, transactions in rural census tracts and transactions for which first sale included VAT and resold after the VAT was implemented.

Figure 11: VAT Effect on Property Size



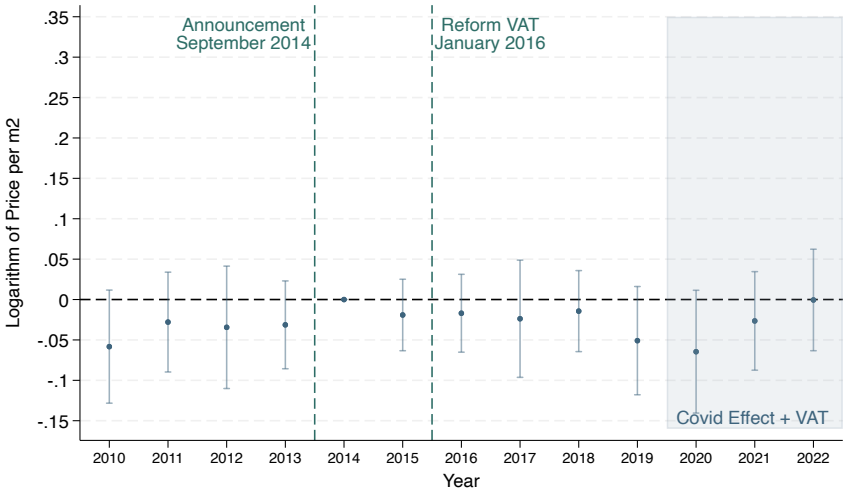
Note: The figure shows the coefficients β_j estimated from specification 9 where the outcome is the logarithm of transaction price. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. The regression includes census tract fixed effects, dummy variables for properties under 50 square meters, dummy variables indicating that the property's assessment is on the top quartile of the census tract distribution, if the property is located in a multi-unit complex and whether the unit includes a parking, storage, commercial or office space. We exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, as well as, transactions in rural census tracts and transactions for which first sale included VAT and resold after the VAT was implemented. Standard errors are clustered at the census tract level and lines represent 95% confidence intervals.

Figure 12: High and Low Spillover Census Tracts in the Metropolitan Region of Santiago de Chile



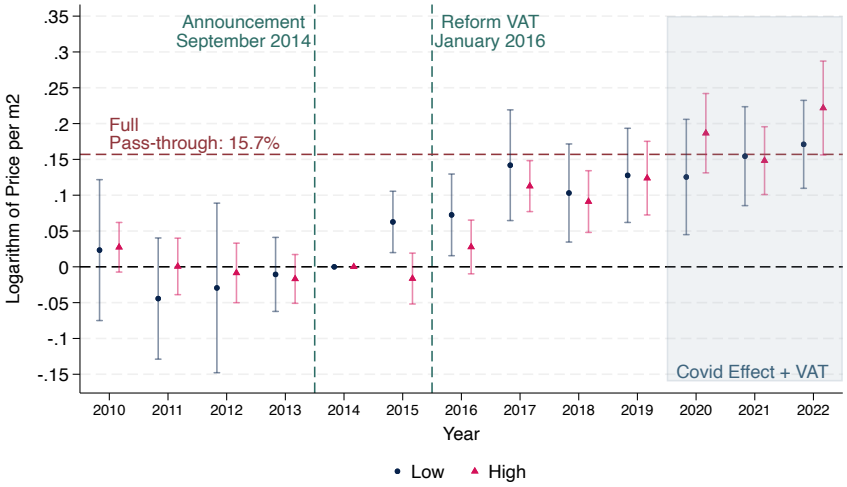
Note: This figure presents a map of census tracts classified as high-spillover (blue) and low-spillover (yellow) areas in the Metropolitan Region of Santiago for the period 2010–2013. A census tract is defined as high-spillover if more than 75% of transactions during this period correspond to new properties, and as low-spillover otherwise. Thicker lines denote municipality boundaries.

Figure 13: High and Low Spillover Census Tracts in the Metropolitan Region of Santiago de Chile



Note: The figure shows the coefficients β_j estimated from specification 11 where the outcome is the logarithm of transaction price per square meter. We restrict the sample to control units. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). High-spillover areas correspond to census tracts with more than 75% of transactions during 2010-2013 correspond to new properties, and as low-spillover otherwise. The regression includes municipality fixed effects, dummy variables for properties under 50 square meters, dummy variables indicating that the property's assessment is on the top quartile of the census tract distribution, if the property is located in a multi-unit complex and whether the unit includes a parking, storage, commercial or office space. We exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, as well as, transactions in rural census tracts and transactions for which first sale included VAT and resold after the VAT was implemented. Standard errors are clustered at the census tract level and lines represent 95% confidence intervals.

Figure 14: Heterogeneity Density



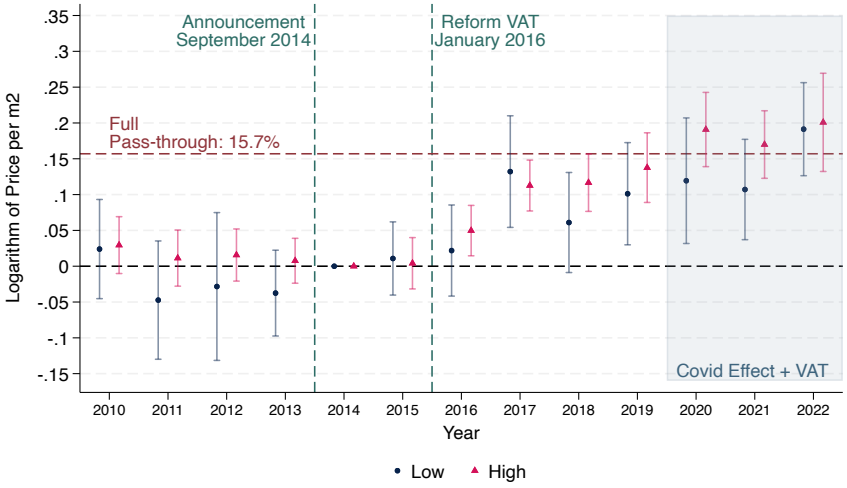
Note: Estimated $\hat{\beta}_j$ coefficients from equation (9) with log price per m^2 as the outcome, estimated separately for municipalities above (high) and below (low) the 75th percentile of the median pre-reform housing density, measured as residential properties per km^2. The dashed horizontal line indicates the full pass-through benchmark of 15.7%. 2014 is the omitted reference year. Standard errors clustered at the census tract level. Shaded areas represent 95% confidence intervals.

Figure 15: Heterogeneity Construction Intensity



Note: Estimated $\hat{\beta}_j$ coefficients from equation (9) with log price per m^2 as the outcome, estimated separately for municipalities above (high) and below (low) the 75th percentile of pre-reform construction intensity, measured as newly approved residential units relative to the total property stock in 2009. The dashed horizontal line indicates the full pass-through benchmark of 15.7%. 2014 is the omitted reference year. Standard errors clustered at the census tract level. Shaded areas represent 95% confidence intervals.

Figure 16: Heterogeneity Census Tract Median Appraisal



Note: Notes: Estimated $\hat{\beta}_j$ coefficients from equation (9) with log price per m^2 as the outcome, estimated separately for municipalities above (high) and below (low) the 75th percentile of the median pre-reform appraised property value. The dashed horizontal line indicates the full pass-through benchmark of 15.7%. 2014 is the omitted reference year. Standard errors clustered at the census tract level. Shaded areas represent 95% confidence intervals.

8 Tables

Table 2: Descriptive Statistics by Transaction Type

	Resale (0)	New (1)	p-value
Log. price per m^2	3.51 (0.44)	3.56 (0.36)	<0.001
Log. sale price	7.57 (0.62)	7.55 (0.56)	<0.001
Size (m^2)	62.31 (24.73)	57.62 (23.70)	<0.001
Log. appraised value per m^2	2.91 (0.61)	2.99 (0.43)	<0.001
Log. appraised value	6.98 (0.75)	6.97 (0.61)	0.007
Years since construction	4.10 (1.70)	0.77 (0.66)	<0.001
Observations	45,856	169,086	

Notes: The table reports means and standard deviations (in parentheses) for the pre-reform period (2010–2014) separately for resale transactions (column 0) and new apartment sales (column 1). Log price per m^2 and log sale price are expressed in Unidades de Fomento (UF). Appraised value is the SII assessed value used for property tax purposes. Years since construction measures the difference between the transaction year and the property's recorded construction year. The p-value corresponds to a two-sample t-test of equality of means between the two groups.

Table 3: Bunching Estimates - Robustness Checks

	Main	Polynomial degree (p)		Bunching window (LB)		Tolerance
Panel A: Elasticities						
ε_1	2.143	2.170	2.330	1.978	1.556	2.200
	(0.243)	(0.145)	(0.302)	(0.228)	(0.206)	(0.210)
ε_2	0.193	0.195	0.210	0.198	0.187	0.198
	(0.022)	(0.013)	(0.027)	(0.023)	(0.025)	(0.019)
Panel B: Excess and missing mass						
b (excess mass)	3.028	3.067	3.293	3.106	2.931	3.108
	(0.344)	(0.205)	(0.426)	(0.357)	(0.387)	(0.297)
m (missing mass)	3.074	2.928	3.196	3.087	2.869	2.818
	(1.013)	(0.571)	(1.372)	(1.013)	(1.024)	(0.878)
Panel C: Parameters						
Polynomial degree	3	2	4	3	3	3
Bunching window LB	-9	-9	-9	-10	-12	-9
Missing region UB	21	20	23	21	16	19
Tolerance	0.05	0.05	0.05	0.05	0.05	0.10

Notes: The first column reports the preferred specification, which uses a third-degree polynomial, a bunching window of $[\text{LB}, 0] = [-9, 0]$ months relative to the January 2016 VAT notch, and a convergence tolerance of 5%. The remaining columns report robustness checks varying one parameter at a time while holding all others at their preferred values. Columns 2–3 vary the polynomial degree to $p = 2$ and $p = 4$. Columns 4–5 extend the bunching window lower bound to LB$= -10$ and LB$= -12$ months. Column 6 relaxes the convergence tolerance to 10%. The missing region upper bound (UB) is determined recursively in all specifications by the convergence criterion $|\text{excess} - \text{missing}|/\text{excess} < \text{tolerance}$. b measures the total excess of contracts before the notch expressed in units of the average monthly counterfactual volume. m is the analogous measure of missing contracts after the notch. ε_1 measures the excess percentage of contracts relative to the counterfactual within the bunching window per percentage point of VAT. ε_2 measures average retiming in months per percentage point of VAT. Bootstrap standard errors using 500 replications following Chetty et al. (2011) in parentheses. All specifications use monthly data of purchase contracts of new apartments from 2010–2019 with an effective VAT rate of 15.7%.

Appendix

A Figures

Figure A.1: Advertisements in anticipation of the VAT reform



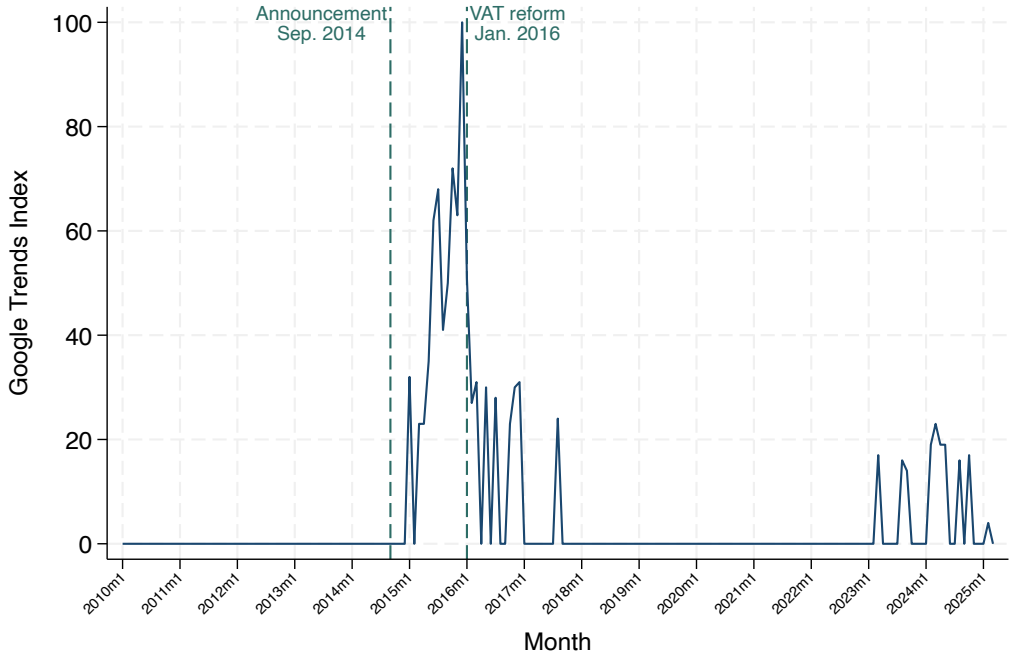
(a) Advertisement "Buy VAT free today"



(b) "Last months: take advantage and buy VAT free"

Note: The figure displays examples of advertising campaigns urging potential buyers to purchase homes before the VAT took effect. Taken from https://blog.toctoc.com/cerca-de-cuatro-mil-viviendas-sin-iva-quedan-para-la-venta-en-la-rm/ and https://www.elmostrador.cl/mercados/2015/12/01/compre-ahora-viviendas-sin-iva/

Figure A.2: Google Trends Index for online search of "VAT Housing"



Note: The figure depicts the monthly google trends index for the words "VAT Housing" (IVA vivienda) in Chile for the period 2010-2025. The index ranges between 0 and 100. It is normalized such that the month with the maximum search interest in the country takes value 100 (Google News Lab, 2024). Related queries included "VAT purchase housing" and "VAT purchase new housing".

B Tables

Table B.1: Specification Robustness Checks - Transaction Prices per Square Meter

	(1) Baseline	(2) No Controls	(3) Municipality FE	(4) Municipality Trends
Pre-reform (placebo)				
β_{2010}	0.029	0.031*	0.027	0.022
	(0.018)	(0.018)	(0.021)	(0.016)
β_{2011}	-0.005	-0.002	-0.032	-0.006
	(0.019)	(0.020)	(0.030)	(0.016)
β_{2012}	-0.001	0.001	0.003	-0.011
	(0.021)	(0.022)	(0.032)	(0.018)
β_{2013}	-0.006	-0.002	-0.009	-0.011
	(0.015)	(0.015)	(0.021)	(0.013)
Anticipation				
β_{2015}	0.004	0.005	0.010	0.004
	(0.015)	(0.015)	(0.019)	(0.013)
Post-reform				
β_{2016}	0.038**	0.035**	0.040**	0.038**
	(0.016)	(0.016)	(0.019)	(0.015)
β_{2017}	0.118***	0.120***	0.117***	0.115***
	(0.018)	(0.018)	(0.022)	(0.016)
β_{2018}	0.093***	0.090***	0.049*	0.098***
	(0.019)	(0.020)	(0.027)	(0.017)
β_{2019}	0.123***	0.123***	0.098***	0.126***
	(0.021)	(0.021)	(0.025)	(0.019)
β_{2020}	0.160***	0.157***	0.141***	0.162***
	(0.024)	(0.025)	(0.031)	(0.021)
β_{2021}	0.147***	0.149***	0.118***	0.151***
	(0.020)	(0.021)	(0.027)	(0.018)
β_{2022}	0.199***	0.199***	0.162***	0.185***
	(0.024)	(0.026)	(0.030)	(0.025)
Observations	693145	693145	693325	693145
Census Tract FE	Yes	Yes	No	Yes
Comuna FE	No	No	Yes	No
Year FE	Yes	Yes	Yes	No
Comuna × Year FE	No	No	No	Yes
Property Controls	Yes	No	Yes	Yes
Cluster Level	Census Tract	Census Tract	Comuna	Census Tract
Clusters	740	740	157	740
Adj. R-Squared	0.693	0.685	0.584	0.730

Notes: The table reports coefficients β_j from the equation 9 where the outcome is the logarithm of transaction price per square meter. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. Uses our baseline specification outlined in section 4. Column (2) replicates Column (1) without property controls. Column (3) replaces census tract fixed effects with municipality fixed effects and clusters standard errors at the municipality level. Column (4) includes census tract fixed effects and municipality × year fixed effects with property controls, clustered at the census tract level. All columns exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, transactions in rural census tracts, and transactions for which the first sale included VAT and were resold after VAT implementation. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Specification Robustness Checks: Transaction Prices

	(1) Baseline	(2) No Controls	(3) Municipality FE	(4) Municipality Trends
Pre-reform (placebo)				
β_{2010}	0.045**	0.044*	0.030	0.045***
	(0.019)	(0.024)	(0.027)	(0.017)
β_{2011}	0.024	-0.009	-0.015	0.037**
	(0.020)	(0.023)	(0.036)	(0.017)
β_{2012}	0.011	-0.017	0.004	0.003
	(0.020)	(0.023)	(0.033)	(0.017)
β_{2013}	-0.004	-0.023	-0.010	-0.013
	(0.015)	(0.017)	(0.024)	(0.014)
Anticipation				
β_{2015}	0.010	0.008	0.017	0.012
	(0.015)	(0.018)	(0.022)	(0.014)
Post-reform				
β_{2016}	0.045***	0.052***	0.046**	0.049***
	(0.017)	(0.020)	(0.021)	(0.016)
β_{2017}	0.098***	0.125***	0.097***	0.095***
	(0.019)	(0.021)	(0.025)	(0.017)
β_{2018}	0.070***	0.071***	0.017	0.091***
	(0.020)	(0.023)	(0.029)	(0.018)
β_{2019}	0.096***	0.095***	0.067**	0.112***
	(0.023)	(0.026)	(0.029)	(0.020)
β_{2020}	0.124***	0.121***	0.100**	0.133***
	(0.028)	(0.032)	(0.040)	(0.024)
β_{2021}	0.108***	0.121***	0.068**	0.116***
	(0.022)	(0.025)	(0.030)	(0.020)
β_{2022}	0.173***	0.169***	0.135***	0.172***
	(0.025)	(0.029)	(0.030)	(0.026)
Observations	693145	693145	693325	693145
Census Tract FE	Yes	Yes	No	Yes
Comuna FE	No	No	Yes	No
Year FE	Yes	Yes	Yes	No
Comuna × Year FE	No	No	No	Yes
Property Controls	Yes	No	Yes	Yes
Cluster Level	Census Tract	Census Tract	Comuna	Census Tract
Clusters	740	740	157	740
Adj. R-Squared	0.744	0.639	0.647	0.772

Notes: The table reports coefficients β_j from the equation 9 where the outcome is the logarithm of transaction price. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. Uses our baseline specification outlined in section 4. Column (2) replicates Column (1) without property controls. Column (3) replaces census tract fixed effects with municipality fixed effects and clusters standard errors at the municipality level. Column (4) includes census tract fixed effects and municipality × year fixed effects with property controls, clustered at the census tract level. All columns exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, transactions in rural census tracts, and transactions for which the first sale included VAT and were resold after VAT implementation. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Specification Robustness Checks - Property Size

	(1) Baseline	(2) No Controls	(3) Municipality FE	(4) Municipality Trends
Pre-reform (placebo)				
β_{2010}	0.016*	0.013	0.003	0.022**
	(0.009)	(0.016)	(0.011)	(0.010)
β_{2011}	0.029***	-0.007	0.017	0.043***
	(0.010)	(0.017)	(0.013)	(0.010)
β_{2012}	0.012	-0.018	0.001	0.014
	(0.011)	(0.017)	(0.013)	(0.010)
β_{2013}	0.002	-0.021*	-0.001	-0.001
	(0.009)	(0.012)	(0.012)	(0.010)
Anticipation				
β_{2015}	0.006	0.003	0.007	0.007
	(0.008)	(0.012)	(0.007)	(0.007)
Post-reform				
β_{2016}	0.007	0.017	0.006	0.011
	(0.008)	(0.013)	(0.009)	(0.009)
β_{2017}	-0.020**	0.006	-0.021**	-0.020**
	(0.008)	(0.012)	(0.008)	(0.008)
β_{2018}	-0.023**	-0.019	-0.032***	-0.007
	(0.009)	(0.014)	(0.009)	(0.009)
β_{2019}	-0.027***	-0.028**	-0.031***	-0.014*
	(0.009)	(0.014)	(0.009)	(0.008)
β_{2020}	-0.036***	-0.037**	-0.041***	-0.029***
	(0.011)	(0.017)	(0.014)	(0.009)
β_{2021}	-0.039***	-0.028*	-0.050***	-0.035***
	(0.009)	(0.014)	(0.010)	(0.009)
β_{2022}	-0.026***	-0.030*	-0.027***	-0.014
	(0.010)	(0.017)	(0.010)	(0.009)
Observations	693145	693145	693325	693145
Census Tract FE	Yes	Yes	No	Yes
Comuna FE	No	No	Yes	No
Year FE	Yes	Yes	Yes	No
Comuna × Year FE	No	No	No	Yes
Property Controls	Yes	No	Yes	Yes
Cluster Level	Census Tract	Census Tract	Comuna	Census Tract
Clusters	740	740	157	740
Adj. R-Squared	0.783	0.451	0.742	0.793

Notes: The table reports coefficients β_j from the equation 9 where the outcome is the logarithm of property size in square meters. Treatment units include new property transactions whereas control units include resale transactions. Uses our baseline specification outlined in section 4. Column (2) replicates Column (1) without property controls. Column (3) replaces census tract fixed effects with municipality fixed effects and clusters standard errors at the municipality level. Column (4) includes census tract fixed effects and municipality × year fixed effects with property controls, clustered at the census tract level. All columns exclude the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, transactions in rural census tracts, and transactions for which the first sale included VAT and were resold after VAT implementation. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Sample Robustness Checks - Transaction Prices per Square Meter

	(1) Baseline	(2) Incl. Outliers	(3) Incl. Post-VAT Resales	(4) Older Properties
Pre-reform (placebo)				
β_{2010}	0.029	0.010	0.032*	0.034**
	(0.018)	(0.021)	(0.018)	(0.017)
β_{2011}	-0.005	-0.014	-0.001	0.000
	(0.019)	(0.022)	(0.019)	(0.019)
β_{2012}	-0.001	-0.007	0.000	0.002
	(0.021)	(0.030)	(0.021)	(0.019)
β_{2013}	-0.006	-0.019	-0.006	-0.007
	(0.015)	(0.019)	(0.015)	(0.014)
Anticipation				
β_{2015}	0.004	0.006	0.001	0.011
	(0.015)	(0.022)	(0.015)	(0.017)
Post-reform				
β_{2016}	0.038**	0.034*	0.037**	0.033**
	(0.016)	(0.018)	(0.016)	(0.016)
β_{2017}	0.118***	0.109***	0.108***	0.101***
	(0.018)	(0.020)	(0.018)	(0.016)
β_{2018}	0.093***	0.104***	0.078***	0.067***
	(0.019)	(0.029)	(0.019)	(0.019)
β_{2019}	0.123***	0.109***	0.090***	0.096***
	(0.021)	(0.023)	(0.021)	(0.020)
β_{2020}	0.160***	0.149***	0.116***	0.133***
	(0.024)	(0.026)	(0.024)	(0.023)
β_{2021}	0.147***	0.136***	0.102***	0.129***
	(0.020)	(0.022)	(0.020)	(0.020)
β_{2022}	0.199***	0.193***	0.128***	0.181***
	(0.024)	(0.025)	(0.024)	(0.024)
Observations	693145	723371	717894	747719
1% Outliers	Excl.	Incl.	Excl.	Excl.
Post-VAT Resales	Excl.	Excl.	Incl.	Excl.
Construction Years	2006–2022	2006–2022	2006–2022	2000–2022
Clusters	740	755	748	778
Adj. R-Squared	0.693	0.687	0.687	0.681

Notes: The table reports coefficients β_j from equation 9 where the outcome is the logarithm of transaction price per square meter. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. All columns use the baseline specification outlined in Section 4, which includes census tract fixed effects, year fixed effects, and property controls, with standard errors clustered at the census tract level. Column (1) corresponds to the baseline sample, which excludes the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, transactions in rural census tracts, post-VAT resales of properties first sold under VAT, and restricts to properties built after 2005. Column (2) replicates Column (1) retaining observations in the top and bottom 1% tails of the price, price per square meter, and size distributions. Column (3) replicates Column (1) including resales of properties whose first sale occurred under VAT after implementation, which are excluded from the baseline to avoid contaminating the control group. Column (4) replicates Column (1) extending the sample to properties built after 1999, allowing for a longer pre-reform construction history. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Sample Robustness Checks - Transaction Prices

	(1) Baseline	(2) Incl. Outliers	(3) Incl. Post-VAT Resales	(4) Older Properties
Pre-reform (placebo)				
β_{2010}	0.045**	0.024	0.048**	0.047***
	(0.019)	(0.021)	(0.019)	(0.018)
β_{2011}	0.024	0.017	0.028	0.032*
	(0.020)	(0.023)	(0.020)	(0.019)
β_{2012}	0.011	0.003	0.012	0.022
	(0.020)	(0.029)	(0.020)	(0.019)
β_{2013}	-0.004	-0.016	-0.004	-0.002
	(0.015)	(0.019)	(0.015)	(0.015)
Anticipation				
β_{2015}	0.010	0.013	0.008	0.016
	(0.015)	(0.021)	(0.015)	(0.018)
Post-reform				
β_{2016}	0.045***	0.042**	0.045***	0.042**
	(0.017)	(0.019)	(0.017)	(0.016)
β_{2017}	0.098***	0.089***	0.090***	0.081***
	(0.019)	(0.021)	(0.018)	(0.017)
β_{2018}	0.070***	0.082***	0.060***	0.046**
	(0.020)	(0.030)	(0.020)	(0.020)
β_{2019}	0.096***	0.086***	0.072***	0.072***
	(0.023)	(0.024)	(0.023)	(0.022)
β_{2020}	0.124***	0.116***	0.090***	0.100***
	(0.028)	(0.029)	(0.028)	(0.027)
β_{2021}	0.108***	0.098***	0.072***	0.094***
	(0.022)	(0.024)	(0.022)	(0.022)
β_{2022}	0.173***	0.168***	0.114***	0.157***
	(0.025)	(0.026)	(0.026)	(0.026)
Observations	693145	723371	717894	747719
1% Outliers	Excl.	Incl.	Excl.	Excl.
Post-VAT Resales	Excl.	Excl.	Incl.	Excl.
Construction Years	2006–2022	2006–2022	2006–2022	2000–2022
Clusters	740	755	748	778
Adj. R-Squared	0.744	0.749	0.742	0.738

Notes: The table reports coefficients β_j from equation 9 where the outcome is the logarithm of transaction price. Prices are measured in the inflation-adjusted currency Unidades de Fomento (UF). Treatment units include new property transactions whereas control units include resale transactions. All columns use the baseline specification outlined in Section 4, which includes census tract fixed effects, year fixed effects, and property controls, with standard errors clustered at the census tract level. Column (1) corresponds to the baseline sample, which excludes the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, transactions in rural census tracts, post-VAT resales of properties first sold under VAT, and restricts to properties built after 2005. Column (2) replicates Column (1) retaining observations in the top and bottom 1% tails of the price, price per square meter, and size distributions. Column (3) replicates Column (1) including resales of properties whose first sale occurred under VAT after implementation, which are excluded from the baseline to avoid contaminating the control group. Column (4) replicates Column (1) extending the sample to properties built after 1999, allowing for a longer pre-reform construction history. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, , *** $p < 0.01$.

Table B.6: Sample Robustness Checks - Property Size

	(1) Baseline	(2) Incl. Outliers	(3) Incl. Post-VAT Resales	(4) Older Properties
Pre-reform (placebo)				
β_{2010}	0.016*	0.015	0.016*	0.012
	(0.009)	(0.010)	(0.009)	(0.008)
β_{2011}	0.029***	0.031***	0.029***	0.032***
	(0.010)	(0.011)	(0.010)	(0.010)
β_{2012}	0.012	0.010	0.012	0.020*
	(0.011)	(0.012)	(0.011)	(0.010)
β_{2013}	0.002	0.003	0.002	0.004
	(0.009)	(0.010)	(0.009)	(0.009)
Anticipation				
β_{2015}	0.006	0.006	0.007	0.005
	(0.008)	(0.008)	(0.008)	(0.008)
Post-reform				
β_{2016}	0.007	0.008	0.008	0.009
	(0.008)	(0.009)	(0.008)	(0.008)
β_{2017}	-0.020**	-0.020**	-0.018**	-0.020**
	(0.008)	(0.009)	(0.008)	(0.008)
β_{2018}	-0.023**	-0.023**	-0.019**	-0.021**
	(0.009)	(0.010)	(0.009)	(0.009)
β_{2019}	-0.027***	-0.023**	-0.019**	-0.024***
	(0.009)	(0.010)	(0.009)	(0.009)
β_{2020}	-0.036***	-0.033***	-0.026**	-0.032***
	(0.011)	(0.012)	(0.011)	(0.011)
β_{2021}	-0.039***	-0.038***	-0.030***	-0.035***
	(0.009)	(0.010)	(0.009)	(0.009)
β_{2022}	-0.026***	-0.025**	-0.014	-0.024**
	(0.010)	(0.011)	(0.010)	(0.010)
Observations	693145	723371	717894	747719
1% Outliers	Excl.	Incl.	Excl.	Excl.
Post-VAT Resales	Excl.	Excl.	Incl.	Excl.
Construction Years	2006–2022	2006–2022	2006–2022	2000–2022
Clusters	740	755	748	778
Adj. R-Squared	0.783	0.784	0.783	0.780

Notes: The table reports coefficients β_j from equation 9 where the outcome is the logarithm of property size per square meter. Treatment units include new property transactions whereas control units include resale transactions. All columns use the baseline specification outlined in Section 4, which includes census tract fixed effects, year fixed effects, and property controls, with standard errors clustered at the census tract level. Column (1) corresponds to the baseline sample, which excludes the top and bottom 1% of observations from each treatment group distribution of prices, prices per square meter, and size, transactions in rural census tracts, post-VAT resales of properties first sold under VAT, and restricts to properties built after 2005. Column (2) replicates Column (1) retaining observations in the top and bottom 1% tails of the price, price per square meter, and size distributions. Column (3) replicates Column (1) including resales of properties whose first sale occurred under VAT after implementation, which are excluded from the baseline to avoid contaminating the control group. Column (4) replicates Column (1) extending the sample to properties built after 1999, allowing for a longer pre-reform construction history. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, , *** $p < 0.01$.

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