The Assessment Gap in Quebec

Vertical and Horizontal Inequity in Municipal Property Valuation: Evidence from 522,769 Sales Matched to the Assessment Roll

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Abstract

Municipal property taxes are levied on assessed values, so any systematic relationship between assessment ratios and market value redistributes the tax burden within a municipality. We match 522,769 residential sales recorded in Quebec between 2021 and 2026 to the triennial assessment rolls of 625 municipalities at the parcel level and provide the first province-wide ratio study of vertical and horizontal assessment equity for a Canadian province. Because the *Loi sur la fiscalité municipale* pins every roll to a single statutory market-condition date, municipality $\times$ roll $\times$ sale-year fixed effects separate the mechanical drift of ratios between reference dates from within-market inequity by design. The central finding is that the measured degree of regressivity hinges on the treatment of extreme assessment ratios. Under a lenient 1/99 trim, the within-market elasticity of the assessment ratio with respect to price is -0.34 (-0.08 under a rank instrument in the spirit of Clapp, 1990) and 95% of municipalities fall below the IAAO price-related-bias band. Under the IAAO outlier rule, which removes 9% of sales concentrated in the bottom price decile, the elasticity is -0.09 (-0.01 and insignificant under the instrument), the province-wide PRB is $+0.02$, the COD is 17, and three quarters of municipalities meet the vertical-equity standard. A piecewise specification shows that proportionality holds across the upper 80% of each market's price distribution and fails only in the bottom quintile. Two patterns are invariant to the trimming rule: condominiums are assessed proportionally while single-family homes, plexes, older buildings, land-heavy properties and small municipalities remain mildly regressive, and within-municipality price-related bias widened in 2021–2022 when the post-pandemic boom pulled prices away from the rolls. The implied overpayment by the bottom within-market price decile ranges from about 6% to 65% depending on whether extreme-ratio sales are treated as market transactions, and the top decile underpays by 3–5%; narrowing that range requires sale-validation data that Quebec's registries do not currently provide.

Keywords: property tax, assessment ratio, vertical equity, regressivity, ratio study, measurement error, Quebec

JEL Classification: H71, H22, R51, R31

1. Introduction

The property tax is the fiscal backbone of Canadian local government and is, by statute, an *ad valorem* tax: every dwelling in a municipality is meant to be taxed in proportion to its market value. That proportionality rests entirely on the quality of the assessment roll. If assessors systematically overvalue inexpensive homes relative to expensive ones, the legal tax rate becomes a fiction—the effective rate falls with value, and the resulting redistribution from modest toward affluent neighbourhoods occurs without any legislative decision, embedded in a technical document that few taxpayers contest. A large literature documents this pattern in the United States, where assessment regressivity has been measured at national scale (Berry, 2021; Avenancio-León and Howard, 2022; Amornsiripanitch, 2022). Whether the same pathology afflicts Canadian assessment systems—centralised, professionally supervised, and widely presumed to be of high quality—has not been examined with sale-level data at the scale of a province.

This paper provides a province-wide ratio study for Quebec. We assemble 522,769 residential transactions recorded between January 2021 and July 2026 and match each sale, at the parcel level, to the triennial municipal assessment roll in force on the day of the sale. The match is performed on geographic coordinates and validated against the roll's recorded value; the median match distance is under one metre. The resulting file couples every sale price with the taxable value that generated the owner's tax bill, together with the roll's decomposition of that value into land and building components, structural descriptors (lot area, floor area, year built, number of units), and the statutory market-condition reference date of the roll. The data span 625 municipalities—from Montréal (79,581 usable sales) to towns with barely a hundred—and a housing cycle of unusual amplitude: the post-pandemic boom, the 2022–2023 interest-rate shock, and the subsequent recovery.

Quebec's institutional design makes it a clean laboratory for equity measurement. Every roll is redrawn on a three-year cycle, and article 46 of the *Loi sur la fiscalité municipale* requires that the values entered on a roll reflect market conditions on July 1 of the second fiscal year preceding the first year of the roll—a single reference date shared by every property in the municipality (Gouvernement du Québec, 2026; Ministère des Affaires municipales et de l'Habitation, 2024). Within a municipality $\times$ roll $\times$ sale-year block, every assessment therefore embodies the same market vintage, and any systematic relationship between assessment ratios and prices inside such a block is not a timing artefact. Our regression estimates absorb 2,884 such fixed effects, so that vertical inequity is identified from comparisons between cheap and expensive dwellings facing the same assessor, the same roll and the same market moment.

Three findings emerge, and the first qualifies the other two. First, the measured degree of regressivity depends decisively on how extreme assessment ratios are treated—a choice that ratio-study practice regulates explicitly (International Association of Assessing Officers, 2013) but

that the academic literature rarely reports. Under a lenient rule that trims only the 1st and 99th percentiles of the ratio within each roll vintage, the elasticity of the assessment ratio with respect to the sale price—zero under proportional assessment—is -0.34 (s.e. 0.03) in the fixed-effects log-log regression and -0.08 (s.e. 0.02) under a rank-based instrument in the spirit of Clapp (1990); 95% of municipalities then fall outside the IAAO band for price-related bias. Under the IAAO outlier rule, which removes ratios beyond 1.5 interquartile ranges within each market block—9% of sales, more than half of them in the bottom within-market price decile—the elasticity is -0.09 (s.e. 0.01) and the instrumental-variable estimate is -0.01 and statistically indistinguishable from zero; the province-wide coefficient of price-related bias is $+0.02$, inside the acceptable band, the coefficient of dispersion falls from 28 to 17, and three quarters of municipalities meet the vertical-equity standard. A piecewise-linear specification locates the discrepancy precisely: assessments are proportional to price across the upper 80% of each market's price distribution (β between 0.95 and 1.00) and lose contact with price only in the bottom quintile, where the extreme ratios sit. Whether those sales are over-assessed homes or non-market transfers recorded at prices below value cannot be settled with the registry data available in Quebec, and we therefore report the two treatments as bounds throughout.

Second, the *ordering* of difficulty is invariant to the trimming rule. Condominiums—homogeneous units with abundant comparables—are assessed proportionally under either treatment, while single-family homes and plexes, buildings over sixty years old, properties whose assessed value is mostly land, and municipalities with few sales remain regressive, at roughly a quarter of the lenient-trim magnitude once outliers are removed. This gradient tracks the difficulty of the mass-appraisal problem (Bostic et al., 2007; Gloudemans and Almy, 2011; McMillen and Weber, 2008) and is the part of the audit that survives every specification we tried. Horizontal dispersion follows the same map: the ten largest cities post trimmed CODs between 9 and 12, comfortably inside the IAAO ceiling, while 30% of municipalities—almost all small—exceed it.

Third, the statutory reference date lets us measure how far *staleness* contributes to the gap. The province-wide price-related bias is three to four times larger in 2021–2022 than afterwards, but most of that swing reflects composition across municipalities whose rolls were referenced on different markets. Within municipalities, the median price-related bias of the trimmed sample moves from -0.04 in 2021–2022, when the boom pulled prices away from rolls anchored in 2018–2020 conditions, to approximately zero from 2024 onward, and the within-market elasticity is essentially flat across roll-lag bands. Staleness therefore widens measured inequity at the margin but does not generate it.

These findings bear on the tax burden in a specific way. Because the levy is proportional to the rolled value within a municipality, a property whose assessment ratio exceeds the local norm by x% pays x% more tax than uniform assessment would imply. The median dwelling in the bottom within-

market price decile pays between 6% and 65% more than under uniform assessment, depending on whether extreme-ratio sales are counted as market transactions; the top decile pays 3–5% less under any treatment, and sorting on a value proxy that averages price and assessment, as the IAAO recommends, yields an overpayment near 6% for the bottom decile. The redistribution is real but an order of magnitude smaller than the lenient-trim figure suggests, and it is concentrated among the cheapest tenth of the stock. These results complement Carbonnier (2024), who documents the regressivity of Quebec's property tax relative to income with survey microdata but could not observe the assessment channel isolated here.

The paper makes two methodological contributions to the ratio-study literature. It exploits a statutory single-date reference regime to separate timing drift from vertical inequity by design, and it shows, on more than half a million sales, that the classical attenuation critique of Kochin and Parks (1982) and the practitioners' outlier rules of the IAAO interact: the lower tail of the ratio distribution is exactly where transaction noise and non-market sales concentrate, so that the measurement-error bias that Clapp (1990) sought to correct econometrically is largely a sample-screening problem. We make the dependence explicit by reporting the Cheng, Clapp and reverse-regression estimators (Kochin and Parks, 1982; Kennedy, 1984), quantile and piecewise profiles, and the full IAAO battery under five trimming rules.

The remainder of the paper proceeds as follows. Section 2 positions the paper in the ratio-study and property-tax-equity literatures. Section 3 describes Quebec's assessment institutions. Section 4 presents the data, the matched sample and the two trimming rules. Section 5 lays out the diagnostics, the econometric specifications and the measurement-error bounds. Section 6 reports the results, Section 7 the robustness battery, Section 8 discusses mechanisms and policy implications, and Section 9 concludes. Appendix A derives the bounds and collects supplementary tables.

2. Related literature

2.1. Measuring vertical inequity: from ratio studies to modern tests

The empirical study of assessment equity is nearly as old as the modern property tax. Its workhorse is the *sales ratio study*: for each sale i, compute the ratio $r_i = AV_i/SP_i$ of assessed value to sale price and ask whether r_i is uniform across the value distribution. The practitioners' canon—codified in the IAAO *Standard on Ratio Studies* (International Association of Assessing Officers, 2013) and the mass-appraisal manuals of Eckert et al. (1990) and Gloudemans and Almy (2011)— summarises a jurisdiction with the median ratio, the coefficient of dispersion (COD) for horizontal uniformity, and the price-related differential (PRD) or, since 2013, the coefficient of price-related bias (PRB) introduced by Gloudemans (2011) for vertical equity. The standard also prescribes how the sample is to be prepared before any statistic is computed: sales are screened for arm's-length status, ratios

beyond 1.5 interquartile ranges are flagged as outliers and trimmed within documented limits, and "statistics calculated from trimmed distributions … cannot be compared to those from untrimmed distributions" (International Association of Assessing Officers, 2013, Appendix B). We report every diagnostic for every Quebec municipality with sufficient sales, and we treat the screening step as part of the object of study rather than as a nuisance.

The academic literature grew out of dissatisfaction with summary measures. Cheng (1970, 1974) and Paglin and Fogarty (1972) proposed regression tests—Paglin and Fogarty (1972) in levels, where a positive intercept signals regressivity, Cheng (1974) in logarithms, where the slope β is an elasticity and $\beta < 1$ signals regressivity. Sunderman et al. (1990) and Birch and Sunderman (2014) added splines and structural breaks so that regressivity could differ across price segments, and Sirmans et al. (1995) and Sirmans et al. (2008) surveyed the proliferating battery. A decisive critique came from Kochin and Parks (1982): because the sale price is a noisy measure of market value, the log-log slope is attenuated toward zero even under perfectly proportional assessment, so ratio studies are biased *toward* finding regressivity. Kennedy (1984) replied that the reverse regression the critics proposed is biased in the opposite direction when assessments themselves carry error, and the exchange (Kochin and Parks, 1984) established that the two least-squares slopes bracket the truth under classical errors in both variables, a result that Klepper and Leamer (1984) formalised for the general errors-in-variables regression. Clapp (1990) proposed a rank-based instrumental-variables estimator whose coarse instrument is nearly orthogonal to transitory price noise; Goolsby (1997) measured assessment error directly, McMillen and Singh (2020) showed with quantile regressions that the correction can reverse conclusions, and McMillen and Singh (2023) and Quintos (2020) compared the sensitivity of the alternative vertical-equity measures to the shape of the ratio distribution. Our design follows this lesson: we report the naïve estimates, the Clapp instrument, the reverse-regression bound and piecewise and quantile profiles side by side, and we show that the attenuation problem and the outlier problem are two descriptions of the same tail.

2.2. Evidence: pervasive regressivity, mostly documented in the United States

Substantively, the modern consensus is that assessment regressivity is the rule rather than the exception. Berry (2021) computes ratio statistics for essentially every U.S. county and finds that the lowest-decile home within a jurisdiction faces an effective tax rate well above that of the top decile; Avenancio-León and Howard (2022) show that the same machinery generates a 10–13% assessment gap against Black and Hispanic homeowners, driven partly by unpriced neighbourhood attributes and partly by differential appeals; Amornsiripanitch (2022) attributes much of the gradient to assessors' inability to capture unobserved quality that scales with price. Earlier strands documented the pattern city by city—Engle (1975) in Boston, McMillen (2011) and McMillen and Weber (2008) in Illinois, where thin markets and infrequent reassessment emerge as structural drivers,

Hodge et al. (2017) in collapsing Detroit, and Cornia and Slade (2005) for multifamily housing. Ross (2012) shows that regressivity responds to the institutional incentives of assessors; Plummer (2014) and McMillen (2013) show how appeals reshape the ratio distribution, and Ihlanfeldt and Rodgers (2022) and McMillen and Singh (2026) how homestead exemptions interact with it. The mechanism menu that emerges—stale valuations, hard-to-value heterogeneous properties, thin markets, asymmetric appeals—guides our heterogeneity analysis. A point less often made explicit is that these studies differ widely in how they screen sales: some rely on validated arm's-length flags supplied by assessors, others on deed records with price filters. Our results suggest that this choice can move the measured elasticity by a factor of four.

2.3. Canada: a conspicuous gap

For Canada the evidence is thin. Harris and Lehman (2001) review the social and geographic incidence of residential assessment error and study Hamilton, Ontario, with assessment data from the 1980s and 1990s; Bird et al. (2012) chronicle Ontario's move to current-value assessment and the political economy of its phase-in provisions; Slack and Bird (2014) place Canadian reforms in an international perspective. None of these measures sale-level vertical equity at the scale of a province. For Quebec, Carbonnier (2024) establishes with survey microdata that property-tax payments are steeply regressive relative to *income*; the assessment channel—whether the tax base itself is mismeasured against *market value*—has not been examined at scale. This is the gap the present paper fills. The Canadian case is of independent interest because its institutions differ from the fragmented U.S. county system on the dimensions the U.S. literature blames: Quebec assessment is governed by a uniform provincial statute and manual (Gouvernement du Québec, 2026; Ministère des Affaires municipales et de l'Habitation, 2024), performed by chartered appraisers, and synchronised to a statutory market-condition date. Finding that the residual regressivity, once extreme ratios are set aside, is modest and concentrated where mass appraisal is hardest is consistent with that apparatus doing most of its job.

2.4. Hedonic foundations and incidence

Mass appraisal is applied hedonics (Rosen, 1974; Malpezzi, 2003): the assessor's model prices a bundle of structural and locational attributes, and equity failures are hedonic specification failures—unpriced land-share gradients (Bostic et al., 2007), depreciation curvature, or neighbourhood effects that Quebec practitioners have long modelled in the Quebec City market (Des Rosiers et al., 2000). Reading our subgroup estimates through this lens turns an audit into a diagnosis. Finally, the translation of assessment error into tax burdens raises an incidence question that we treat explicitly: the statutory burden falls on the owner of record, but property taxes are partly capitalised into prices

(Oates, 1969), and the progressivity of the tax as a whole depends on views about incidence that remain contested (Oates and Fischel, 2016). Our tax-shift calculations are statutory, not economic, incidence.

3. Institutional setting: property assessment in Quebec

Quebec's municipal fiscal regime is governed by the *Loi sur la fiscalité municipale* (LFM) and operationalised by the province's assessment manual, the *Manuel d'évaluation foncière du Québec* (Gouvernement du Québec, 2026; Ministère des Affaires municipales et de l'Habitation, 2024). Five features matter for the measurement design of this paper.

Triennial rolls drawn by a designated assessing body. Under article 14 of the LFM, the body responsible for assessment (the *organisme municipal responsable de l'évaluation*) has its roll drawn by its assessor every three years, for three consecutive fiscal years. Outside the large cities that hold their own assessment competence, that body is the regional county municipality (article 5), which either employs an in-house assessor or contracts a private firm; in every case the roll is signed by a chartered appraiser. The roll lists, for each assessment unit, the taxable value and its decomposition into land and building components, together with the structural descriptors used in valuation (lot area, floor area, year of construction, number of dwelling units, physical configuration, and a standardised use code, the CUBF). The three-year cycle is not absolute: the Minister may extend the life of a roll (article 14.2), and a roll that is not deposited on time remains in force (article 72). In our sample 98.5% of sales occur within 54 months of their roll's reference date, but the lag reaches 102 months for a small number of extended rolls.

A statutory market-condition date. Article 46 requires that the real value underlying the rolled value take account of the state of the unit and of market conditions "as they exist on July 1 of the second fiscal year preceding the first of those for which the roll is made": a roll entering force on January 1 of year t values every property as of July 1 of year $t-2$. This single reference date is common to every property in the municipality. Two implications follow. First, in a rising market the *level* of assessment ratios drifts mechanically below one as sales occur further from the reference date; the median lag in our sample is 36 months. This drift is a feature of the system, not an inequity, and our design absorbs it with municipality $\times$ roll $\times$ sale-year fixed effects. Second, *within* such a block every property shares the same market vintage, so cross-sectional patterns in ratios cannot be attributed to timing. The province itself measures the level: for each fiscal year the assessor establishes the median proportion of real value at which the roll stands and the corresponding comparative factor, which the Minister approves and uses for equalisation across municipalities

(article 264). Quebec thus runs an annual ratio study of *level*; it does not publish uniformity or vertical-equity statistics.

Taxation of the rolled value. The general property tax is the taxable value multiplied by the municipal rate for the property's category (articles 244.29–244.30); the non-residential, industrial, serviced vacant land, forestry and agricultural categories may carry distinct rates, while the dwellings in our sample all belong to the residual category and face a single rate within a municipality. School taxes are levied on the same base. There are no assessment-growth caps, homestead exemptions or acquisition-value rules of the Californian or Floridian type. Two provisions qualify the exactness of the mapping from assessed value to tax bill. A municipality may spread the change in taxable values caused by a new roll over the three years of the roll (*étalement*, article 253.27), in which case the taxable value in the first two years is a weighted average of the old and new rolled values, and it may cap the decrease in a tax bill at the entry of a new roll (article 253.51). Where these options are exercised, relative tax bills within a municipality deviate temporarily from relative rolled values; the deviation vanishes by the third year of each roll and does not alter the ranking of assessment ratios. Our tax-shift calculation is therefore exact for municipalities that do not phase in and an approximation elsewhere.

Review and appeal. An interested person may file a request for administrative review of an entry on the roll (article 124), in general before May 1 of the year the roll enters into force, and may then contest the assessor's response before the Tribunal administratif du Québec. We do not observe reviews or appeals, and the province publishes no systematic statistics on residential review rates. Unlike several U.S. jurisdictions studied in the literature (Plummer, 2014; Avenancio-León and Howard, 2022), the Quebec system has no exemption or cap whose take-up differs by price segment; the valuation model and the sales it is calibrated on are therefore the natural loci of any inequity we measure.

Sales information. Quebec has no mandatory declaration of arm's-length status at the time of registration. The transaction registry we use records the consideration stated in the deed, so family transfers, estate settlements, sales between related companies and distressed sales enter the data at whatever price was declared, and no administrative flag distinguishes them from market sales. This is the institutional root of the screening problem that Section 6 shows to be decisive, and it contrasts with jurisdictions where assessors validate sales before ratio studies are run (International Association of Assessing Officers, 2013).

Taken together, these institutions make Quebec a favourable environment for assessment equity: uniform provincial methodology, professional certification, statutory synchronisation of market

vintage, and a tax that consumes the rolled value with few exemption-driven distortions. They also leave the equity dimension unmonitored and the sales base unvalidated. The results below should be read against both facts.

4. Data and sample construction

4.1. Sources and matching

We start from a compiled registry of 745,119 residential-market transactions recorded in Quebec between January 2021 and July 2026, each carrying the sale price, sale date, civic address, geographic coordinates, and a listing-derived property type. Each transaction has been matched at the parcel level to the municipal assessment roll in force on the sale date. The match keys on geographic proximity between the transaction's coordinates and the roll unit's coordinates and is validated against the roll's recorded value; a composite score (maximum 220) summarises the quality of the address, distance and value agreement. Matches are tight: the median distance between the transaction and the matched roll unit is 0.6 metres, and every observation retained in the estimation sample reproduces the roll's assessed value exactly.

From the roll we observe the taxable value of the property (AV, `valeur immeuble`), its land and building components, lot area, total floor area, year of construction, number of dwelling units, the standardised use code (CUBF), the roll vintage, and the statutory market-condition reference date discussed in Section 3. The registry does not record whether a transaction was at arm's length.

4.2. Sample restrictions

Table 1 describes the estimation sample; the selection cascade is as follows. We keep sales of residential use codes—dwellings (CUBF 1000), cottages (1100), mobile homes (1211) and other residential (1990)—which removes vacant land, commercial property and construction in progress (672,276 sales remain). We require a high-confidence roll match (distance ≤ 50 m and score ≥ 150; 568,711 sales), a positive assessed value and a price of at least \$50,000, the floor of the source registry. Assessment ratios $r_i = AV_i/SP_i$ are then trimmed at the 1st and 99th percentiles *within each roll vintage*, so that the mechanical drift of ratio levels across vintages is not trimmed asymmetrically (557,325 sales). Finally, since all estimates compare sales within a municipality $\times$ roll $\times$ sale-year block ("cell"), we require at least 20 sales per cell. The estimation sample contains **522,769 sales** in 625 municipalities and 2,884 cells: 345,053 single-family homes, 81,693 plexes (2–5 units), 81,461 condominiums, 9,287 cottages, 4,701 mobile homes and 574 other residential units. Because rolls enter into force on January 1, a municipality–year almost always falls under a single roll; 85 of the 2,798 municipality–years in the sample span two rolls (late deposits and extensions), which is why

Table 1. Summary statistics, estimation sample

Variable	*n*	Mean	SD	P10	Median	P90
Sale price ($)	522,769	462,131	331,669	175,000	400,000	787,500
Assessed value ($)	522,769	378,576	284,509	155,000	309,400	664,500
Assessment ratio AV/SP	522,769	0.878	0.429	0.579	0.784	1.138
Roll lag (months)	522,769	37.1	11.3	22.5	36.2	50.7
Assessed land share	519,330	0.310	0.149	0.126	0.296	0.511
Building age (years)	498,458	43.6	30.1	10.0	39.0	79.0
Lot area (m^2)	522,475	1,406	10,906	112	556	2,258
Floor area (m^2)	515,958	137.6	84.9	75.9	112.0	225.9

Notes: 522,769 residential sales, January 2021 – July 2026, matched at the parcel level to the assessment roll in force at the sale date. The assessment ratio is assessed value divided by sale price. Roll lag is the number of months between the roll's statutory market-condition reference date (July 1) and the sale date. The assessed land share is the roll's land value divided by total assessed value.

cells are defined on the roll vintage as well as on the year.

4.3. Two treatments of extreme ratios

The 1/99 trim within vintage is deliberately lenient: it discards sales whose ratio is implausible for the market as a whole but retains, inside each municipality, every ratio that is implausible for that market. The retained distribution has a long upper tail—6.1% of sales have a ratio above 1.5 and 3.2% above 2, that is, a declared price below half the rolled value in a period when the typical dwelling sold for 28% more than its assessment. The IAAO standard treats such observations differently. Its Appendix B identifies as outliers the ratios that lie more than 1.5 interquartile ranges beyond the first and third quartiles of the stratum, requires that they be scrutinised and trimmed within documented limits (5 to 10% of a large, unvalidated sample), and warns that statistics from trimmed and untrimmed distributions are not comparable (International Association of Assessing Officers, 2013). Applied within each cell, the rule flags 9.2% of our sales. The flagged sales are not spread evenly: 53% of the sales in the bottom within-cell price decile are outliers, 16% in the second decile, and fewer than 5% in every decile above the fourth (Table 11). Whether they are market sales of severely over-assessed homes or non-market transfers recorded at nominal prices cannot be determined from the registry. We therefore carry both treatments through the paper: the *baseline* (lenient-trim) sample of 522,769 sales, and the *IAAO-trimmed* sample of 471,148 sales, and we report a grid of intermediate rules in Section 6.3.

4.4. The raw pattern

The median assessment ratio is 0.78: the typical dwelling sells for about 28% more than its rolled value, the expected imprint of a rising market on back-dated rolls. Figure 1 shows the distribution

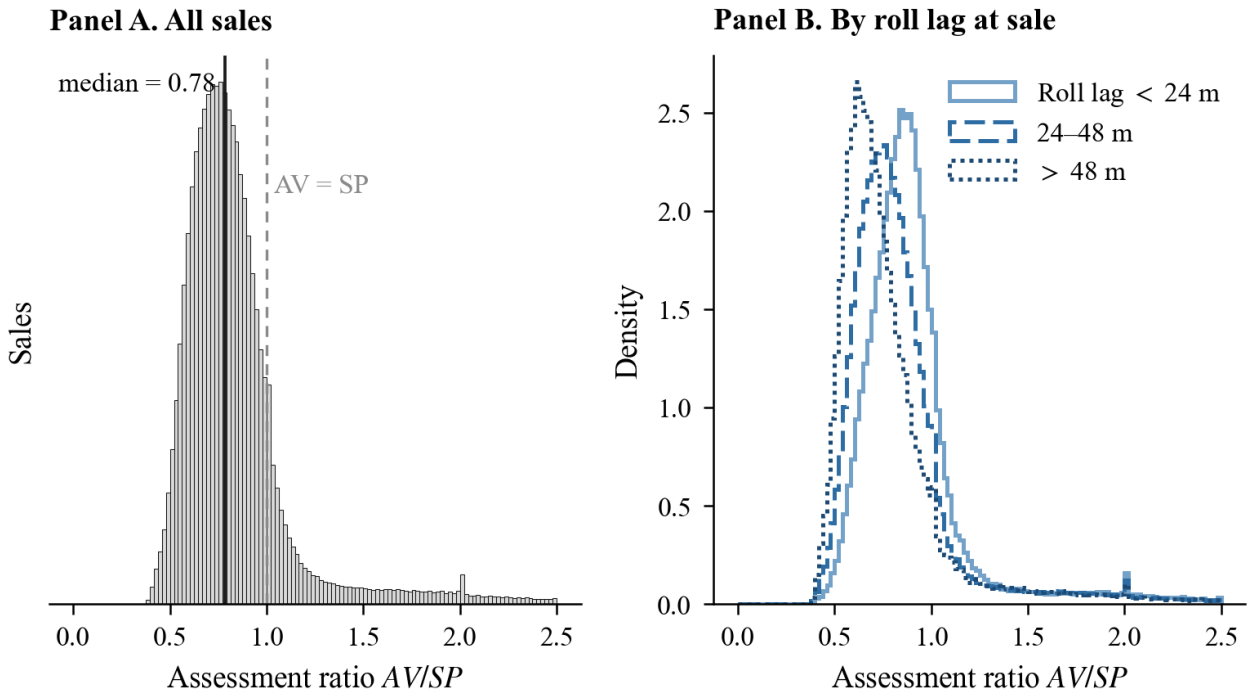


Figure 1. Assessment ratios, baseline sample. Panel A: distribution of AV/SP; the median is 0.78 and the dashed line marks parity. Panel B: kernel density of the same distribution split by the number of months between the roll's market-condition reference date and the sale; older rolls sit systematically further below parity.

and its decomposition by roll lag: sales occurring within two years of the reference date centre near 0.87, while sales more than four years out centre near 0.64—the staleness gradient that our fixed effects absorb. Figure 2 traces the same mechanics in calendar time: each roll vintage enters near parity with the market it was referenced on, then drifts down as prices rise, and the 2022–2023 rate shock is visible as a flattening of the drift. Everything that follows nets out this timing structure and asks a sharper question: *within* a given municipality, roll and year, do cheap and expensive homes face the same ratio?

5. Methodology

Let AV_i denote the assessed value of property i on the roll in force at its sale, SP_i its sale price, V_i its (unobserved) market value on the sale date, and $r_i = AV_i/SP_i$ the assessment ratio. Under proportional (vertically equitable) assessment, $\mathbb{E}[AV_i \mid V_i]$ is proportional to V_i and $\mathbb{E}[r_i \mid V_i]$ is constant. All tests below ask, in different ways, whether r_i instead declines with value, and all must confront the fact that V_i is observed only through two noisy proxies, SP_i and AV_i.

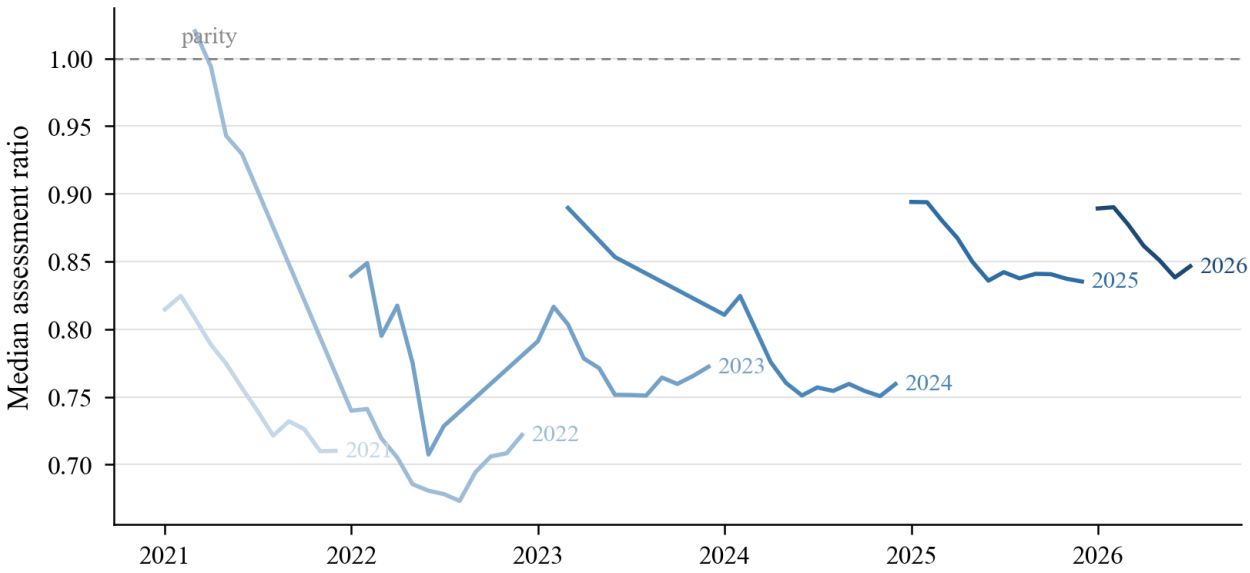


Figure 2. Median assessment ratio by sale month and roll vintage, baseline sample. Each line follows sales assessed under one triennial vintage (labelled by entry year); monthly medians with fewer than 100 sales are suppressed. Vintages enter near their reference-date market level and drift down as prices rise; the flattening after 2022 reflects the interest-rate correction.

5.1. IAAO ratio-study diagnostics

For any group of sales we report the four statistics of the IAAO *Standard on Ratio Studies* (International Association of Assessing Officers, 2013). The *median ratio* $\tilde{r}$ measures the assessment level. The *coefficient of dispersion*,

$$\mathrm{COD} = \frac{100}{n}\sum_{i=1}^{n}\frac{|r_i - \tilde{r}|}{\tilde{r}}, \tag{1}$$

measures horizontal uniformity; the standard's acceptable range is 5 to 15 for single-family dwellings and condominiums in older or heterogeneous areas, 5 to 10 in newer or homogeneous areas, and 5 to 20 for two- to four-unit buildings, seasonal and manufactured housing. We quote the 15-point ceiling as the reference for the sample as a whole, noting that it is stricter than the standard requires for plexes and cottages. The *price-related differential*,

$$\mathrm{PRD} = \frac{\bar{r}}{\sum_i AV_i / \sum_i SP_i}, \tag{2}$$

compares the unweighted mean ratio with the value-weighted (aggregate) ratio; values above 1.03 indicate regressivity, below 0.98 progressivity. Because the PRD is sensitive to extreme prices, the standard's preferred vertical measure is the *coefficient of price-related bias* (PRB), the slope b in

$$\frac{r_i - \tilde{r}}{\tilde{r}} = a + b \cdot \log_2\left(\tfrac{1}{2}SP_i + \tfrac{1}{2}AV_i/\tilde{r}\right) + u_i, \tag{3}$$

which measures the proportional change in the ratio per doubling of value, the value proxy being an equal-weight blend of the price and the median-deflated assessment (Gloudemans, 2011). The standard states that the PRB "should fall between -0.05 and 0.05" and that coefficients whose 95% confidence intervals lie outside $[-0.10, 0.10]$ "indicate unacceptable vertical inequities" (section 9.2.7). We report both thresholds. Confidence intervals for all four statistics are obtained by percentile bootstrap. Because ratio levels drift with roll staleness, municipality-level diagnostics are computed within municipality $\times$ sale-year blocks—inside which a single roll is almost always in force—and aggregated across years by the median; the robustness section verifies that finer time controls do not change the picture.

5.2. Regression tests with market-timing fixed effects

Our workhorse is the log-log specification of Cheng (1974),

$$\ln AV_i \;=\; \alpha_{c(i)} + \beta \, \ln SP_i + \varepsilon_i, \tag{4}$$

where $c(i)$ indexes the municipality $\times$ roll $\times$ sale-year cell of sale i and the α_c are 2,884 absorbed fixed effects. Proportionality implies $\beta = 1$; we report $\gamma \equiv \beta - 1$, the elasticity of the assessment *ratio* with respect to price ($\gamma < 0$: regressive). The fixed effects guarantee that γ is identified only from comparisons of dwellings assessed by the same authority, on the same roll vintage, and sold in the same year—purging the staleness drift, municipal composition, and aggregate market movements at once. For completeness we also report the pooled regression without fixed effects and the levels test of Paglin and Fogarty (1972), $AV_i = a + b\,SP_i + e_i$, where a positive intercept indicates regressivity.

Inference is clustered at the municipality level throughout (625 clusters). Clusters nest the fixed-effect cells, so the covariance estimator accommodates arbitrary dependence among sales of the same municipality across rolls and years, which is the level at which the assessing body and its model are fixed (Cameron and Miller, 2015; Abadie et al., 2023). Two caveats follow for the subsample estimates. Where a subsample retains few municipalities—eight for the largest markets, 66 for condominiums—cluster-robust standard errors are downward-biased and we report the number of clusters alongside them; where a single municipality is analysed, clustering is impossible and we report heteroskedasticity-robust standard errors, which understate the uncertainty attached to that municipality's assessment model.

5.3. Measurement error: attenuation, bounds and the Clapp instrument

Sale prices measure market value with idiosyncratic noise—bilateral bargaining, unobserved conditions of sale, and, in an unvalidated registry, non-market transfers—so least squares on equation (4)

is biased toward regressivity (Kochin and Parks, 1982). To make the bias explicit, write the within-cell (demeaned) variables as

$$\ln SP_i = \ln V_i + u_i, \qquad \ln AV_i = \beta \ln V_i + \varepsilon_i, \tag{5}$$

with u_i and ε_i mean-zero, mutually uncorrelated and uncorrelated with $\ln V_i$. The forward (Cheng) slope converges to

$$\operatorname{plim} \hat{\beta}_F \;=\; \beta \, \frac{\sigma_V^2}{\sigma_V^2 + \sigma_u^2} \;=\; \beta\,(1-\lambda), \qquad \lambda \equiv \frac{\sigma_u^2}{\sigma_V^2 + \sigma_u^2}, \tag{6}$$

where λ is the share of within-cell price variance that is noise. The attenuation is *larger* within cells than in the pooled regression, because the fixed effects remove the between-market variance of $\ln V$ but none of the transaction noise (Griliches and Hausman, 1986); this is why absorbing the cells steepens the slope in Table 4, and it is a reason not to read the difference between the pooled and fixed-effects estimates as evidence of within-market inequity. The reverse regression of $\ln SP$ on $\ln AV$ yields a slope $\hat{\beta}_R$ with $\operatorname{plim}(1/\hat{\beta}_R) = \beta + \sigma_\varepsilon^2/(\beta\sigma_V^2) \geq \beta$ (Kennedy, 1984), so that under (5)

$$\operatorname{plim} \hat{\beta}_F \;\leq\; \beta \;\leq\; \operatorname{plim} 1/\hat{\beta}_R, \tag{7}$$

the bracket of Klepper and Leamer (1984). The bracket is informative about regressivity only if its upper end lies below one; we report it, and we also report the noise share λ that would be needed to rationalise $\beta = 1$ given $\hat{\beta}_F$, so that the reader can judge the regressivity claim against external evidence on the dispersion of transaction prices around value.

Following Clapp (1990), we instrument $\ln SP_i$ with a coarse rank variable $Z_i \in \{-1, 0, +1\}$ that equals -1 for sales in the bottom third of *both* the within-cell $\ln AV$ and $\ln SP$ distributions, $+1$ for sales in the top third of both, and 0 otherwise. Because Z_i retains only ordinal information on which both measures of value agree, it is nearly orthogonal to the transitory component of either, and the two-stage least-squares estimate of β is approximately consistent under (5). The approximation is not exact: Z_i depends on the rank of AV_i and hence on ε_i, which biases the instrumental-variable slope *upward* when assessment errors are large, so that $\hat{\beta}_{IV}$ overstates β when $\hat{\beta}_F$ understates it. We therefore treat γ_{FE} and γ_{IV} as a descriptive bracket rather than as a point identification, and we complement them with the trimming grid of Section 6.3, which addresses the same tail from the sampling side. The within-cell implementation preserves the fixed-effects structure; the first stage is reported in Appendix A.

5.4. Where along the price distribution does proportionality fail?

Averaging can hide where proportionality fails, and two tools answer different versions of the question. A *piecewise-linear* version of equation (4), in the tradition of Sunderman et al. (1990) and Birch and Sunderman (2014), lets the slope differ across segments of the within-cell price distribution:

$$\ln AV_i \;=\; \alpha_{c(i)} + \beta_1\,\tilde{p}_i + \sum_{k=1}^{4} \delta_k\,(\tilde{p}_i - \kappa_k)_+ + \varepsilon_i, \tag{8}$$

where $\tilde{p}_i$ is the within-cell demeaned log price, the knots κ_k are its pooled quintile boundaries, and $(x)_+ = \max(x,0)$. The slope in the j-th quintile is $\beta_1 + \sum_{k<j}\delta_k$; it tells us whether the cheapest or the most expensive fifth of each market is where assessments decouple from prices. *Quantile regression* (Koenker and Bassett, 1978; McMillen and Singh, 2020) of the demeaned $\ln AV$ on the demeaned $\ln SP$ at $\tau \in \{0.10, 0.25, 0.50, 0.75, 0.90\}$ answers a different question: $\beta(\tau)$ is the slope of the τ-th conditional quantile of the assessment *given* the price, so a profile that declines in τ means that the conditional distribution of assessments narrows as prices rise—properties assessed far above their price are concentrated among cheap dwellings. We present both and are careful not to read the quantile index as a position in the price distribution. We then re-estimate the fixed-effects model on subsamples—property class, building-age bands, assessed-land-share bands, roll-lag bands, municipality size, and sale year—keeping only cells that retain at least 20 sales, to locate the inequity where the mass-appraisal problem is hardest.

5.5. Horizontal inequity

Vertical tests concern the *mean* of r_i given value; horizontal equity concerns its *dispersion* among comparable properties (Allen and Dare, 2002; Sirmans et al., 2008). Beyond the COD, we ask who receives noisy assessments: we regress the absolute deviation of a sale's log ratio from its cell median, $|\ln r_i - \mathrm{med}_{c(i)} \ln r|$, on property characteristics (age, land share, property-class indicators) with cell fixed effects.

5.6. The implied tax shift

Because the general property tax applies a single rate to the taxable value of every dwelling in the residual category of a municipality (Section 3), a property whose ratio exceeds a benchmark ratio $\bar{r}_c$ by $x\%$ pays $x\%$ more tax than it would if every property were assessed at $\bar{r}_c$. For each sale we compute the relative assessment error

$$e_i \;=\; \frac{r_i}{\bar{r}_{c(i)}} - 1, \tag{9}$$

and summarise it by within-cell decile of a sorting variable. Three choices matter and we report each. The *benchmark* $\bar{r}_c$ is either the cell median ratio—the IAAO's measure of level, which makes e_i the deviation of the typical property—or the aggregate ratio $\sum AV / \sum SP$, which is revenue-neutral: if every property were assessed at the aggregate ratio and the rate adjusted to hold the levy constant, the percentage change in each bill would equal e_i. The *sorting variable* is the sale price, the IAAO blended proxy $\frac{1}{2}SP_i + \frac{1}{2}AV_i/\tilde{r}_c$, or the assessed value; because the price contains the noise u_i, sorting on it places negative price shocks in the bottom decile and mechanically inflates the measured overpayment there, while sorting on the assessed value does the opposite, so the two bracket the proxy-based estimate. The *sample* is the baseline or the IAAO-trimmed sample. Finally, the calculation is one of statutory incidence: it describes the bill delivered to the owner of record and abstracts from capitalisation of the tax into prices (Oates, 1969), from the phase-in provisions of Section 3, and from school taxes, which are levied on the same base at a province-wide rate and therefore shift in the same proportion.

6. Results

6.1. Province-wide diagnostics, baseline sample

Table 2 reports the IAAO statistics for the baseline sample and by sale year. Three facts stand out. First, dispersion is high: the province-wide COD is 28.5, and within municipality–year blocks the median municipality posts a COD of 26, against a ceiling of 15 for single-family dwellings; 99% of the 261 municipalities with at least 100 usable sales exceed it (Figure 3). Second, every vertical measure points toward regressivity: the PRD is 1.072 (acceptable range 0.98–1.03) and the PRB is -0.029 with a bootstrap interval far from zero—ratios fall by about 3% of the median with every doubling of value. Third, the province-wide PRB varies strongly over time, from -0.104 and -0.117 in 2021 and 2022 to -0.002 in 2024. Section 7 shows that most of this swing is compositional. These statistics are computed on an untrimmed-within-market distribution; Section 6.3 shows how each of them changes under the IAAO outlier rule.

Table 3 disaggregates the ten largest markets on the baseline sample. The PRB is negative in nine of them in every sale year; Montréal's is $+0.07$. Figure 5 maps the municipal estimates and Figure 4 their distribution: 99% of the 261 municipalities have a negative PRB, 95% fall below -0.05 and 84% below -0.10, the standard's threshold for unacceptable inequity. We return to the largest markets in Section 6.8, where the Montréal contrast turns out to be weaker than these level statistics suggest.

Table 2. IAAO ratio-study diagnostics, province-wide, baseline sample

Sample	n	Median ratio	COD	PRD	PRB
All sales 2021–2026	522,769	0.784	28.5	1.072	-0.0292
		[0.784, 0.785]	[28.3, 28.6]	[1.071, 1.073]	[-0.0312, -0.0272]
2021	115,221	0.759	26.6	1.098	-0.1044
		[0.757, 0.760]	[26.4, 26.8]	[1.096, 1.100]	[-0.1078, -0.1013]
2022	90,627	0.700	30.2	1.114	-0.1165
		[0.699, 0.701]	[29.9, 30.5]	[1.112, 1.117]	[-0.1209, -0.1107]
2023	74,110	0.768	30.2	1.067	-0.0146
		[0.767, 0.769]	[29.8, 30.4]	[1.064, 1.069]	[-0.0198, -0.0085]
2024	89,204	0.765	29.5	1.061	-0.0022
		[0.764, 0.767]	[29.2, 29.8]	[1.059, 1.063]	[-0.0071, 0.0031]
2025	102,026	0.851	25.5	1.070	-0.0389
		[0.850, 0.851]	[25.2, 25.7]	[1.068, 1.072]	[-0.0440, -0.0344]
2026	51,581	0.857	25.5	1.068	-0.0305
		[0.855, 0.858]	[25.1, 25.9]	[1.065, 1.071]	[-0.0383, -0.0240]

Notes: Median assessment ratio, coefficient of dispersion (COD), price-related differential (PRD) and coefficient of price-related bias (PRB), computed on the baseline estimation sample (1/99 trim by roll vintage); 95% percentile-bootstrap confidence intervals (200 replications) in brackets. IAAO acceptable ranges: COD ≤ 15 (single-family residential, older or heterogeneous areas; ≤ 20 for 2–4 unit, seasonal and manufactured housing), PRD $\in [0.98, 1.03]$, PRB $\in [-0.05, 0.05]$.

6.2. Regression estimates

Table 4 presents the econometric core on the baseline sample. Figure 6 shows the raw material: within municipality $\times$ roll $\times$ year cells, mean log assessment ratios fall as log prices rise, steeply through the bottom vigintiles and almost not at all through the top half of the distribution. The pooled Cheng (1974) elasticity is $\beta = 0.812$ (s.e. 0.029); absorbing the 2,884 cell fixed effects moves it to $\beta = 0.656$, i.e. $\gamma_{\text{FE}} = -0.344$: within the same market and vintage, a home priced 10% above its neighbours is assessed about 6.6% higher, so its ratio is 3.4% lower. As Section 5.3 explains, part of the steepening from the pooled to the within estimate is the mechanical amplification of attenuation bias by the fixed effects, not additional inequity. The levels test of Paglin and Fogarty (1972) agrees in sign: the intercept is \$31,729 (s.e. \$5,378) with a slope of 0.751.

How much of this is measurement-error artefact? The Clapp (1990) instrument delivers $\beta_{\text{IV}} = 0.917$ (s.e. 0.021): the instrument accounts for three quarters of the naïve gap, as the critique of Kochin and Parks (1982) predicts, and the corrected elasticity remains four standard errors below one. The reverse-regression bound of equation (7) is less favourable to the regressivity reading: the inverse reverse slope is $1/\hat{\beta}_R = 1.137$, so the classical errors-in-variables bracket $[0.656, 1.137]$ contains proportionality (Appendix Table 17). The bracket is wide because assessment errors are large—the within-cell correlation between $\ln AV$ and $\ln SP$ is only 0.76—and it can be narrowed only with an assumption about the dispersion of transaction prices around value. For the point

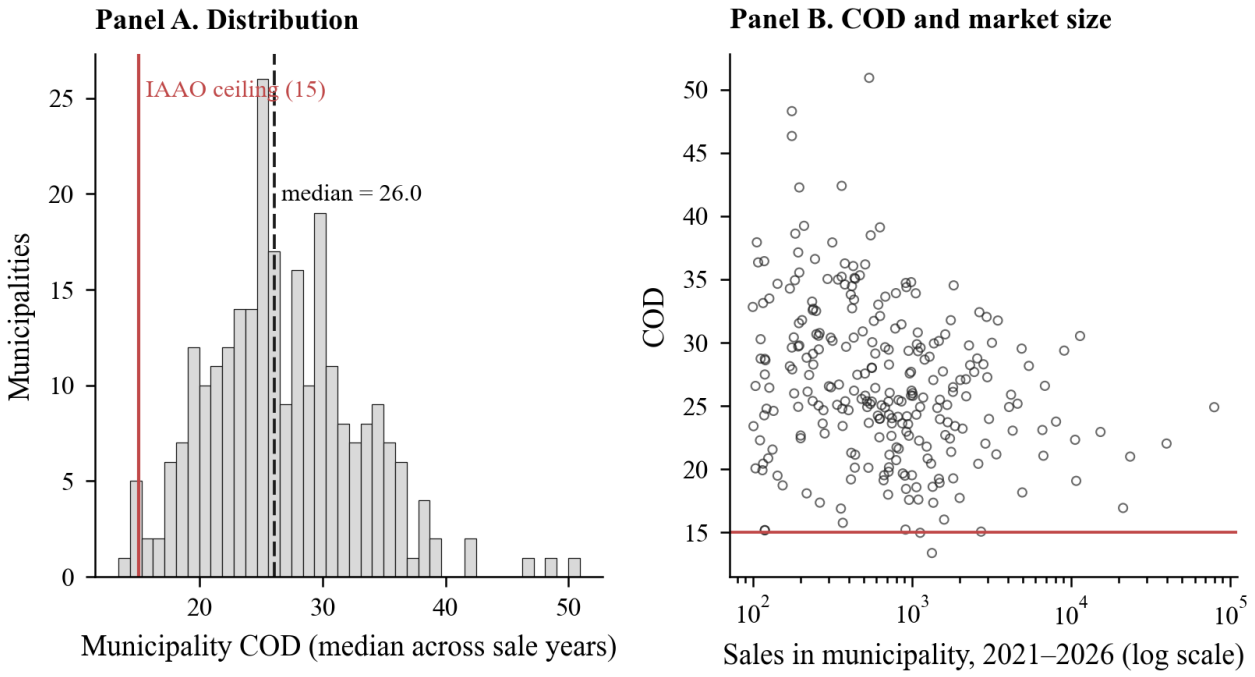


Figure 3. Horizontal uniformity, baseline sample. Panel A: distribution of municipality-level CODs (median across sale-year blocks); the IAAO ceiling for single-family residential property is 15. Panel B: COD against the number of sales in the municipality; dispersion is worst in the thinnest markets. Table 5 reports the corresponding shares after IAAO outlier trimming.

estimate $\hat{\beta}_F = 0.656$ to be consistent with $\beta = 1$, 34% of the within-cell variance of log prices would have to be transaction noise, a standard deviation of 0.28 log points, or roughly 28% of value; the Clapp estimate itself implies a noise standard deviation of 0.25. Hedonic models fitted to comparable Quebec data achieve out-of-sample median absolute errors of 14–17% *including* model misspecification, so a purely transactional noise of 25–28% is implausible for arm's-length sales. It is, however, entirely plausible for a sample that contains non-market transfers recorded at nominal prices, which is why the next subsection approaches the same tail from the sampling side.

6.3. Sensitivity to the treatment of extreme ratios

Table 5 re-estimates the entire battery under five treatments of the ratio tail: the baseline, two IAAO-style interquartile rules applied within cells (3 and 1.5 interquartile ranges), and symmetric percentile trims within cells (2.5/97.5 and 5/95). The IAAO rule removes 9.2% of sales—within the standard's 5–10% limit for large unvalidated samples—and changes every headline statistic. The province-wide COD falls from 28.5 to 17.0, the PRD from 1.072 to 1.004, and the PRB from -0.029 to $+0.025$, inside the acceptable band. The within-cell elasticity falls from -0.344 to -0.088 (s.e. 0.013) and the Clapp estimate from -0.083 to -0.013 (s.e. 0.009), which is not significantly different from zero. The median municipality's COD falls from 26.0 to 12.5; the share

Table 3. Assessment equity in the ten largest markets, baseline sample

Municipality	n	Median ratio	COD	PRD	PRB	Share of years PRB < 0
Montréal	79,581	0.892	24.9	1.057	0.0697	0.00
Québec	39,927	0.809	22.0	1.071	-0.0480	1.00
Laval	23,569	0.729	21.0	1.076	-0.1677	1.00
Gatineau	21,270	0.798	16.9	1.050	-0.1366	1.00
Longueuil	15,343	0.699	22.9	1.074	-0.1276	1.00
Sherbrooke	11,413	0.744	30.5	1.137	-0.3872	1.00
Saguenay	10,794	0.787	19.1	1.064	-0.1885	1.00
Lévis	10,622	0.827	22.3	1.089	-0.2369	1.00
Trois-Rivières	9,070	0.729	29.4	1.123	-0.3167	1.00
Terrebonne	8,061	0.795	23.8	1.096	-0.2896	1.00

Notes: Statistics computed within municipality $\times$ sale-year blocks (a single roll in force per block) and aggregated across years by the median; n is the total number of sales. The last column is the share of the six sale years in which the municipality's PRB is negative. Table 12 reports the within-city elasticities and the statistics after IAAO outlier trimming.

of municipalities above the 15-point ceiling falls from 99% to 30%, the share with a PRB below -0.05 from 95% to 24%, and the share below -0.10 from 84% to 5%. The intermediate rules trace a monotone path between the two ends: the more of the ratio tail is removed, the closer Quebec assessments come to proportionality.

Two readings of this table are possible, and the data cannot adjudicate between them. If the trimmed sales are market transactions, the baseline describes Quebec's assessment system and the IAAO rule discards its worst failures—precisely the "outliers concentrated in certain areas or other subsets of the sample" that the standard says "can point directly to systematic errors in the appraisal process" (International Association of Assessing Officers, 2013, Appendix B.5). If they are non-market transfers—estate settlements, intra-family sales, sales between related parties, recorded at the nominal consideration stated in the deed—then the baseline measures a property of the registry, not of the roll, and the trimmed sample is the relevant one. Three pieces of evidence favour the second reading for most, though not all, of the flagged sales. The outliers are overwhelmingly on the high-ratio side and concentrated in the bottom price decile (Table 11), where a price below half the rolled value is common and where deeds recording a transfer "for one dollar and other good consideration" are known to cluster. Dropping all sales below \$100,000, which removes 3% of the sample, moves the fixed-effects elasticity from -0.34 to -0.30 but has little effect on the rest of the distribution. And the piecewise profile below shows that assessments are proportional to price everywhere except in the bottom quintile, which is not the shape a systematically misspecified valuation model would produce. We therefore read the baseline as an upper bound on vertical inequity and the IAAO-trimmed sample as a lower bound, and we present the remaining results

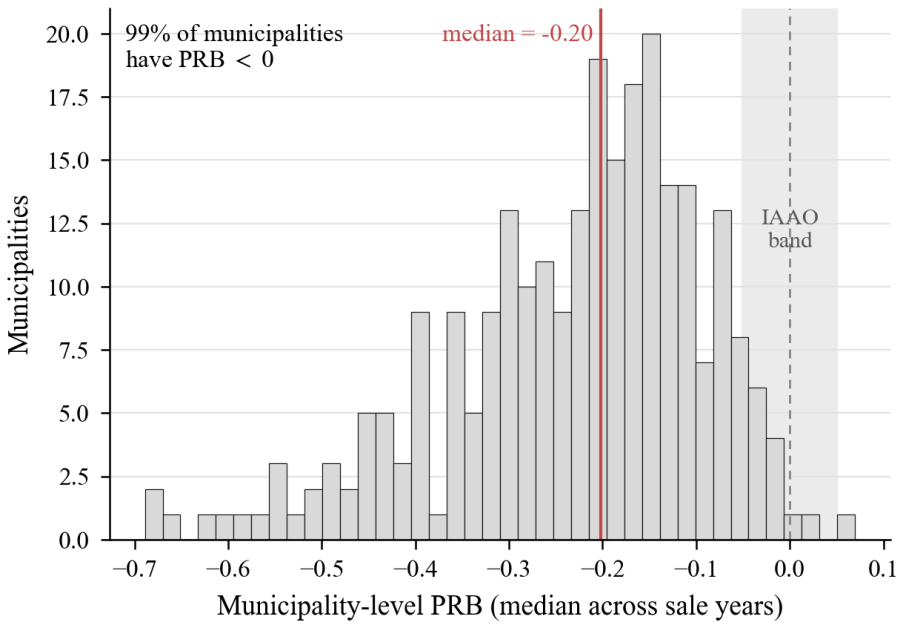


Figure 4. Distribution of municipality-level PRB across the 261 municipalities with at least 100 usable sales, baseline sample. The shaded band is the IAAO acceptable range $[-0.05, 0.05]$.

under both.

6.4. Where proportionality fails: piecewise and quantile profiles

Table 6 reports the piecewise-linear specification (8) on the baseline sample. The slope is 0.07 (s.e. 0.02) in the bottom within-cell price quintile—assessments are essentially unrelated to prices among the cheapest fifth of each market—and 0.89, 0.71, 1.00 and 0.95 in the four quintiles above. Across the upper 80% of the price distribution, in other words, the elasticity is between 0.95 and 1.00 in two quintiles and the deviations from one in the other two are modest; proportionality fails at the bottom, not at the top. Under the IAAO rule the bottom-quintile slope rises to 0.87 and the top-quintile slope is unchanged at 0.95 (Table 5), so the one segment where the two samples disagree is the one that contains the outliers.

Figure 7 and Table 14 report the quantile profile. $\beta(\tau)$ is 0.87 at $\tau = 0.10$ and 0.86 at $\tau = 0.25$, then falls to 0.70 at $\tau = 0.75$ and 0.49 at $\tau = 0.90$. The index τ refers to the conditional distribution of the assessment given the price, not to the price distribution: the profile says that the upper conditional quantiles of $\ln AV$—properties assessed far above what their price would predict—rise much less than proportionally with price, so that the conditional dispersion of assessments shrinks as prices rise. This is the same fact as the piecewise profile seen from the other axis: the thick over-assessed tail lives among cheap dwellings. The pattern matches McMillen and Singh (2020), who find the steepest departures from proportionality at the upper conditional quantiles in Illinois, and it is what one expects if the lower tail of prices mixes market sales with transfers at nominal

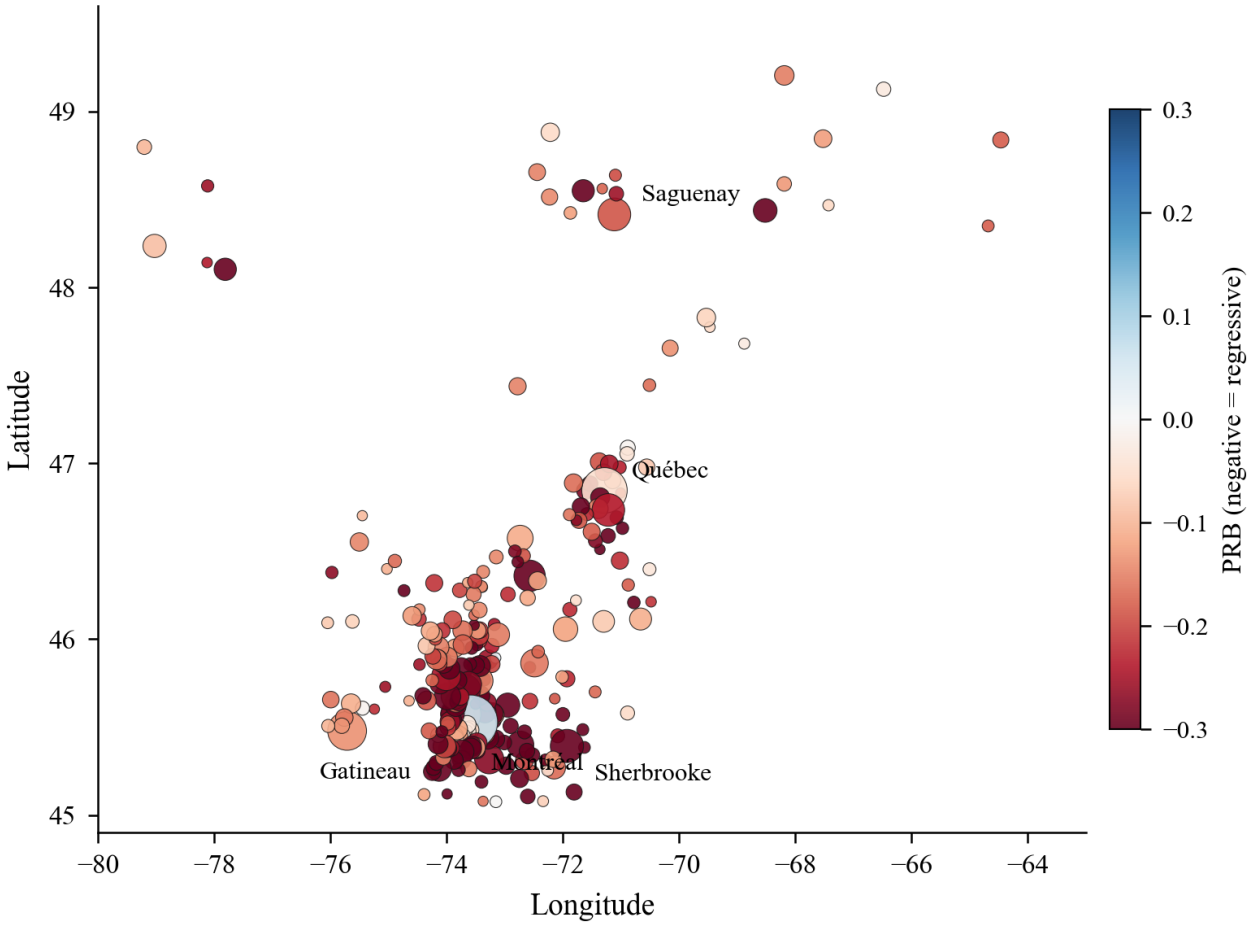


Figure 5. The geography of price-related bias, baseline sample. Municipality-level PRB (median across sale-year blocks); bubble area is proportional to the square root of the number of sales. Montréal is the only large jurisdiction with a positive PRB on this sample; see Table 12 for the within-city elasticities and the trimmed statistics.

prices.

6.5. The anatomy of regressivity

Figure 8 and Table 7 dissect γ_{FE} by subsample on the baseline sample, and Table 8 repeats the exercise on the IAAO-trimmed sample with the Clapp estimate alongside. The ordering is the same in both and reads like a map of mass-appraisal difficulty. Condominiums—homogeneous units with abundant comparables—show $\gamma_{\text{FE}} = -0.12$ on the baseline and 0.00 after trimming, with Clapp estimates of $+0.06$ and $+0.08$; single-family homes show -0.45 and -0.12, plexes -0.49 and -0.12. Regressivity deepens with building age (-0.30 to -0.43 on the baseline, -0.04 to -0.14 trimmed) and with the assessed land share (-0.26 to -0.50, and -0.07 to -0.14): land is the component without construction-cost anchoring (Bostic et al., 2007), and properties whose value is mostly land are assessed worst. Municipalities with more than 10,000 sales do better (-0.27

Table 4. Vertical-inequity regressions, baseline sample

	(1) Cheng OLS	(2) Cheng FE	(3) Clapp IV
β (ln sale price)	0.812***	0.656***	0.917***
	(0.029)	(0.029)	(0.021)
$\gamma = \beta - 1$	-0.188	-0.344	-0.083
Cell fixed effects	No	Yes	Yes
Error-in-price robust	No	No	Yes
Observations	522,769	522,769	522,769
R^2	0.705	0.796	0.486

Notes: Dependent variable: $\ln AV$. Column (1): pooled OLS. Column (2): absorbing least squares with 2,884 municipality $\times$ roll $\times$ sale-year fixed effects. Column (3): within-cell 2SLS using the Clapp (1990) rank instrument $Z \in \{-1, 0, +1\}$; first-stage coefficient 0.509 (s.e. 0.010), cluster-robust $F = 2{,}418$. Standard errors clustered by municipality (625 clusters) in parentheses. Stars test $H_0 : \beta = 1$ (proportional assessment); *** $p < 0.01$.

and -0.05) than those with fewer than 1,000 (-0.38 and -0.14), consistent with the thin-market mechanism of McMillen and Weber (2008), although the large-market estimate rests on eight clusters and its standard error should be read with caution. After trimming, every Clapp estimate lies between -0.07 and $+0.08$: the residual regressivity that survives both the outlier rule and the measurement-error correction is small in every subgroup, and the subgroups differ mainly in how much of the lenient-trim gap they carried.

6.6. Horizontal inequity

Table 9 reports the dispersion regression on the baseline sample. The mean absolute deviation of a sale's log ratio from its cell median is 0.188: the typical Quebec dwelling is assessed 19% away from its market's norm, a figure that the IAAO rule reduces to about 12% (the median municipal COD of Table 5). The deviation rises by 0.75 percentage points per decade of building age and by 5.2 points for cottages, and falls by 5.5 points for condominiums; the coefficients on the assessed land share and on plexes are not statistically different from zero, so dispersion is a matter of age and property type rather than of land intensity. The regression explains little of the dispersion ($R^2 = 0.05$ within cells): most horizontal noise is idiosyncratic to the property.

6.7. The implied tax shift

Figure 9 and Table 10 translate the baseline estimates into tax bills under the conventions of the original calculation: sales sorted on price, cell-median benchmark. The median dwelling in the

Table 5. Sensitivity of the audit to the treatment of extreme assessment ratios

Panel A. Province-wide statistics and within-cell elasticities

Outlier rule	Dropped (%)	COD	PRD	PRB	γ_{FE}	γ_{IV}
Baseline (1/99 pct by vintage)	0.0	28.5	1.072	-0.029	-0.344***	-0.083***
					(0.029)	(0.021)
3 × IQR within cell	6.6	18.2	1.011	+0.012	-0.124***	-0.027**
					(0.018)	(0.012)
1.5 × IQR within cell (IAAO)	9.2	17.0	1.004	+0.025	-0.088***	-0.013
					(0.013)	(0.009)
2.5/97.5 pct within cell	5.4	22.1	1.032	-0.002	-0.223***	-0.058***
					(0.027)	(0.019)
5/95 pct within cell	10.5	18.8	1.015	+0.011	-0.151***	-0.040**
					(0.023)	(0.016)

Panel B. Tax shift and municipal compliance

Outlier rule	Decile 1 (%)	Decile 10 (%)	Median muni. COD	COD >15 (%)	PRB <0 (%)	PRB <-0.05 (%)
Baseline (1/99 pct by vintage)	+64.9	-4.7	26.0	99	99	95
3 × IQR within cell	+9.4	-3.6	14.4	44	83	49
1.5 × IQR within cell (IAAO)	+6.6	-3.2	12.5	30	68	24
2.5/97.5 pct within cell	+24.5	-3.6	19.3	80	96	85
5/95 pct within cell	+13.1	-2.9	15.5	55	91	66

Notes: Each row re-runs the full analysis on the sample obtained by applying the stated outlier rule; interquartile and percentile rules are applied to the ratio within each municipality × roll × sale-year cell, after which cells with fewer than 20 sales are dropped. Panel A: province-wide COD, PRD and PRB and the Cheng fixed-effects and Clapp IV elasticities (municipality- clustered standard errors in parentheses; stars test $\gamma = 0$ at 10/5/1%). Panel B: median relative assessment error of the bottom and top within-cell sale-price deciles (cell-median benchmark), and municipality-level compliance among municipalities with at least 100 retained sales (261 in the baseline, 241 under the IAAO rule).

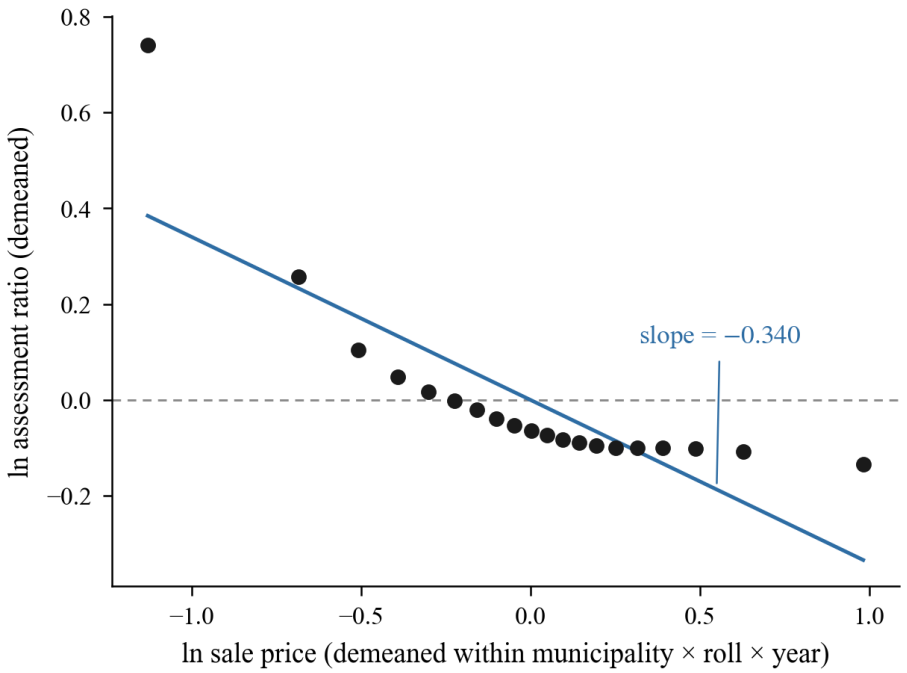


Figure 6. Vertical inequity within markets, baseline sample. Mean of the within-cell demeaned log assessment ratio by vigintile of the within-cell demeaned log sale price; whiskers are 95% confidence intervals (smaller than the markers). The fitted line is the sale-weighted least-squares slope. The decline is concentrated in the four lowest vigintiles.

bottom within-market price decile is assessed 65% above the local norm and therefore pays 65% more property tax than uniform assessment would imply; the overpayment falls to +7% in the second decile and crosses zero at the median, and the top decile underpays by about 5%. The mean for the bottom decile is +102%, a sign of how heavy the tail is.

Table 11 shows how much of this figure is a property of the conventions. Replacing the cell median by the revenue-neutral aggregate ratio lowers every decile by four to five points, because the aggregate ratio exceeds the median in a right-skewed distribution; the bottom decile then overpays by 59% and the top underpays by 9%. Changing the sorting variable matters far more. Sorted on the IAAO blended proxy, which averages the price and the median-deflated assessment and therefore halves the noise that sorting on price alone loads into the bottom decile, the median overpayment of the bottom decile is +5.6% and the top decile is at −0.2% (+1.7% and −4.5% under the aggregate benchmark). Sorted on the assessed value, the sign reverses: the bottom decile underpays by 5% and the top decile overpays by 6%, which is the mirror image one expects when the sorting variable carries the *assessment* error instead of the price error. The last column shows why: 53% of the sales in the bottom price decile are IAAO outliers. On the IAAO-trimmed sample, sorting on price gives +6.6% for the bottom decile and −3.2% for the top (Table 5). Taking the proxy-sorted and trimmed estimates as the most defensible, the redistribution generated by Quebec's assessment machinery is

Table 6. Piecewise-linear elasticities by within-market price quintile, baseline sample

Within-cell price quintile	Range of demeaned $\ln SP$	β	$\gamma = \beta - 1$	n
1	< -0.34	0.075*** (0.023)	-0.925	104,554
2	$[-0.34, -0.07)$	0.890 (0.097)	-0.110	104,550
3	$[-0.07, 0.12)$	0.710*** (0.024)	-0.290	104,555
4	$[0.12, 0.35)$	0.999 (0.008)	-0.001	104,552
5	≥ 0.35	0.952*** (0.008)	-0.048	104,558

Notes: Continuous piecewise-linear regression of $\ln AV$ on the within-cell demeaned $\ln SP$ with knots at the pooled quintile boundaries of the demeaned log price, 2,884 cell fixed effects absorbed; the slope in each segment is the sum of the base slope and the preceding knot coefficients. Municipality-clustered standard errors in parentheses; stars test $\beta = 1$ at 10/5/1%.

of the order of 5–7% of the tax bill against the cheapest tenth of dwellings and 3–5% in favour of the most expensive tenth—real, concentrated at the bottom, and an order of magnitude smaller than the baseline convention suggests.

6.8. The largest markets

Table 12 returns to the ten largest markets with the within-city elasticities and the trimmed statistics. On the baseline sample, Montréal's within-city Cheng elasticity is -0.18 and its Clapp estimate $+0.03$: the positive PRB of Table 3 does not mean that Montréal assessments rise more than proportionally with price in the log-log sense, but that its level-based PRB is pulled up by a stock that is 49% condominiums—the class assessed most accurately—and by a compressed ratio distribution. The other nine cities have baseline elasticities between -0.28 (Québec) and -0.51 (Sherbrooke), with Clapp estimates between -0.04 and -0.20. After IAAO trimming, all ten cities post CODs between 9.2 and 12.3, inside the ceiling for single-family property, PRDs between 1.002 and 1.022, and PRBs between -0.056 (Saguenay) and $+0.027$ (Terrebonne); their within-city elasticities range from -0.03 (Montréal) to -0.15 (Saguenay), with Clapp estimates between -0.07 and $+0.03$. The ordering is informative: the four cities with the smallest condominium shares—Sherbrooke, Saguenay, Trois-Rivières and Lévis—are the most regressive under either sample, and Montréal is the least regressive rather than an exception. Quebec's large cities meet the IAAO uniformity standard once sales are screened the way the standard prescribes; the compliance problem of Table 5 lies in the smaller municipalities.

Table 7. Heterogeneity of vertical inequity, baseline sample

Subsample	γ	Std. error	n
Property class			
Single-family	-0.454***	(0.018)	337,855
Condominium	-0.125***	(0.015)	77,976
Plex (2–5 units)	-0.487***	(0.054)	69,745
Cottage	-0.288***	(0.026)	2,114
Building age			
Age $<$ 20 y	-0.297***	(0.041)	117,898
Age 20–60 y	-0.384***	(0.034)	227,857
Age $>$ 60 y	-0.435***	(0.028)	108,049
Assessed land share			
Land share $<$ 0.2	-0.262***	(0.042)	119,718
Land share 0.2–0.4	-0.372***	(0.032)	236,923
Land share $>$ 0.4	-0.503***	(0.030)	125,714
Roll lag at sale			
Roll lag $<$ 24 m	-0.335***	(0.028)	86,001
Roll lag 24–48 m	-0.338***	(0.033)	327,031
Roll lag $>$ 48 m	-0.361***	(0.026)	91,367
Municipality size			
Muni $<$ 1k sales	-0.379***	(0.007)	125,139
Muni 1k–10k sales	-0.406***	(0.014)	185,111
Muni $>$ 10k sales	-0.267***	(0.043)	212,519
Sale year			
Sales 2021	-0.316***	(0.027)	115,221
Sales 2022	-0.380***	(0.030)	90,627
Sales 2023	-0.365***	(0.034)	74,110
Sales 2024	-0.333***	(0.032)	89,204
Sales 2025	-0.335***	(0.026)	102,026
Sales 2026	-0.349***	(0.030)	51,581

Notes: Cheng fixed-effects estimates of $\gamma = \beta - 1$ by subsample; cells with fewer than 20 sales after masking are dropped. Municipality-clustered standard errors in parentheses; the number of clusters is 527 for single-family homes, 66 for condominiums, 121 for plexes, 29 for cottages, and 540, 77 and 8 for the three municipality-size bands. *, **, *** denote significance at 10%, 5%, 1%.

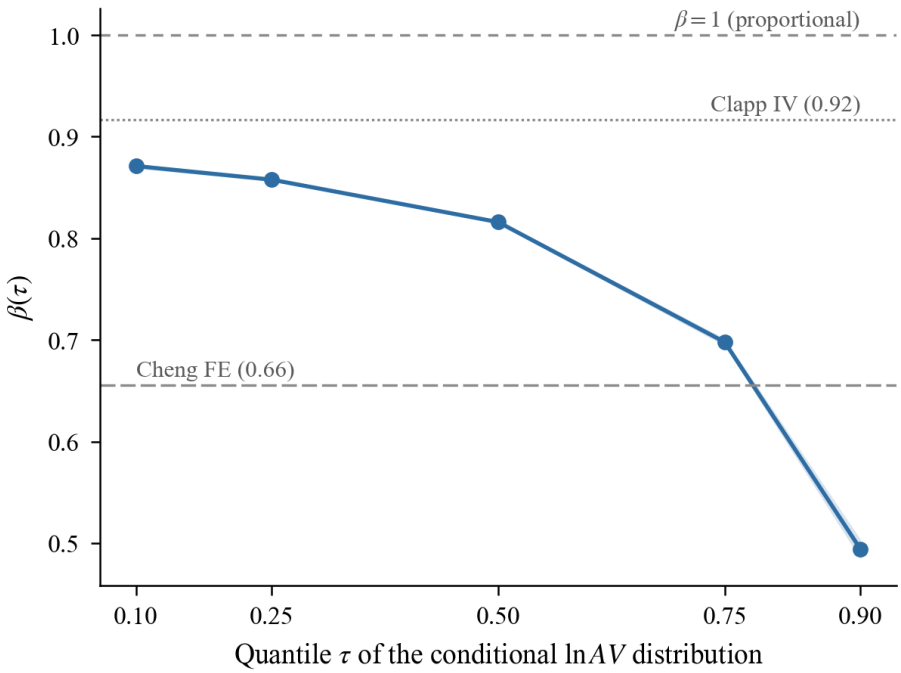


Figure 7. Quantile profile of the Cheng elasticity, baseline sample. Quantile regressions of within-cell demeaned lnAV on demeaned lnSP; the shaded band is the pointwise 95% confidence interval. The index τ is a quantile of the conditional distribution of lnAV given the price. Reference lines mark proportionality ($\beta = 1$) and the FE and IV point estimates.

Table 8. Heterogeneity after IAAO outlier trimming

	Baseline	IAAO 1.5 × IQR trimmed			
Subsample	γ_{FE}	γ_{FE}	Std. error	γ_{IV}	Clusters (munic.)
Single-family	-0.454	-0.121***	(0.009)	-0.023	527
Condominium	-0.125	-0.001	(0.015)	+0.082	66
Plex (2–5 units)	-0.487	-0.117***	(0.006)	-0.009	121
Age $<$ 20 y	-0.297	-0.038***	(0.012)	+0.019	—
Age $>$ 60 y	-0.435	-0.142***	(0.032)	-0.043	—
Land share $<$ 0.2	-0.262	-0.074***	(0.024)	+0.018	—
Land share $>$ 0.4	-0.503	-0.144***	(0.018)	-0.052	—
Muni $<$ 1k sales	-0.379	-0.142***	(0.004)	-0.039	540
Muni $>$ 10k sales	-0.267	-0.053***	(0.017)	-0.001	8
Sales 2021–2023	—	-0.111***	(0.018)	-0.033	—
Sales 2024–2026	—	-0.060***	(0.007)	+0.011	—

Notes: Cheng fixed-effects and Clapp IV estimates of γ by subsample on the sample trimmed at 1.5 interquartile ranges within cells; the first column repeats the baseline estimate of Table 7 where the subsample is identical. Municipality- clustered standard errors in parentheses; the last column gives the number of municipality clusters where it differs materially from the full sample. *, **, *** denote significance at 10%, 5%, 1%.

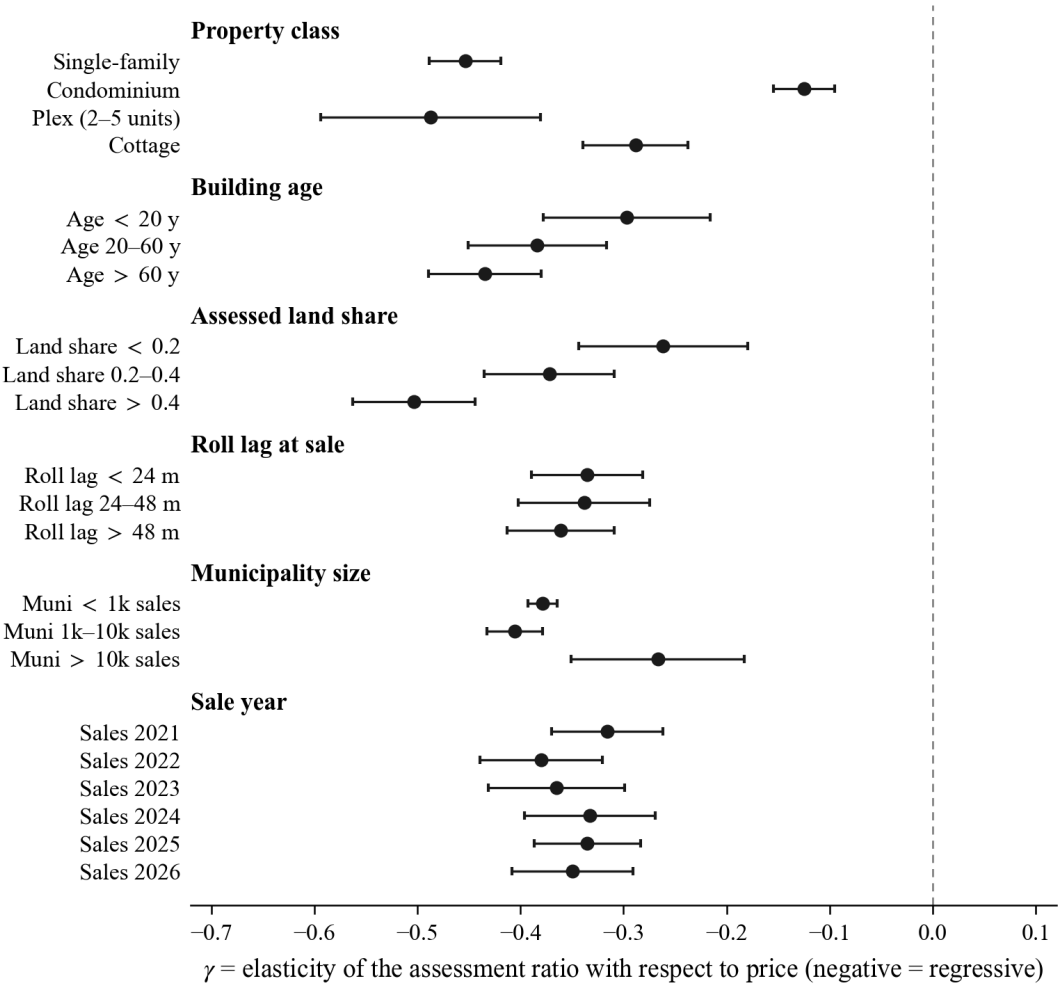


Figure 8. Heterogeneity of the within-market elasticity γ, baseline sample. Each point is a separate fixed-effects estimate of equation (4) on the indicated subsample; whiskers are 95% confidence intervals with municipality-clustered standard errors.

Table 9. Horizontal inequity: who receives noisy assessments?

	Coefficient	Std. error
Building age (decades)	+0.0075***	(0.0012)
Assessed land share	+0.0031	(0.0298)
Condominium	-0.0551***	(0.0074)
Plex (2–5 units)	+0.0247	(0.0151)
Cottage	+0.0520***	(0.0073)
Mean of dependent variable	0.188	
Observations	495,092	

Notes: Dependent variable: $|\ln r_i - \text{med}_{c(i)} \ln r|$, baseline sample. Absorbing least squares with cell fixed effects; municipality-clustered standard errors in parentheses. Omitted class: single-family. Within-cell $R^2 = 0.045$. *, **, *** denote significance at 10%, 5%, 1%.

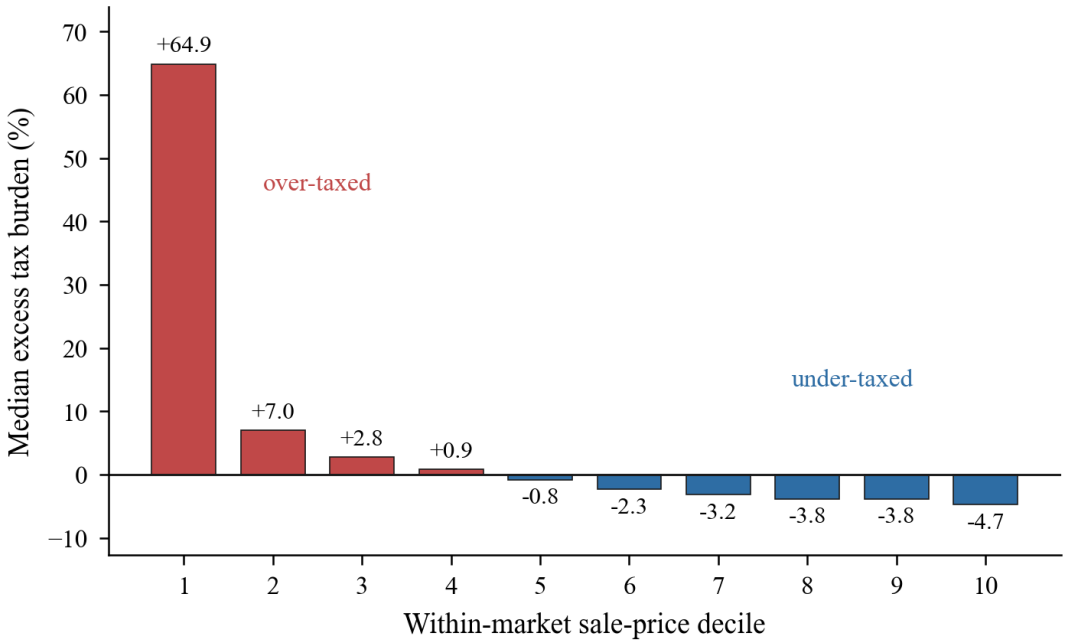


Figure 9. The implied property-tax shift, baseline sample, sales sorted on price. Median relative assessment error $e_i = r_i / \text{med}_{c(i)}(r) - 1$ by within-market sale-price decile. Because the levy is proportional to the rolled value, e_i equals the percentage over- or under-payment relative to uniform assessment at the cell median. Table 11 reports the alternative sorting variables and benchmarks.

Table 10. Excess tax burden by within-market price decile, baseline sample

Price decile	n	Mean excess burden (%)	Median excess burden (%)	Std. error
1	51,007	+101.7	+64.9	(0.51)
2	52,458	+24.2	+7.0	(0.23)
3	51,976	+11.2	+2.8	(0.16)
4	52,385	+6.3	+0.9	(0.14)
5	52,879	+3.0	-0.8	(0.11)
6	51,767	+0.8	-2.3	(0.10)
7	52,236	-0.7	-3.2	(0.09)
8	52,364	-1.9	-3.8	(0.09)
9	52,123	-2.2	-3.8	(0.08)
10	53,574	-3.6	-4.7	(0.08)

Notes: Relative assessment error by within-cell sale-price decile, cell-median benchmark. The mean is sensitive to extreme low-price sales; the median is the preferred summary. Standard errors of the mean in parentheses.

Table 11. Excess tax burden under alternative sorting variables and benchmarks, baseline sample

	Sorted on sale price		Sorted on blended proxy		Sorted on assessed value		
Decile	Median	Aggregate	Median	Aggregate	Median	Aggregate	IQR outliers (%)
1	+64.9	+59.2	+5.6	+1.7	-5.4	-9.7	53.4
2	+7.0	+2.9	+2.4	-1.7	-2.8	-7.2	16.0
3	+2.8	-1.2	+1.2	-3.0	-2.0	-6.3	7.1
4	+0.9	-3.2	-0.0	-4.3	-1.3	-5.5	4.4
5	-0.8	-5.0	-0.9	-5.1	-0.5	-4.8	3.0
6	-2.3	-6.4	-1.5	-5.7	+0.0	-4.1	2.6
7	-3.2	-7.3	-1.5	-5.7	+0.9	-3.2	2.0
8	-3.8	-7.9	-1.0	-5.3	+2.0	-2.2	1.7
9	-3.8	-7.9	-0.6	-4.8	+3.2	-0.9	1.6
10	-4.7	-8.8	-0.2	-4.5	+5.7	+1.5	1.8

Notes: Median relative assessment error (%) by within-cell decile of the sorting variable. "Median" uses the cell median ratio as benchmark; "Aggregate" uses the revenue-neutral aggregate ratio $\sum AV / \sum SP$ of the cell. The blended proxy is $\frac{1}{2}SP_i + \frac{1}{2}AV_i/\tilde{r}_c$ (International Association of Assessing Officers, 2013). The last column is the share of sales in each sale-price decile flagged as outliers by the 1.5 × IQR rule within cells.

Table 12. Within-city elasticities and trimmed diagnostics, ten largest markets

			Baseline sample		IAAO 1.5 × IQR trimmed				
Municipality	n	Condo share	γ_{FE}	γ_{IV}	COD	PRD	PRB	γ_{FE}	γ_{IV}
Montréal	79,581	0.49	-0.177	+0.034	11.8	1.008	+0.024	-0.026	+0.029
Québec	39,927	0.29	-0.283	-0.078	9.8	1.019	-0.040	-0.095	-0.057
Laval	23,569	0.24	-0.325	-0.067	9.2	1.007	-0.005	-0.050	-0.004
Gatineau	21,270	0.10	-0.290	-0.053	9.5	1.006	+0.004	-0.075	+0.001
Longueuil	15,343	0.28	-0.331	-0.036	10.3	1.006	+0.010	-0.048	+0.012
Sherbrooke	11,413	0.03	-0.510	-0.196	10.9	1.013	-0.019	-0.108	-0.042
Saguenay	10,794	0.02	-0.359	-0.132	12.3	1.022	-0.056	-0.153	-0.069
Lévis	10,622	0.07	-0.405	-0.147	9.9	1.013	-0.035	-0.104	-0.053
Trois-Rivières	9,070	0.03	-0.455	-0.163	12.2	1.014	-0.026	-0.129	-0.050
Terrebonne	8,061	0.10	-0.452	-0.109	9.4	1.002	+0.027	-0.049	+0.033

Notes: γ_{FE} and γ_{IV} are the Cheng fixed-effects and Clapp IV elasticities estimated within each city, with roll × year cells absorbed; because a single municipality offers one cluster, standard errors are heteroskedasticity-robust and lie between 0.001 and 0.011 for every entry—they understate the uncertainty attached to a city's valuation model. Trimmed statistics apply the 1.5 × IQR rule within cells; COD, PRD and PRB are medians across sale-year blocks.

7. Robustness

Table 13 and Figure 10 re-estimate the two headline elasticities—γ_{FE} and the Clapp γ_{IV}—across ten sample and measurement variants of the baseline sample; Table 14 reports the quantile coefficients underlying Figure 7; Tables 15 and 16 examine the time structure of the IAAO statistics.

Match quality. Requiring the maximum matcher score of 220 (210,820 sales) or a match distance under 10 metres changes γ_{FE} to -0.41 and -0.36 respectively, and γ_{IV} to -0.10 and -0.09: the cleanest matches show, if anything, more regressivity, so geocoding mismatch is not the source of the baseline pattern.

Extreme sales and the ratio tail. Dropping all sales under \$100,000—the segment most likely to harbour non-market transfers—moves the fixed-effects estimate to -0.30 and the Clapp estimate to -0.05. Trimming the pooled ratio distribution at the 5th and 95th percentiles cuts them to -0.15 and -0.04. These two rows anticipate the within-cell grid of Table 5: the result is robust to every restriction on prices, matches, periods and market depth, and sensitive to exactly one thing, the treatment of extreme ratios. The ordering $|\gamma_{\text{IV}}| < |\gamma_{\text{FE}}|$ holds in every variant, as the measurement-error logic requires.

Composition. Single-family homes alone give $\gamma_{\text{FE}} = -0.45$; condominiums alone -0.12, with a Clapp estimate of $+0.06$. The aggregate result is therefore not an artefact of pooling heterogeneous classes: each class is regressive or neutral on its own, and condominiums are, if anything, mildly progressive under the instrument.

Time and market depth. Splitting the window into 2021–2023 and 2024–2026 yields -0.35 and -0.34 for the fixed-effects estimate (-0.10 and -0.06 for the Clapp estimate); restricting to municipalities with at least 300 sales, or to cells with at least 50, moves the estimates by less than 0.01. The within-market elasticity is stable across the largest housing-cycle swing in recent Canadian history.

Time controls in the IAAO statistics. Table 15 verifies that the municipal CODs of Table 5 are not inflated by price movements within a sale year. Computing the statistics within municipality $\times$ quarter blocks lowers the median municipal COD from 26.0 to 23.9, and adjusting each ratio for the province-wide monthly drift of its roll vintage before computing municipality $\times$ year statistics lowers it to 25.6; the share of municipalities above 15 remains 97–99% on the baseline sample. Within-year time adjustment is therefore a second-order issue relative to the outlier rule.

Table 13. Robustness of the vertical-inequity elasticities, baseline sample

Sample variant	γ_{FE}	Std. err.	γ_{IV}	Std. err.	n
Baseline	-0.344***	(0.029)	-0.083***	(0.021)	522,769
Condominiums only	-0.125***	(0.015)	+0.058***	(0.016)	77,976
Excluding sales < $100k	-0.295***	(0.026)	-0.048***	(0.017)	507,024
Match score = 220 (max)	-0.415***	(0.040)	-0.095***	(0.029)	210,820
Match distance $\leq$ 10 m	-0.355***	(0.030)	-0.087***	(0.022)	481,476
Single-family only	-0.454***	(0.018)	-0.146***	(0.010)	337,855
Ratio trim 5/95	-0.147***	(0.022)	-0.042***	(0.016)	465,318
Sales 2021-2023	-0.350***	(0.030)	-0.101***	(0.025)	279,958
Sales 2024-2026	-0.337***	(0.029)	-0.062***	(0.016)	242,811
Munis $\geq$ 300 sales	-0.341***	(0.033)	-0.077***	(0.022)	474,004
Cells $\geq$ 50 sales	-0.340***	(0.032)	-0.078***	(0.022)	476,001

Notes: Each row re-estimates the Cheng fixed-effects and Clapp IV regressions on the indicated variant; cells with fewer than 20 sales after restriction are dropped. The 5/95 trim is applied to the pooled ratio distribution. Municipality-clustered standard errors in parentheses. *, **, *** denote significance at 10%, 5%, 1% against $\gamma = 0$.

Table 14. Quantile-regression coefficients, baseline sample

Quantile τ	0.10	0.25	0.50	0.75	0.90
$\beta(\tau)$	0.871***	0.858***	0.816***	0.698***	0.494***
	(0.0012)	(0.0008)	(0.0009)	(0.0018)	(0.0049)

Notes: Quantile regressions of within-cell demeaned $\ln AV$ on demeaned $\ln SP$, estimated on a seeded 250,000-sale subsample. Standard errors are the conventional quantile-regression errors and are not clustered. Stars test $H_0: \beta(\tau) = 1$.

Staleness: between and within municipalities. Table 16 decomposes the time pattern of the PRB. On the baseline sample the province-wide PRB swings from -0.10 in 2021–2022 to -0.00 in 2024, but the median PRB of municipality–year blocks is between -0.18 and -0.26 in every year and shows no gradient across roll-lag bands (-0.21, -0.22 and -0.19 for lags under 24, 24–48 and over 48 months). The province-wide swing is therefore compositional: pooling municipalities whose rolls are referenced on different markets mixes level differences into a vertical statistic. On the IAAO-trimmed sample the within-municipality pattern does move with the cycle, but modestly: the median block PRB is -0.044 in 2021 and -0.043 in 2022, when the boom pulled prices away from rolls anchored in 2018–2020 conditions, -0.016 in 2023, and between $+0.001$ and $+0.008$ from 2024 onward, with 82% of blocks negative in 2021 against 43–49% in 2024–2026; across lag bands it moves from -0.010 to -0.021. The within-market Cheng elasticity, for its part, is flat across lag bands (-0.34, -0.34, -0.36 on the baseline; Table 7). Staleness thus widens price-related bias at the margin in fast markets and does not generate it.

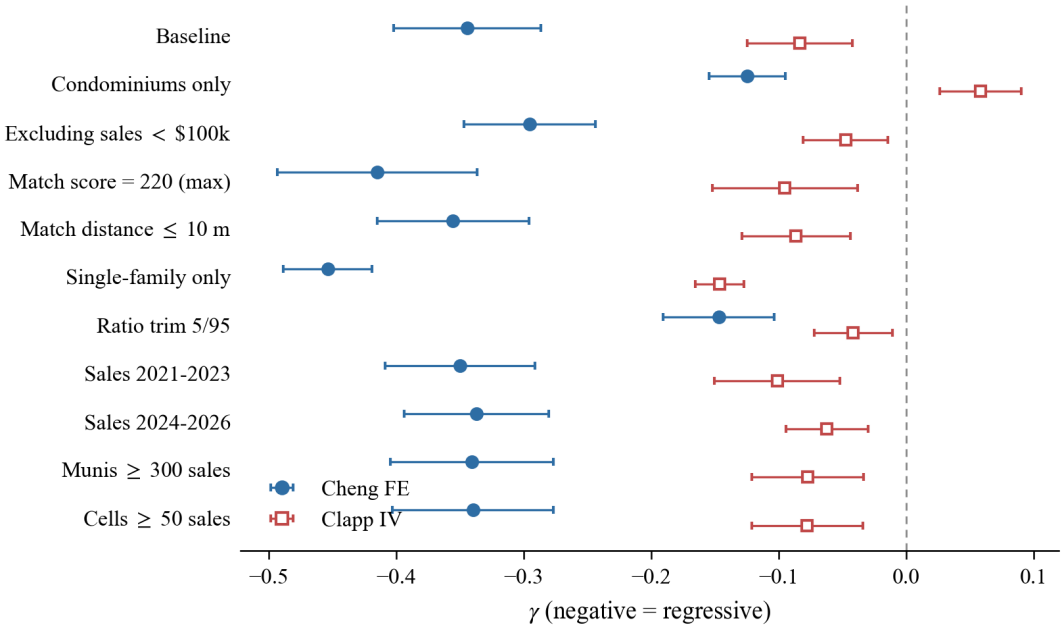


Figure 10. Stability of γ across sample variants, baseline sample. Filled circles: fixed-effects estimates; open squares: Clapp IV. Whiskers are 95% confidence intervals, municipality-clustered.

Table 15. Municipal COD and PRB under alternative time controls, baseline sample

Time-control scheme	Munis	Median COD	COD IQR	COD > 15 (%)	Median PRB	PRB < −0.05 (%)
Muni. × year blocks (baseline)	261	26.0	[22.6, 30.4]	99	-0.202	95
Muni. × quarter blocks	169	23.9	[19.7, 27.9]	97	-0.184	93
Muni. × year, time-adjusted	261	25.6	[22.3, 30.1]	98	-0.201	94
Province-wide, time-adjusted	—	28.3	—	—	-0.029	—

Notes: Municipalities with at least 100 retained sales; statistics are medians across blocks. Quarter blocks require at least 30 sales (169 municipalities qualify), year blocks at least 50. The time-adjusted ratio multiplies each ratio by the ratio of its roll vintage's annual to monthly province-wide median.

Inference. With 625 municipality clusters, the cluster-robust t-statistics on γ_{FE} exceed 6 in every variant of Table 13 and 11 in the baseline; on the IAAO-trimmed sample the fixed-effects estimate remains significant ($t \approx 7$) while the Clapp estimate does not ($t \approx 1.4$). The first stage of the Clapp instrument is strong—a coefficient of 0.51 on Z with a cluster-robust F of 2,418 and an R^2 of 0.59 (Appendix Table 17)—so weak-instrument concerns do not arise; the relevant limitation of the instrument is the one discussed in Section 5.3, not its strength.

Table 16. Price-related bias by roll lag and sale year: municipality–year blocks versus province-wide

Blocks	*n* blocks	Median ratio	Median COD	Median PRB	Share PRB < 0	Province-wide PRB
Panel A. Baseline sample (1/99 trim by roll vintage)						
Roll lag $<$ 24 m	254	0.830	25.1	-0.205	0.98	—
Roll lag 24–48 m	868	0.761	25.8	-0.216	0.95	—
Roll lag $>$ 48 m	226	0.677	26.9	-0.185	0.96	—
2021	289	0.744	25.0	-0.186	0.96	-0.104
2022	244	0.669	29.8	-0.262	0.98	-0.116
2023	195	0.708	27.4	-0.248	0.95	-0.015
2024	217	0.729	25.5	-0.181	0.95	-0.002
2025	242	0.841	24.8	-0.198	0.96	-0.039
2026	161	0.847	24.3	-0.225	0.94	-0.030
Panel B. IAAO 1.5 $\times$ IQR trimmed sample						
Roll lag $<$ 24 m	236	0.812	11.7	-0.010	0.59	—
Roll lag 24–48 m	819	0.748	12.4	-0.014	0.63	—
Roll lag $>$ 48 m	194	0.660	13.0	-0.021	0.66	—
2021	268	0.724	14.6	-0.044	0.82	-0.067
2022	222	0.654	13.8	-0.043	0.81	-0.052
2023	182	0.697	11.8	-0.016	0.63	+0.054
2024	201	0.711	11.2	+0.005	0.47	+0.067
2025	230	0.831	11.8	+0.001	0.49	+0.037
2026	146	0.834	10.1	+0.008	0.43	+0.049

Notes: Municipality $\times$ sale-year blocks with at least 50 sales, grouped by the block's median roll lag or by sale year; "Median PRB" and "Share PRB < 0" are computed across blocks. The last column is the PRB computed on the pooled province-wide sample for the year (Table 2 for Panel A).

8. Discussion

8.1. Mechanisms

The evidence assembles into a mechanism story with four layers, the first of which conditions the others.

1. The lower tail of prices. The single most important determinant of measured regressivity in Quebec is the set of sales whose declared price lies far below the rolled value. Fewer than one sale in ten belongs to it, but it supplies most of the baseline elasticity, most of the COD, and almost all of the bottom-decile tax shift. Two interpretations coexist. Some of these sales are market transactions of dwellings the roll overvalues badly—derelict structures, properties with latent defects, lots whose

highest and best use has changed—and for them the baseline is the right measure and the IAAO rule discards genuine inequity. Others are transfers recorded at a consideration unrelated to value, which no Quebec registry flags. The piecewise profile and the decile distribution of outliers point to the second interpretation for the majority, but not the totality, of the tail, and the honest statement is that Quebec's assessment regressivity lies between a lenient-trim upper bound and an IAAO-trimmed lower bound. The attenuation critique of Kochin and Parks (1982) and the practitioners' trimming rules address the same observations: the Clapp instrument discounts them econometrically, the interquartile rule removes them. That the two approaches agree qualitatively—both bring the elasticity most of the way to zero—is reassuring; that neither can tell a mis-assessed dwelling from a nominal transfer is the binding limitation of registry-based ratio studies, in Quebec and elsewhere.

2. Hedonic difficulty, not assessor idiosyncrasy. Whatever the trimming rule, the heterogeneity profile tracks the difficulty of the valuation problem. Condominiums (homogeneous, comparable-rich) are assessed proportionally; old houses, plexes, cottages and high-land-share properties (heterogeneous, comparable-poor, unanchored by construction costs) are regressive. This is the pattern predicted if assessors run reasonable hedonic models whose unexplained component is largest—and most value-correlated—where the housing bundle is hardest to price (Amornsiripanitch, 2022; Gloudemans and Almy, 2011; Bostic et al., 2007). Regression toward the mean in any imperfect valuation model over-values the cheap tail and under-values the expensive tail; in Quebec the second half of that prediction is nearly absent, since the top quintile's elasticity is 0.95, and the first half is confounded with the registry problem just described.

3. Staleness amplifies at the margin. The province-wide PRB swings by a factor of fifty between 2022 and 2024, but the swing is mostly composition: municipalities whose rolls were referenced on different markets have different ratio levels, and pooling them loads level differences into a vertical statistic. Within municipalities, the trimmed PRB moves from -0.04 in the boom years to zero afterwards and the elasticity does not move at all. Triennial rolls thus interact with fast markets—when prices move 30–40% between reference dates, the cross-sectional dispersion of appreciation, which is correlated with price segment, shows up in the ratio distribution—but the effect is a few hundredths of the PRB, not a doubling of inequity. Jurisdictions cannot control housing cycles, but they control revaluation frequency, and annual-cycle systems cap this channel (Berry, 2021; Ross, 2012).

4. Scale and composition in the large cities. Montréal is not the exception its positive PRB suggests. Its within-city elasticity is negative on the baseline sample and near zero after trimming, like Laval's, Longueuil's, Gatineau's and Terrebonne's; the four large cities with almost no

condominiums—Sherbrooke, Saguenay, Trois-Rivières and Lévis—are the most regressive under either sample. The PRB is a level-weighted statistic whose sign depends on the composition of the stock, which is one reason the IAAO pairs it with the PRD and why McMillen and Singh (2023) caution against reading any single vertical measure in isolation. Once sales are screened as the standard prescribes, all ten large cities meet the uniformity ceiling; the municipalities that do not are small, and the thin-market mechanism of McMillen and Weber (2008) operates within a single provincial rulebook—identical statutes, divergent outcomes, sorted by market depth and assessment resources. We note that a city's cell is the whole city, pooling boroughs and neighbourhoods, and that borough-level analysis is a natural extension.

8.2. Policy implications

Four levers follow from the diagnosis. First, **validate sales**: the largest source of uncertainty in this audit is the absence of an arm's-length flag in Quebec's transaction records. A declaration of transfer type at registration, as several U.S. states and Ontario's assessment corporation require before sales enter a ratio study, would let the province and its municipalities distinguish over-assessed dwellings from nominal transfers and would sharpen every statistic in this paper. Second, **audit vertically, not just by level**: article 264 of the LFM already produces an annual median proportion for every roll; adding the COD and the PRB, computed on validated and trimmed sales under a promulgated uniform procedure (International Association of Assessing Officers, 2013, section 5.2.2), and publishing them by municipality, would make the equity dimension visible and contestable at low cost. Third, **pool assessment capacity**: the compliance gap after trimming is a small-municipality phenomenon, which argues for regional or provincial mass-appraisal services for the municipalities that cannot sustain a modelling team. Fourth, **shorten or index the cycle**: the staleness channel is modest but real, and indexing rolled values between triennial revaluations, as British Columbia's annual cycle effectively does, would remove it. Because owners in the lowest price segment appeal least (Avenancio-León and Howard, 2022; Plummer, 2014), supply-side correction—better data and better models—will do more than demand-side remedies.

8.3. Limitations

Five limitations bound the interpretation. First, and most important, we cannot observe whether a sale was at arm's length; the bounds we report are the honest consequence, and the bottom-decile tax-shift magnitude on the baseline sample should be read as an upper bound. Second, sale prices below $50,000 are absent from the source registry, which truncates the very segment where the registry problem is worst. Third, our data cover 2021–2026, an unusually turbulent market; the within-market elasticity is stable across the window, but level statistics from calmer periods would

differ. Fourth, we observe no appeals, renovations between the reference date and the sale, or conditions of sale; some of what we call horizontal noise is genuine unobserved quality change, and the assessment at the time of sale may differ from the assessment at the time the tax was levied for properties that were reviewed. Fifth, without owner demographics we cannot speak to the distributional incidence across income or racial groups as Avenancio-León and Howard (2022) do, and our tax-shift figures are statutory rather than economic incidence; linking the matched file to census characteristics and to the province's phase-in resolutions is the obvious next step.

9. Conclusion

Matching more than half a million residential sales to the assessment rolls that tax them, this paper delivers the first province-wide ratio study of assessment equity for a Canadian province. The answer to the question it set out to ask—is Quebec's property-tax base regressive?—turns out to depend on a choice that ratio-study practice regulates and that the academic literature rarely reports: how to treat the sales whose declared price lies far below the rolled value. Under a lenient trim, Quebec looks like the regressive U.S. jurisdictions of the recent literature: a within-market elasticity of the assessment ratio of -0.34, a COD near 30, and 95% of municipalities outside the IAAO band for price-related bias. Under the outlier rule the IAAO standard prescribes, which removes 9% of sales concentrated in the bottom price decile, Quebec looks like a system that works: an elasticity of -0.09 that a measurement-error-robust instrument brings to zero, a PRB inside the acceptable band, CODs between 9 and 12 in the ten largest cities, and three quarters of municipalities meeting the vertical-equity standard. Proportionality holds across the upper 80% of every market's price distribution under either treatment; the two readings differ only about the bottom quintile, and the registry does not record what would settle the difference.

Within those bounds, three results are robust. The ordering of difficulty is invariant to the trimming rule: condominiums are assessed proportionally, while single-family homes, plexes, older and land-heavy properties and small municipalities are regressive, at a quarter of the lenient-trim magnitude once outliers are removed. Staleness widens price-related bias at the margin in fast markets—the within-municipality PRB of the trimmed sample moves from -0.04 in 2021–2022 to zero from 2024—but does not generate inequity, and the province-wide swing that suggested otherwise is compositional. And the redistribution implied by the most defensible conventions is real but modest: the cheapest tenth of dwellings in a market pays about 5–7% more property tax than uniform assessment would imply, the most expensive tenth 3–5% less.

These findings qualify the presumption, in both directions, about centralised assessment systems. Quebec's uniform statutes, chartered appraisers and statutory market-vintage synchronisation do not immunise it against the pathologies of the fragmented U.S. system, but neither do they reproduce

them at U.S. magnitude once sales are screened as the profession prescribes. The binding constraint the audit reveals is informational: the province measures assessment *level* every year and publishes nothing on uniformity or vertical equity, and its transaction records carry no indication of whether a sale was at arm's length. Both are inexpensive to fix relative to the redistribution at stake, and fixing the second would turn the bounds of this paper into a point.

The matched sale–roll architecture built here supports a research agenda beyond this audit: linking assessment gaps to neighbourhood socio-demographics, exploiting the discrete arrival of new rolls to study price anchoring, using the land–building decomposition to measure land-value dynamics at the parcel level, and, once validated sale flags become available, revisiting the bottom quintile that this paper could only bracket.

A. Measurement-error bounds and first stage

This appendix derives the bounds used in Section 5.3 and reports the moments behind them. Write the within-cell demeaned variables as in equation (5), $\ln SP_i = \ln V_i + u_i$ and $\ln AV_i = \beta \ln V_i + \varepsilon_i$, with u_i, ε_i and $\ln V_i$ mutually uncorrelated and σ_V^2, σ_u^2, σ_ε^2 their variances. The observable within-cell moments are

$$\mathrm{Var}(\ln SP) = \sigma_V^2 + \sigma_u^2, \qquad \mathrm{Var}(\ln AV) = \beta^2 \sigma_V^2 + \sigma_\varepsilon^2, \qquad \mathrm{Cov}(\ln AV, \ln SP) = \beta\, \sigma_V^2. \tag{10}$$

The forward slope is $\hat{\beta}_F = \mathrm{Cov} \,/\, \mathrm{Var}(\ln SP) \to \beta\, \sigma_V^2/(\sigma_V^2 + \sigma_u^2) \le \beta$, and the inverse of the reverse slope is $1/\hat{\beta}_R = \mathrm{Var}(\ln AV)/\mathrm{Cov} \to \beta + \sigma_\varepsilon^2/(\beta \sigma_V^2) \ge \beta$, which gives equation (7). Given $\hat{\beta}_F$, the noise share $\lambda = \sigma_u^2/\mathrm{Var}(\ln SP)$ that would rationalise a true elasticity β^* is $\lambda = 1 - \hat{\beta}_F/\beta^*$, and the implied standard deviation of transaction noise is $\sqrt{\lambda\, \mathrm{Var}(\ln SP)}$. Table 17 reports the moments, the bounds, the noise shares implied by $\beta^* = 1$ and by the Clapp estimate, and the first stage of the Clapp instrument. Three features deserve comment. The bracket $[0.656, 1.137]$ contains one, so the classical errors-in-variables model alone cannot establish regressivity on the baseline sample; this is a property of the large assessment-error variance (the within-cell correlation is 0.76), not of the sample size. Rationalising proportionality requires a transaction-noise standard deviation of 0.28 log points, and the Clapp estimate itself implies 0.25; both exceed the out-of-sample dispersion of hedonic predictions around prices on comparable Quebec data, which suggests that part of the "noise" the instrument discounts is non-market sales rather than bargaining dispersion. The first stage is strong by any standard, so the instrument's limitation is the dependence of Z on the assessment rank discussed in Section 5.3, not weak identification.

Table 17. Within-cell moments, measurement-error bounds and Clapp first stage, baseline sample

Quantity	Value
Within-cell variance of $\ln SP$	0.224
Within-cell variance of $\ln AV$	0.167
Within-cell covariance	0.147
Within-cell correlation	0.759
Forward slope $\hat{\beta}_F$ (Cheng FE)	0.656
Inverse reverse slope $1/\hat{\beta}_R$ (Kochin–Parks)	1.137
Noise share λ rationalising $\beta = 1$	0.344
Implied transaction-noise s.d. for $\beta = 1$ (log points)	0.277
Clapp IV $\hat{\beta}_{IV}$	0.917
Noise share λ implied by $\hat{\beta}_{IV}$	0.285
Implied transaction-noise s.d. under $\hat{\beta}_{IV}$ (log points)	0.252
First stage: coefficient on Z (s.e.)	0.509 (0.010)
First stage: cluster-robust F	2,418
First stage: R^2	0.587
Share of sales with $Z = -1$ / $Z = +1$	0.244 / 0.263

Notes: All moments are computed on variables demeaned within the 2,884 municipality $\times$ roll $\times$ sale-year cells. The first-stage regression is of the demeaned $\ln SP$ on $Z \in \{-1, 0, +1\}$ with municipality-clustered standard errors; F is the square of the cluster-robust t-statistic.

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