

Reassessing the Property Tax

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This draft: January 28, 2021

Abstract. The property tax is the single largest source of revenue for American local governments. It is designed to be an *ad valorem* tax. The fairness and accuracy of the tax hinges on the quality of property valuation by local assessors. Using data from millions of residential real estate transactions, this paper shows that assessments are typically regressive, with low-priced properties being assessed at a higher value, relative to their actual sale price, than are high-priced properties. As a result, the property tax disproportionately burdens owners of less valuable homes. Such regressivity is evident throughout the US. This result cannot be explained by measurement error in sale prices, or by explicit policy choices, such as assessment limits. Rather, regressivity appears to arise from limitations in the data and models used by assessors.

I am grateful for helpful comments from Bernadette Atuahene, Dan Black, Anthony Fowler, Joshua Gottlieb, Damon Jones, and Vincent Pons. Eric Langowski, Nicholas Norton, Jonathan Rockower, Maxwell Schmidt, and Xiaoyan Wang provided outstanding research assistance.

The property tax is the single largest source of revenue for American local governments. Cities, counties, school districts, and special districts raise roughly \$500 billion per year in property taxes, accounting for 72% of local taxes and 47% of local own-source general revenue nationwide.¹ Whether residents rent or own, property taxes directly or indirectly impact almost everyone.

In many communities, however, property taxes are inequitable: low-priced properties are assessed at a higher value, relative to their actual sale price, than are high-priced properties. Because a property's assessed value is the basis for determining its tax bill, inequities in assessments translate into inequities in taxation.

While property tax assessment has not been a subject of great interest in the academic literature on public finance, the issue of assessment regressivity has begun to attract popular attention in cities throughout the country. Figure 1 shows patterns of regressive assessment and taxation in four major cities that have recently been sites of activism around property tax inequities.

The accuracy of assessment can be evaluated at the time a property sells by comparing its sale price with the assessed value in place at the time of sale. Jurisdictions vary in their legally required *levels of assessment*, meaning the fraction of a property's market value that is put on the assessment roll for purposes of computing the tax levy (IAAO 2013). For example, in Chicago, assessed value is defined to be 10% of market value, meaning that a home worth \$250,000 should have an assessed value of \$25,000. In New York City the assessment level is 6%, and in Detroit it is 50%. In New Orleans, and many other jurisdictions, it is 100%.² Importantly, the required assessment level is the same for every property of the same class (e.g., residential) within a jurisdiction. In principle, the ratio of the sale price to the assessed value, also known as the *sales ratio*, should equal the assessment level, at least in expectation.

In Figure 1, the left column shows binned scatter plots of sales ratios against sale prices for homes that have sold since 2015 in each city. The right column shows binned scatter plots of the effective tax rate, defined as the property tax due in the year of sale divided by the sale price of the property. Data were obtained from Corelogic, a private data vendor (details below). The data are divided into deciles of sale price and each dot represents the average sale price and average sales ratio (left column) or average effective tax rate (right column) in the decile. In every case, lower priced properties are, on average, assessed at a higher rate, relative to their

¹ U.S. Census Bureau, 2016 Annual Surveys of State and Local Government Finances. <https://www.census.gov/data/datasets/2016/econ/local/public-use-datasets.html>.

² The assessment level is a legal artifact with no direct effect on property taxation, since a lower assessment level can be offset by a higher tax rate.

sale price, than are higher priced properties, and as a result lower-priced properties are subject to higher effective tax rates on average.

Each city has a story to tell. In Chicago, powerful local assessor and chair of the Cook County Democratic party, Joseph Berrios, was unseated in a high-profile election after the *Chicago Tribune* published a series of stories documenting systematic over-assessment of low valued-properties, predominantly on the city's south and west sides, and under-assessment of properties on the city's affluent north side (Grotto 2017a). The political fallout from assessment regressivity was a long time in coming, as a series of analyses had documented the problem as far back as the 1970s (B. J. L. Berry and Bednarz 1975; McMillen 2011). Assessment regressivity was seen as resulting, at least in part, from the cozy relationship between the assessor and property tax lawyers, including long-serving Illinois House Speaker Michael Madigan and recently indicted city councilor Ed Burke, each of whom operates a prominent law firm specializing in property tax appeals (Novak 2020). Berrios remains under federal investigation in a case that has also ensnared Madigan, Burke, and Illinois governor J.B. Pritzker (Dardick and Lighty 2020).

In New Orleans, the local assessor has been accused of failing to regularly assess properties. A report by the Louisiana Legislative Auditor (2020) found that the Orleans Parish assessor failed to assess 18% of properties that were due to be reassessed for fiscal year 2020, and more than a quarter of the city's properties had not been reassessed in the last four years. Because property values were rising rapidly during this time, the failure to reassess constituted an effective tax cut. Local journalists and advocacy groups have argued that the unassessed properties were disproportionately in affluent neighborhoods (Adelson 2019; Together New Orleans 2019).

Property values in Detroit cratered during the Great Recession and assessed values failed to keep pace with that decline, resulting in widespread over-assessment of property (e.g., MacDonald 2020). Because property values fell more sharply in poorer neighborhoods, however, low-priced properties became relatively more overassessed. The results were staggering: the average home that sold in Detroit in 2010 was assessed at 11 times its sale price. In the bottom quintile of sale price, the average home sold for \$1700 but was assessed at \$41,000, or 30 times its price; in the top quintile the average home was assessed at twice its sale price (Hodge et al. 2017). This results in eye-popping tax rates for some homes, even if the tax bills are not especially large in absolute terms. Per Figure 1, the average home in the bottom decile saw a tax bill of about \$600, making the effective tax rate of 37 percent of sale price per year. A citywide reassessment to address these problems was initiated in 2014 and implemented in 2017. While the extent of

over-assessment has been improved, assessments continue to be regressive (C. R. Berry 2020; Center for Municipal Finance 2020). Concerns about assessment regressivity in Detroit are compounded by the fact that more than 100,000 homes, fully one-quarter of the city's residential properties, have been foreclosed on for failure to pay property taxes (Berry and Atuahene 2019).

In New York, statutory caps on annual assessment increases have prevented assessed values from keeping up with sale prices in rapidly appreciating neighborhoods. For example, assessments on 1-3 family homes ("Class 1" properties in NYC assessing parlance) may not be increased by more than 6 percent in a single year or 20 percent over 5 years.³ The NYC Advisory Commission on Property Tax Reform (2020) found that assessment caps led to large disparities in effective tax rates between fast-growing and slow-growing areas of the city. For instance, median assessment ratios for class 1 properties in booming Manhattan (2.1%) and Brooklyn (3.4%) are far lower than in Staten Island (5.2%), Queens (4.4%), and the Bronx (5%), where prices have been rising more slowly (New York City Advisory Commission on Property Tax Reform 2020, p. 48). My analysis in Figure 1 shows the general pattern of assessment regressivity for class 1 properties in New York City. Differential principles of taxation applied to Class 1 (1-3 family homes) and Class 2 (condominiums and cooperatives) properties may lead to further inequities (New York City Advisory Commission on Property Tax Reform 2020) not reflected in Figure 1. A controversial effort in the state legislature to reform the property tax system has been at least temporarily derailed by the coronavirus pandemic (Fitzsimmons, Haag, and Mays 2020; Geringer-Sameth 2020).

While the story of each of these cities has unique features, property tax regressivity is common to all of them. Indeed, the similarity in the patterns across these cities raises the question of whether assessment regressivity is a general phenomenon rather than the product of one city's particular actors and institutions. This paper answers that question in the affirmative. The high-profile cases shown in Figure 1 are only the tip of the iceberg. Analyzing data from millions of residential real estate transactions nationwide, I find assessment regressivity is pervasive and results in regressive property taxation.

The paper proceeds as follows. Section I provides an overview of the potential sources of assessment regressivity. Section II reviews related literature and explains methodological issues involved in measuring assessment regressivity. Section III presents a national analysis of assessment ratios relative to sale prices for a sample of 26 million residential sales from 2006 to 2017. Section IV asks

³ The caps apply to increases in assessed values due to changing market conditions. Increases in assessed value based on physical alterations to the structure are not subject to the same limits. Growth caps also apply to some Class 2 properties, with yearly increases limited to 8 percent annually or 30 percent over five years.

whether measurement error could account for the negative association between assessed values and sale prices, concluding that it cannot. Section V evaluates the distribution of regressivity across counties. Section VI provides some empirical evidence on the relative importance of the sources of regressivity reviewed in Section I.

I. Potential Sources of Assessment Regressivity

If assessment regressivity is not the result of idiosyncratic local factors, what causes? The potential causes can be divided into two broad categories: flaws in the assessment process; and policy choices.

The first source of regressivity derives from data and modeling limitations. Most statistical models used in assessment are based on some form of conditional averaging; that is, the assessed value for a particular property is based on the average value of other properties with the same observable characteristics.⁴ Depending on the jurisdiction, such conditional averaging may be implemented through a regression model or a comparables-based method (Gloudemans and Almy 2011; Officers 2018a). In either case, a property whose value is below average relative to its observable characteristics will be over-assessed, while a property whose price is high relative to its observable features will be under-assessed.

As an extreme example that conveys the intuition of the problem, suppose the assessor has no information about the attributes of properties that have not sold recently. The best the assessor could do (in terms of expected mean squared error) is to value each unsold property at the mean price of all sold properties. This would necessarily lead to below-average properties being over-assessed and above-average properties being under-assessed.

Conditional averaging would not be a major source of inequity if assessors had access to data that explained property prices sufficiently well. In practice, however, assessors usually have access only to a limited set of property characteristics, typically including features such as the age of a property, its square footage, and the number of bedrooms and bathrooms (e.g., Gloudemans and Almy 2011, ch. 8). Assessors often have little information about a property's maintenance condition and almost never have information about many important internal features of a property, such as a chef's kitchen or a spa-like bathroom.⁵ In other

⁴ Assessors are generally using regression or nearest-neighbors matching of some kind (Gloudemans and Almy 2011; Officers 2018a).

⁵ Assessors are constrained by the Fourth Amendment prohibition on unreasonable searches and seizures. A 2005 sixth circuit court of appeals case, *Widgren v. Maple Grove Twp.*, 429 F.3d 575 (6th Cir. 2005), held that an assessor who did an exterior inspection, and did not look into the home, did

words, a large and potentially consequential set of property attributes is observable to buyers and sellers but unobservable to the assessor. As a result, two properties with the same observable-to-the-assessor features may sell for considerably different prices, which will lead to regressivity in assessments.

In addition to data limitations, modeling issues may lead to assessment regressivity. Of particular note, assessors must appropriately capture local variation in housing markets, which is often done by controlling for neighborhood attributes or including a set neighborhood of indicator variables. However, the “neighborhoods” used for assessment purposes are often defined arbitrarily or were created for other purposes. For instance, Cook County is divided into 29 “townships” for assessment purposes, while Detroit defines approximately 200 neighborhoods based on “economic condition factors”, most of which have considerable internal heterogeneity. Again, to the extent that assessment neighborhoods include internal heterogeneity of sale prices, valuing homes at the neighborhood average can lead to below (above) average homes being over (under) assessed.

Similarly, assessors must account for time trends, especially as current assessments are based on sales from prior years and the interval between reassessments can be two to four years. Assessors often model time trends according to a single yearly or quarterly adjustment for the entire jurisdiction. To the extent that some neighborhoods appreciate slower or faster than the jurisdictional average, this can be a source of inaccuracy in assessments, whose effects on regressivity are ambiguous. If low-priced properties appreciate faster than average, this can lead to progressivity in assessments, but when low-priced neighborhoods are hit harder by housing market declines, as appears to have been the case in the Great Recession (Cohen, Coughlin, and Lopez 2012), assessment regressivity may increase if assessors are applying a single temporal adjustment to the entire jurisdiction.

Aside from data and modeling issues, the level of assessment regressivity in a jurisdiction is also shaped by explicit policies. Most notably, 18 states impose caps on annual increases in the taxable value of a property, generally limiting increases to no more than 3 to 6 percent per year (Dornfest, Ireland, and Southard 2020; Haveman and Sexton 2008). Such policies are intended to protect homeowners from unpredictable increases in property taxes when housing markets are booming. With these caps in place, even if assessors correctly estimate a property’s market value, they would be unable to set the assessed value equal to market value if doing so would exceed the increase limit. When low-valued and high-valued homes

not run afoul of the Fourth Amendment. The court warned that the assessor must get permission to conduct an interior inspection.

appreciate at different rates, assessment caps can lead to assessment regressivity or progressivity. In New York City, for example, assessment caps have been shown to disproportionately benefit owners of high-priced properties, increasing overall assessment regressivity (Hayashi 2014; New York City Advisory Commission on Property Tax Reform 2020).

The practice of property classification, in which different categories of residential property (e.g., single-family, condominiums) are assessed at different rates, can also influence assessment regressivity. As well, various exemptions and abatements are generally meant to make property taxes more progressive, although, because of uneven take-up, the extent to which they do so is a matter of debate (Ihlanfeldt 2013; Moore 2008). Similarly, appeals offer residents a chance to protest excessive assessments, but because appeals are disproportionately brought by owners of high-priced property, they may exacerbate rather than remedy regressivity (Ross 2017).

Altogether, there are good reasons to be concerned that assessment regressivity is not the result of idiosyncratic local factors but rather due to systematic flaws in assessment practice and to policy choices.

II. Related Literature

Given the robust theoretical and empirical academic literature on property taxation, issues of property assessment, and of assessment regressivity in particular, have received relatively little attention from scholars of public finance. There have been numerous studies evaluating property assessment in particular cities (for recent examples see, e.g., Hodge et al. 2017; McMillen and Singh 2020; PlaHovinsak and Vicentini n.d.). Recently, Avenancio-Leon and Howard (2020) studied over-assessment of a particular group of home-owners, finding that assessment ratios are higher for African Americans (also see Harris 2004). However, there has not been a systematic national study of assessment regressivity. Meanwhile, most textbook treatments of property taxation in public finance or urban economics make at most passing reference to assessment, evaluating the efficiency and fairness of the property tax under the assumption that property values are known (O'Sullivan 2003 ch. 20; e.g., Rosen 2002 ch. 20). Even a specialized book-length treatment such as Youngman (2016) makes little mention of assessment issues. Rather, most discussions of the incidence of the property tax focus on questions such as how much of the tax is passed from owners to renters and whether the property tax is a benefits tax or a tax on capital (see Oates and Fischel 2016; Zodrow 2001).

Within the professional assessment community, regressivity has received more attention, with the leading professional association, the International Association of Assessing Officers (IAAO), providing standards for measuring regressivity, which they call *vertical equity* (IAAO 2013; Officers 2018a). The IAAO recommends that local assessors study and report on regressivity, along with other aspects of assessment quality, whenever there is a reassessment (IAAO 2013). Many state Departments of Revenue regularly compile assessment statistics from local jurisdictions, often including measures of regressivity (e.g., Center for Municipal Finance 2018).

Much of the literature on assessment regressivity has been devoted to issues of methodology, with many papers proposing and comparing alternative approaches to quantifying regressivity. The literature arguably begins with Paglin and Fogarty (1972), who argued that a regression of assessed value on sale price can reveal regressivity, while subsequent papers have proposed related approaches. Sirmans, Gatzlaff, and MacPherson (2008a) and Carter (2016) provide helpful reviews of the associated literature.

A central methodological concern in this literature is that classical measurement error in sale prices may lead to a false impression of regressivity when sale price is on the right-hand side of a regression (see Kennedy 1984; PlaHovinsak and Vicentini n.d.). The concern is that, given inevitable random factors in the sale of any individual property, the final price is only a noisy measure of its market value. To see the problem, consider two hypothetical neighboring homes that are identical, each with a true market value of \$100,000. If both homes went up for sale at the same time, one might fetch a price of \$105,000, say if the seller is a particularly savvy negotiator, while the other home might garner only \$95,000, say if the buyer is a particularly savvy negotiator. If the assessor appropriately valued both homes at \$100,000, a comparison of sales ratios would indicate regressivity (the higher-priced home is seemingly under-assessed and the lower-priced home is seemingly over-assessed). The same problem extends to a regression of sales ratios against sale prices, where classical measurement error will bias the coefficient downward, indicating regressivity. Reversing the regression--to put the sale price on the left-hand side and assessed value on the right--will not solve the problem because assessed values are also noisy measures of market value, and in that case such a regression would be biased toward showing progressivity (Kennedy 1984).

A number of ad hoc responses to these concerns have been proposed (Clapp 1990; Gloudemans 2011), but the field has not arrived at a generally agreed upon solution. The possibility that metrics of regressivity may be biased due to

measurement error has left many practitioners in a quandary, uncertain whether such estimates are to be trusted (Carter 2016).

My analyses in Section III will come from regressions of assessment ratios against sale prices and, thus, be subject to concerns about bias due to measurement error. In Section IV, I present several auxiliary analyses to show that the resulting findings of regressivity are not the product of measurement error. However, even before showing those auxiliary analyses, a casual inspection of the data from the four cities represented in Figure 1 provides reasons to doubt that measurement error could be the culprit. In Detroit, for example, the average home in the bottom decile of sale prices was assessed at over 3 times its value, and prior research shows that the ratio was even higher in past years (Hodge et al. 2017). It strains credulity to suggest that the average market value of those properties was such a large multiple of their sale price, with the difference due to random factors. Even in the less severe case of Chicago, the gap between assessed values and sale prices seems to be too large to be the result of noise. For instance, between 2007 and 2016, there were 1,015 properties in Chicago that sold for exactly \$100,000. The average assessed value of those properties was \$151,585. Meanwhile, there were 149 properties that sold for exactly \$1 million. The average assessed value of those properties was \$647,030. It is hard to believe that average discrepancies of such a magnitude are caused by random factors in the individual transactions. And, as we will see, discrepancies of such magnitude are typical.

III. Assessment Regressivity Nationwide

The analyses that follow are based on the tax and deed database from Corelogic, a private data vendor that collects records from local assessors and recorders of deeds. The dataset provides information on individual property sales, including addresses, sale prices, and assessed values. I use data from 2006 to 2016, a period during which Corelogic has widespread coverage, including roughly 2,600 counties accounting for 99 percent of the US population.

I restrict my analysis to residential properties, which include single-family homes, duplexes, and condominiums.⁶ I include only transactions classified as arm's length by Corelogic, which excludes, for example, sales between related parties, sales resulting from a divorce settlement, and sales of foreclosed properties. When a property sells more than once in the same year, I exclude all

⁶ In most jurisdictions, apartment buildings are classified as commercial properties.

observations for that property in that year, as it is not uncommon for the same transaction to be re-reported if there is an error in the first recording document.

I exclude California, which uses acquisition-based assessment, in which properties are reassessed at the time of sale rather than at regular intervals (Sexton, Sheffrin, and O'Sullivan 1999). While there may be other sorts of inequities involved in such a system—the owner of a newly purchased home will pay more than the owner who purchased an identical home long ago (Sexton, Sheffrin, and O'Sullivan 1999)—the nature of the system is sufficiently different from most of the rest of the country that California should be considered separately.

A concern in the professional assessment literature is that analyses of sales ratios may be sensitive to influential outliers created by data entry errors or sales between related parties that are erroneously classified as arm's length (IAAO 2013). While Corelogic implements quality control procedures to prevent such issues, I additionally trim the data to exclude the highest and lowest 2% of sales ratios in each jurisdiction-year. However, I emphasize that this restriction is not driving my results. If I include those trimmed observations, evidence of regressivity is even stronger (see Appendix).

After these exclusions, I have a sample of 26 million residential transactions from 2006 to 2016, the most recent complete year of data available at the time of this writing. Over 2,600 counties are represented in the Corelogic data, although not every county is present in every year. For each property, I compute the sales ratio as the assessed value in place on January 1 of the sale year divided by the sale price. Summary statistics are provided in Table 1. The average assessment ratio is 65%, reflecting the fact that many jurisdictions have legally required assessment levels of less than 100%. Interestingly, Table 1 shows that even as average sale prices fell from 2007 through 2009, average assessed values rose, as did effective tax rates. This is one indication that assessments were slow to catch up with market changes during the recession, a theme to which I will return below.

Unequal assessment ratios are only a concern among properties within the same taxing jurisdiction. If Detroit assesses properties at a higher ratio of their sale price than New York does, it is not cause for concern. However, when different properties of the same type within the same taxing jurisdiction are assessed at different sales ratios, this will result in differences in effective tax rates that are inconsistent with ad valorem taxation and violate basic principles of fairness in assessment. In the analyses that follow, it is therefore important to restrict comparisons to properties within a taxing jurisdiction.

An important feature of American local government is that most properties are situated within multiple overlapping tax jurisdictions (C. R. Berry 2009). I matched each property to its overlapping county, incorporated city (if applicable),

and school district. These three layers of government account for 89% of property taxes collected by local governments in the United States (Annual Survey of State and Local Government Finances 2017). I present within-jurisdiction analyses for each type of government, as well as within overlapping combinations of the three types of governments. There are 2,653 counties, 16,630 incorporated places (cities), and 9,613 school districts represented in the Corelogic data, and 31,992 unique overlapping combinations of those jurisdictions.

My main estimating equation is as follows:

$$(1) \ln(A_{ijt}/P_{ijt}) = \alpha_{jt} + \beta(\ln P_{ijt}) + \varepsilon_{ijt}$$

where j denotes jurisdiction and t denotes year of sale. The dependent variable is the natural log of the sales ratio, which can be thought of as approximately the percentage difference between assessed value and sale price. The independent variable of interest is the log of the property sale price. Jurisdiction-year fixed effects, α_{jt} , account for differences in the assessment level across jurisdictions and within jurisdictions over time. In this specification, β represents the elasticity of the assessment ratio with respect to sale price.

Table 2 reports regressions based on Equation (1), using increasingly specific jurisdictional fixed effects. Model (1), using county-year fixed effects, shows an elasticity of $-.34$, implying that, within a county and year, the assessment ratio declines by about 34 percent as the sale price doubles. Models (2) to (4) use city-year, school district-year, and finally county-city-school-year fixed effects, respectively. The coefficient is stable throughout, and, if anything, increases slightly as the jurisdictional fixed effects become more precise.

Figure 2 is a graphical counterpart to Table 2, showing a binned scatter plot of assessment ratios against sale prices based on all 26 million observations. To construct the figure, I first divided each sales ratio and sale price by its jurisdiction-year average. So, for example, if the average sales ratio in a jurisdiction is 10%, a property with a 15% sales ratio would register as 1.5 on this scale. Similarly, if the average sales ratio were 100%, a property assessed at 150% of its sale price would also register 1.5. The price variable is similarly scaled relative to the jurisdiction-year average. The graph shows declining sales ratios as sale prices increase, consistent with the city-specific graphs in Figure 1 and the regression results in Table 2.⁷

⁷ In the appendix, I evaluate an alternative metric of assessment regressivity, the price-related differential, or PRD. The PRD is a preferred metric of professional assessors due to its relative simplicity. I find similarly pervasive regressivity according to the PRD.

IV. Measurement error

The results of Section III show a strong negative relationship between assessment ratios and housing prices. As noted, however, such analyses are subject to concerns about bias due to measurement error in prices. In this section, I first present analyses that are not subject to the measurement error concern, followed by analyses that seek to evaluate the extent to which measurement error is likely to influence the estimates.

Census Tract Analysis

To begin, I analyze the relationship between assessment ratios and census tract characteristics. Any systematic relationship between assessment ratios and neighborhood characteristics cannot be the result of classical measurement error in sale prices, since random errors cannot be correlated with tract-level variables, else they would not be random (see PlaHovinsak and Vicentini n.d.). These analyses show the extent to which properties in different neighborhoods are assessed differently, but they will not capture differential assessment of properties by price within the same tract.

I identified the census tract of each property in the Corelogic data set based on its latitude and longitude. I then appended tract-level Census data for each property. To illustrate this approach, I begin by recreating a version of Figure 1 in which the tract-level median values of owner-occupied housing from the Census, rather than individual property sale prices, are used to define the bins. That is, in Figure 3, properties in each city are sorted into deciles of Census tract median value, and each dot displays the average sales ratio (left column) or average tax rate (right column) along with the average Census tract housing value in each decile. For example, the plot on the top left in Figure 3 shows average sales ratios by decline of census tract median value for Chicago. The leftmost dot in that plot shows that, for the bottom decile of properties defined by tract median value, the average sales ratio is about 16% and the average tract median value is about \$100,000. Meanwhile, for the top decile defined by tract median value, the average sales ratio is about 8.5% and the average tract median value is about \$580,000. All four cities continue to exhibit regressivity in sales ratios and tax rates according to tract-level housing values. In other words, properties located in tracts with lower median housing values have higher sales ratios and higher tax rates, on average. These relationships cannot be the result of idiosyncratic noise in individual property sale prices, since individual property sale prices were not used to construct the bins.

Moving beyond the four example cities, I next run regressions of sales ratios against census tract variables using the entire national data set. Each row in Table 3

contains the result of a different regression of the log assessment ratio against a tract-level demographic variable, as well as jurisdiction-year fixed effects.⁸ The results show that the assessment ratio is significantly correlated with census variables, which cannot be explained by measurement error in prices. Of particular note, the log assessment ratio is negatively correlated with the Census median value of owner-occupied housing, further evidence that lower-valued properties are more likely to be overassessed.

Assessment ratios are also negatively correlated with tract-level household income and education. They are positively correlated with the percent of the tract population that is black, consistent with the findings of Avenancio-Leon and Howard (2020), who have data on the race of individual homeowners, and with other studies of individual jurisdictions (Harris 2004). Assessment ratios are also positively correlated with the tract's Hispanic population share, but that relationship is not statistically significant.

Next, I regress the log of sale prices against the full set of tract-level variables shown in models (1) through (5), as well as jurisdiction-year fixed effects, and I recover the predicted value from that regression (the regression is reported in the appendix). These predicted values represent the component of property sale prices that can be predicted from neighborhood demographics alone. In model (6), I regress the log assessment ratio against the census-predicted housing values. The coefficient is negative and highly significant, indicating that an elasticity of -0.11 . Since census variables cannot predict random errors in prices, by definition, this regression, as well, is not subject to the concerns about bias due to measurement error on the right-hand side.

As noted, the census-based regressions do not capture any within-tract regressivity between properties of different sale prices, and as such the magnitudes of the estimates in Table 3 are not directly comparable with those in Table 2. As a point of comparison, I computed the tract-level median sale price from the Corelogic data, which I used as the independent variable in model (7) of Table 3. Random noise in individual sale prices should average out in the aggregation, meaning that this regression is too not subject to that form of bias. The estimate is roughly half as large as the model (4) in Table 2, an indication that within-tract regressivity is also important.

The results of Table 3 show that assessment regressivity is not merely an artifact of measurement error in sale prices: properties located in tracts with lower median housing values according to the census, as well as those in tracts with lower income and education and a larger proportion of African Americans, are assessed at

⁸ This analysis is equivalent to collapsing the assessment data by census tract and regressing the average assessment ratio against the Census tract median housing value, using weights equal to the number of sales in the tract.

higher levels on average. But these analyses do not imply that measurement error generates no attenuation bias in regressions such as those from Table 2. Therefore, it will be valuable to know how much of estimated assessment regressivity could be due to measurement error. I next present Monte Carlo simulations to shed light on that question.

Simulations

These simulations are meant to demonstrate the level of regressivity that would occur under the assumption that observed differences between assessed values and sale prices are due only to classical measurement error in prices. In other words, in these simulations, assessments are accurate in expectation but prices are subject to random noise. This is meant to represent a situation in which assessors have gotten market values right, but sale prices deviate from market values due to idiosyncratic factors associated with individual transactions. These simulations will be conservative in the sense that they assume assessed values would be perfect in the absence of random noise in sale prices. I vary the amount of noise in sale prices to show the extent to which apparent regressivity increases as noise increases. Specifically, for each property in the data set, I generate a simulated price that is equal to assessed value plus noise, where noise is a normally distributed random variable, mean zero with standard deviation of q .

$$(2) \quad \text{Simulated price} = AV \cdot (1 + \text{noise}(0, q))$$

In these simulations, q denotes a percentage of the property's sale price, and I examine values ranging from 0 to 50%. When $q = 0$, there is no noise in sale prices, assessments are perfect, and there is no regressivity; i.e., β in equation (1) where price is replaced by the simulated price is 0. I then run simulations with increasing noise in sale prices to recover the value of q that generates regressivity equivalent to the level observed in the real data. I vary q between 0 and 50% in increments of 5%, and for each value of q I run 20 simulations in which the simulated sale price is used as the independent variable and the denominator of the sales ratio in the regression defined in equation (1).

Results of the simulations are shown in Figure 4. The graph shows the average value of β from a version equation 1 where the log of simulated sales ratios are regressed against the log of simulated sale prices with a given level of noise. The horizontal line shows the level of regressivity in the real data, taken from equation (4) of Table 2. The average coefficient from the simulations increases as the amount of noise in sale prices increases, consistent with the idea that measurement error in sale prices can generate a spurious indication of regressivity. However, for

substantively plausible amounts of noise, the degree of spurious regressivity is negligible. When there is no noise in the simulated sale prices, the coefficient from the regression of equation (1) is zero, as it should be. The simulations do not reach the level of regressivity seen in the real data until noise reaches 35% to 40% of sale prices. While we have no way of knowing the actual amount of noise in residential sale prices, it seems implausible that it is anything close to 35%. At 5% noise, the observed regressivity coefficient is only $-.005$ and at 10% it is only $-.02$. The simulations indicate that the level of regressivity observed in the actual data cannot result from reasonable levels of idiosyncratic noise in sale prices. Recall that these simulations are conservative, insofar as they assume that assessments would be perfect but for measurement error in sale prices.

Repeat Sales

To further evaluate the role of measurement error in estimates of regressivity, I use a repeat-sales analysis. If the difference between assessed values and sale prices is due to random error in sale prices, then today's sale price should not predict a future sale price for the same property after controlling for today's assessed value. That is, random error cannot be correlated with future prices. However, if today's prices predict future prices, even after controlling for today's assessed value, it must be that the deviation between assessed value and prices today is due to a failure of assessed values to incorporate meaningful information about prices today.

The Corelogic data contains 1.7 million properties that sold more than once during the study period with sale dates separated by at least three years. For these 1.7 million properties, I regress the sale price from the second sale against the sale price from the first sale, the assessed value at the time of the first sale, a set of jurisdiction fixed effects, and year-of-sale fixed effects for each of the two sales. Table 4 shows the results. In models (1) and (2), I regress sale price against previous sale prices and previous assessed value, respectively. Both previous price and previous assessed value predict future prices, although previous prices are a stronger predictor, as measured by the elasticity or the r -squared of the regression. More importantly, model (3), which includes both previous sale price and previous assessed value, shows that previous sale price remains a strong predictor of future prices after accounting for previous assessed value. Indeed, previous price is a much stronger predictor than previous assessed value. This means that the differences between assessed values and sale prices at time 1 cannot be due entirely to random noise in sale prices.

Taken together, the preceding analyses show that assessment regressivity cannot be explained as the result of measurement error in sale prices.

V. County-level Analysis

The analyses in the preceding sections capture a nationwide picture of assessment regressivity. To gauge place-to-place variation, I ran a version of equation (1) separately for each county represented in the Corelogic data. Specifically, for each county, I ran a regression of the log sales ratio against the log sale price, including year-of-sale fixed effects. The distribution of the coefficients for all 1,749 counties with at least 1,000 sales is shown in Figure 5. Nearly all of the coefficients—1,701 out of 1,749—are negative. Most—1,691—are statistically significant at the 5% level (significance is not reflected in the figure).

Next, I repeat the county-by-county analysis using tract-level median housing value rather than sale price, which obviates concerns about measurement error in sale prices. The distribution of the coefficients is shown in Figure 6. Sixty-eight percent of the coefficients—1,193 out of 1,749—are negative. More than half of these negative coefficients—648 out of 1,193—were statistically significant. Among the 529 positive coefficients (indicating progressivity), 172 were statistically significant.

Comparison of the results using individual sale prices versus tract-level median housing values is instructive. The former may overstate regressivity because of measurement error in sale prices—although the results from section IV suggest that such bias is not large. At the same time, the latter ignore within-tract regressivity for properties of different prices. The true proportion of localities with regressive assessments likely lies somewhere between the proportions represented by these two analyses.

VI. Evidence on Sources of Regressivity

The evidence presented thus far shows that assessment regressivity is widespread and that it is likely due mainly to a combination of assessing errors and policy choices. This section evaluates which of the potential causes outlined in Section I are the most important sources of assessment regressivity in practice.

Assessment increase caps

Sixteen states have caps on annual increases in assessed values (see Dornfest, Ireland, and Southard 2020). While details vary, the general goal of such caps is to constrain assessment increases in rapidly appreciating neighborhoods. The ultimate impact of these caps on assessment regressivity is not easy to predict

and will depend on whether there is differential price appreciation between high- and low-priced neighborhoods within a given locality, as well as particular features of the rule (Haveman and Sexton 2008). In some systems, the cap resets at the time of sale, which tends to create a bias in favor of longtime residents, but is ambiguous in terms of regressivity according to price. Under some conditions, assessment limits can even lead to increases in taxes for properties whose appreciation is above but near the cap if effective tax rates are increased to maintain the level of revenue (e.g., Dornfest, Ireland, and Southard 2020). Empirically, the evidence has been mixed. There is evidence that assessment increase caps have been regressive in New York City (Hayashi 2014; New York City Advisory Commission on Property Tax Reform 2020), but mildly progressive in Chicago (Dye, McMillen, and Merriman 2006).

To gain some purchase on the general effects of assessment caps, I estimate equation (1) separately for states with and without assessment limits, as classified by Dornfest, Ireland, and Southard (2020). Results are shown in Table 5. There is no apparent difference in assessment regressivity between the two groups of states. The elasticity of assessment ratio with respect to sale price is slightly stronger for states without assessment caps, but the difference is not significant. While this analysis does not rule out the possibility that assessment caps contribute to regressivity in some localities, it does suggest that caps do not have such impacts in general.

Property Classification

In assessment parlance, *classification* refers to the practice applying different assessment rules to different types of property (Gloudemans and Almy 2011). For example, commercial, residential, agricultural, and industrial properties are often treated differently for tax purposes within the same jurisdiction. In New York City, condominiums and co-ops are treated as a different class from single family homes, and the former are assessed at a higher rate. This differential treatment has been alleged as a source of assessment regressivity in New York City (New York City Advisory Commission on Property Tax Reform 2020). While this sort of differential assessment for classes of residential properties appears to be relatively unusual, it is common for assessors to use different statistical models for evaluating multi-family and single-family homes, which might also be a source of regressivity if one category were systematically over- or under-assessed relative to another.

To investigate whether differential assessment of different categories of residential properties—whether due to official classification or use of different assessment models—contributes to overall regressivity, I run a version of model (1) for each residential property type represented in the Corelogic data: single-family

homes; duplexes; and condominiums. The results are shown in Table 6. There is significant assessment regressivity within each property type. Duplexes and condominiums exhibit greater within-category regressivity than single-family homes. In model (4) I include a set of jurisdiction-by-year-by-category fixed effects to examine whether different assessment rules across categories could influence my overall estimates of assessment regressivity. The coefficient on sale price is effectively unchanged relative to Table 2, suggesting that property classification is not an important source of overall assessment regressivity in general. This is not to say that classification is not a source of regressivity in some jurisdictions, such as may be the case in New York City.

Appeals

In all states, property owners have the right to appeal their assessments (IAAO 2016; Officers 2018b). In particular, a standard practice is for the assessor to mail a *notice of assessment* announcing a property's *first-pass*, or proposed, assessed value, allowing owners a limited period of time to appeal before tax bills are computed. Successful appeals result in reductions in assessed values and resulting tax bills. Making an appeal requires knowledge of the process and, in some cases, legal representation (see Officers 2018b). As such, if appeals are disproportionately brought by owners of more valuable property—who stand to benefit more from a reduction and likely have better access to lawyers—the appeals process may actually generate regressivity by delivering reductions disproportionately to high-priced properties.

The assessed values in the Corelogic data reflect the final, post-appeal assessments in place at the time tax bills are calculated. I am not aware of any comprehensive nationwide dataset on pre-appeal, first-pass assessed values. However, data can often be obtained from individual jurisdictions. To illustrate the potential impact of appeals on assessment regressivity, I use data from Cook County, Illinois, where appeals have been a subject of some controversy (e.g., Grotto 2017b). In 2015 alone, Cook County processed 166,000 appeals, whereas by comparison New York City processed 53,000 appeals and San Francisco processed 4,995 in the same year (Grotto 2017b). Moreover, roughly 80% of appeals in Cook County result in a reduction in assessed values (Ross 2017). If appeals can alter the overall regressivity of assessments, Cook County is an obvious place to look. Prior studies show that appeals increase regressivity, but that assessments were already regressive prior to appeals (McMillen 2013; Ross 2017).

I use data on first-pass and final, post-appeal assessed values obtained from the Cook County Assessor's Office for tax years 2011 through 2016.⁹ I compare pre- and post-appeal assessed values by sale price. The main results are shown in Figure 6, which is a binned scatterplot of assessed values against sale price, adjusted for year fixed effects. Two important facts are worth noting. First, the pre-appeals assessments are regressive. Second, appeals result in greater proportional reductions, on average, for higher-priced homes, meaning that post-appeals assessments are even more regressive.

Table 7 presents comparable regressions using the Cook County appeals data. Model (1) is a linear probability model of the probability of appealing, conditional on sale price and first-pass sales ratio. Owners of more expensive properties are more likely to appeal, holding constant the initial sales ratio. The first-pass sales ratio itself is insignificantly related to the probability of appealing. Models (2) and (3), respectively, show the slope of the sales ratio by sale price before and after appeals. Regressivity is worse after appeals, but not by much. Finally, model (4) adds a dummy variable for properties that appealed, with the coefficient indicating that appeals lead to a 14% reduction in the sales ratio, on average.

The results from Figure 7 and Table 7 suggest that appeals worsen but are not the first cause of regressivity in Cook County, consistent with McMillen (2013), Ross (2017), and Grotto (2017b). While this evidence is specific to one county, there are reasons to think that Cook County would be close to the upper bound for the effect of appeals. Cook County processes perhaps the largest number of appeals of any jurisdiction in the country and grants reductions in 80% of cases (Grotto 2017b; Ross 2017). It seems unlikely, therefore, that appeals would have a much larger effect on assessment regressivity in many other jurisdictions.

Data and Modeling Limitations

As emphasized in section I above, data and modeling limitations may also contribute to assessment regressivity. In particular, when assessors do not have access to all the variables that explain sale prices, the residual variation in prices will be a source of assessment regressivity. A property whose value is below average relative to its observable characteristics will be over-assessed, while a property whose price is high relative to its observable features will be under-assessed. As a general matter, it is difficult to know the extent to which this sort of unexplained variation in sale prices contributes to regressivity, because variables that are unobservable to the assessor are also generally unobservable to outside analysts.

It is possible, however, to evaluate how strongly assessed values correlate

⁹ This is the same data set used in Ross (2017).

with sale prices. Since most assessment models are based on regression methods (Gloudemans and Almy 2011; Officers 2018a), one can think of assessed values as being equivalent to the predicted values from a hedonic regression of sale prices against observable property characteristics. As such, a regression of sale prices against assessed values in place at the time of sale would recover the out-of-sample R-squared from the original hedonic regression (McMillen and Singh 2020; PlaHovinsak and Vicentini n.d.). This is an important metric, considering that assessment is fundamentally about out-of-sample prediction; that is, using a model based on properties that have sold to predict values for properties that have not sold.

I ran a regression of sale price against assessed value for each of the 2,628 counties represented in the Corelogic sample. The average r-squared of these regressions was .46; the median r-squared was .49. The figures do not change substantially (.47 and .50, respectively) if I restrict the summary to the 1,749 counties with at least 1,000 sales. The interquartile range of the county-level r-squared is from .25 to .67. A histogram of the county-level r-squared values is presented in Figure 8. Clearly, there is variation in performance across assessors, but there is a great deal of unexplained variation in sale prices in the vast majority of jurisdictions.

Unexplained variation in these county-level sale price regressions is primarily due to three factors: omitted variables that are observable to buyers and sellers but not to the assessor; imperfections in modeling the included covariates (e.g., functional form); and random factors associated with individual property sales. It is not possible to completely disentangle these three factors without access to the data and variables used in the assessment models of individual jurisdictions. However, auxiliary analyses can shed some light on the role of idiosyncratic shocks and modeling imperfections.

Given the average r-squared of .46, it is hard to imagine that idiosyncratic shocks account for all of the unexplained variation, as this would imply that random noise accounts for more than half of the variation in sale prices. Moreover, the wide range of r-squared values across counties also seems hard to attribute to random noise, without an explanation for why there should be such great variation in noise across local housing markets. If the noise comes from idiosyncratic buyer-seller interactions, one would expect it to be relatively common across markets.

Simulations can provide a sense of the expected r-squared when there is random noise in sale prices. Following the same approach discussed in section IV above, I simulated sale prices following equation (2) and ran the county-level regressions of simulated sale prices against assessed values under different levels of simulated noise. With sale prices equal to assessed values plus a mean zero shock with standard deviation of 5 percent, the average r-squared from the county-level

regression of simulated prices against assessed values is .99. With a noise set to 10 percent of sale prices, the average r-squared is .97. And even with noise of 20 percent, the average r-squared is .90.

Based on both a comparison of the r-squared statistics from the real data and from the simulations, it appears extremely unlikely that idiosyncratic factors are the main source of the unexplained variation in the real county-level regressions. This is consistent with the evidence from section IV, suggesting that idiosyncratic factors do not account for the negative relationship between assessment ratios and sale prices. Rather, there appears to be a great deal of variation in sale prices that is not reflected in assessments, but not random. This variation is likely due to property features that are observable to buyers and sellers but not to the assessor, and to imperfections in assessment models.

Implications

In the presence of assessment regressivity, the property tax cannot be seen as an *ad valorem* tax and, as such, fails to satisfy several basic principles of good taxation (see, e.g., Ihlanfeldt 2013). Because owners of high-priced properties pay a lower effective tax rate than owners of low-priced properties, the property tax, as typically administered, does not satisfy horizontal equity. In addition, given the correlation between residential house values and income, the property tax likely does not satisfy the ability to pay principle.

In addition to violating principles of good taxation, property tax regressivity often violates the law. The 14th amendment's equal protection clause requires that all property of the same class--e.g., residential--be taxed at the same rate. However, courts have interpreted this clause not to require perfect uniformity, but only to protect property owners from intentional and systematic discrimination (Comment 1976). In contrast to the U.S. constitution, most state constitutions require "uniformity" or "proportionality" in tax rates applied to property within a given class, and these state-level protections generally hold regardless of intentionality (Kincaid 2012; Newhouse 1984). In addition, a few states provide absolute limits on assessment levels. For instance, Michigan requires that no property be assessed at more than 50 percent of its market value. Detroit has regularly violated this constitutional requirement by assessing a majority of its residential properties in excess of the 50 percent limit, as shown in Figure 1 above (Atuahene and Berry 2019; also see Hodge et al. 2017).

Legal issues aside, a welfare analysis of assessment regressivity would have to account for tax capitalization, which is one of the thornier issues in local public finance (Oates and Fischel 2016; Zodrow 2006). In principle, property taxes should

be capitalized into property values, meaning that properties that are over-taxed should sell at a lower price. With perfect capitalization, lower sale prices should exactly offset higher taxes. If so, the initial owner of the property at the time the tax is imposed would bear the full burden of the unfair taxation.

As an empirical matter, however, most studies have found that taxes are only partially capitalized (see Sirmans, Gatzlaff, and Macpherson 2008b). And, of course, unexpected future changes in taxes will not be capitalized into property values. For instance, it appears that property taxes became more regressive during the great recession, as shown in Table 8. This is consistent with the notion that low-priced properties lost more value than high-properties during this period (Cohen, Coughlin, and Lopez 2012) and that assessments were slow to capture such changes, consistent with the trends shown in Table 1.

Such considerations notwithstanding, to the extent that property taxes are capitalized into property values, correcting assessment regressivity would generate equity for current owners of over-assessed properties, which are more likely to be low-priced, while eroding equity for current owners of under-assessed properties, which tend to be higher priced.

Conclusion

Assessment regressivity, and resulting property tax regressivity, is widespread in the U.S. The observed patterns of regressivity cannot be explained as artifacts of measurement error, nor do they arise from inequities in the appeals process or statutory limitations on assessment increases. Rather, regressivity arises in the process of assessment, due to a combination of data and modeling limitations. As a result, despite its appealing features in theory, social scientists and policymakers should recognize the property tax as being regressive in practice.

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Sales Ratio

Tax Rate

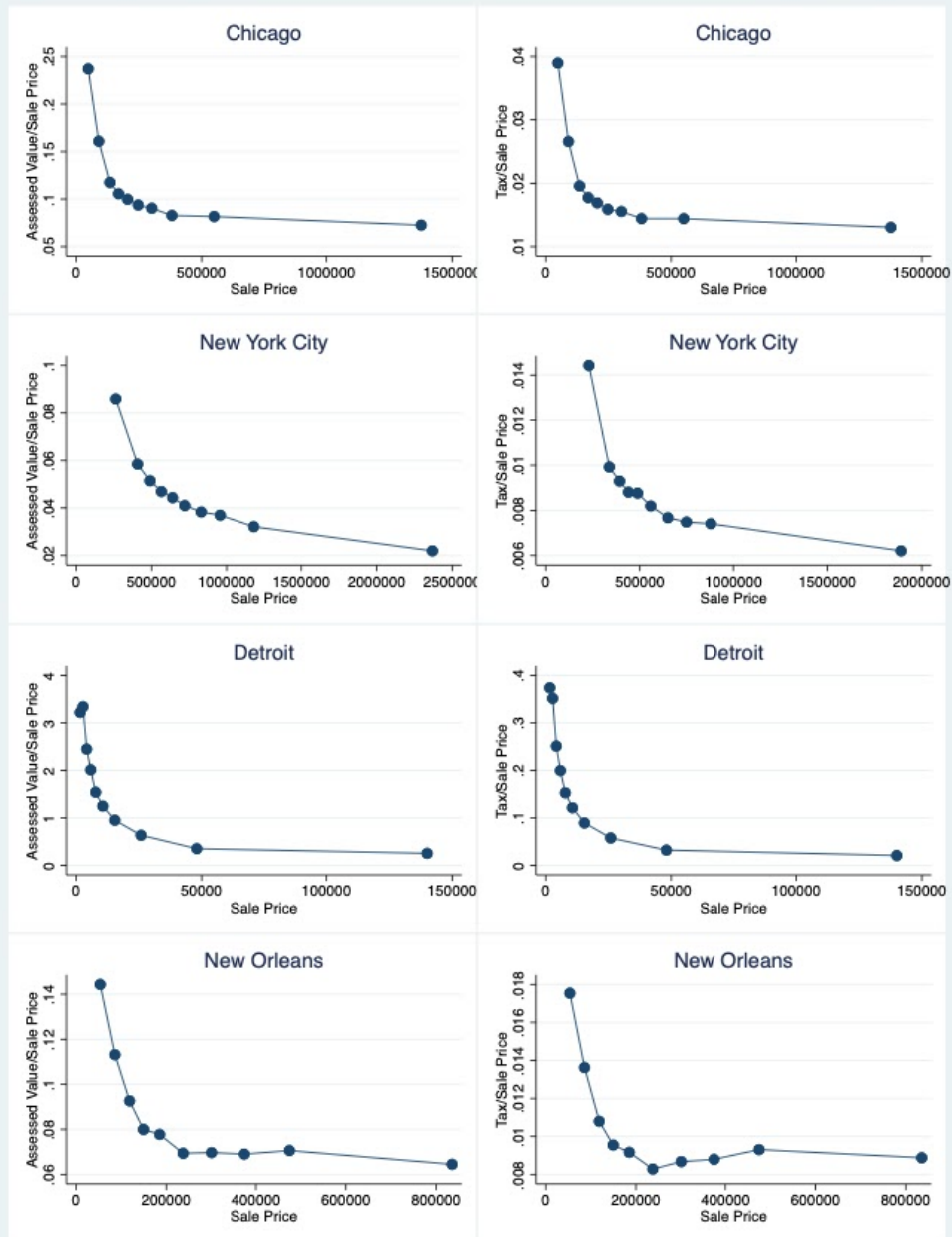


Figure 1: Regressivity in Chicago, New York, Detroit, and New Orleans

Notes: Binned scatter plots show average sales ratio (left column) and average tax rate (right column) by decile of sale price. Sales ratio is the assessed value divided by sale price; tax rate is the tax due in year of sale divided by sale price. Data for single family homes that sold between from 2015 to 2017: 9,638 sales in Chicago; 25,652 in New York City; 12,335 in Detroit; 7,546 in New Orleans.

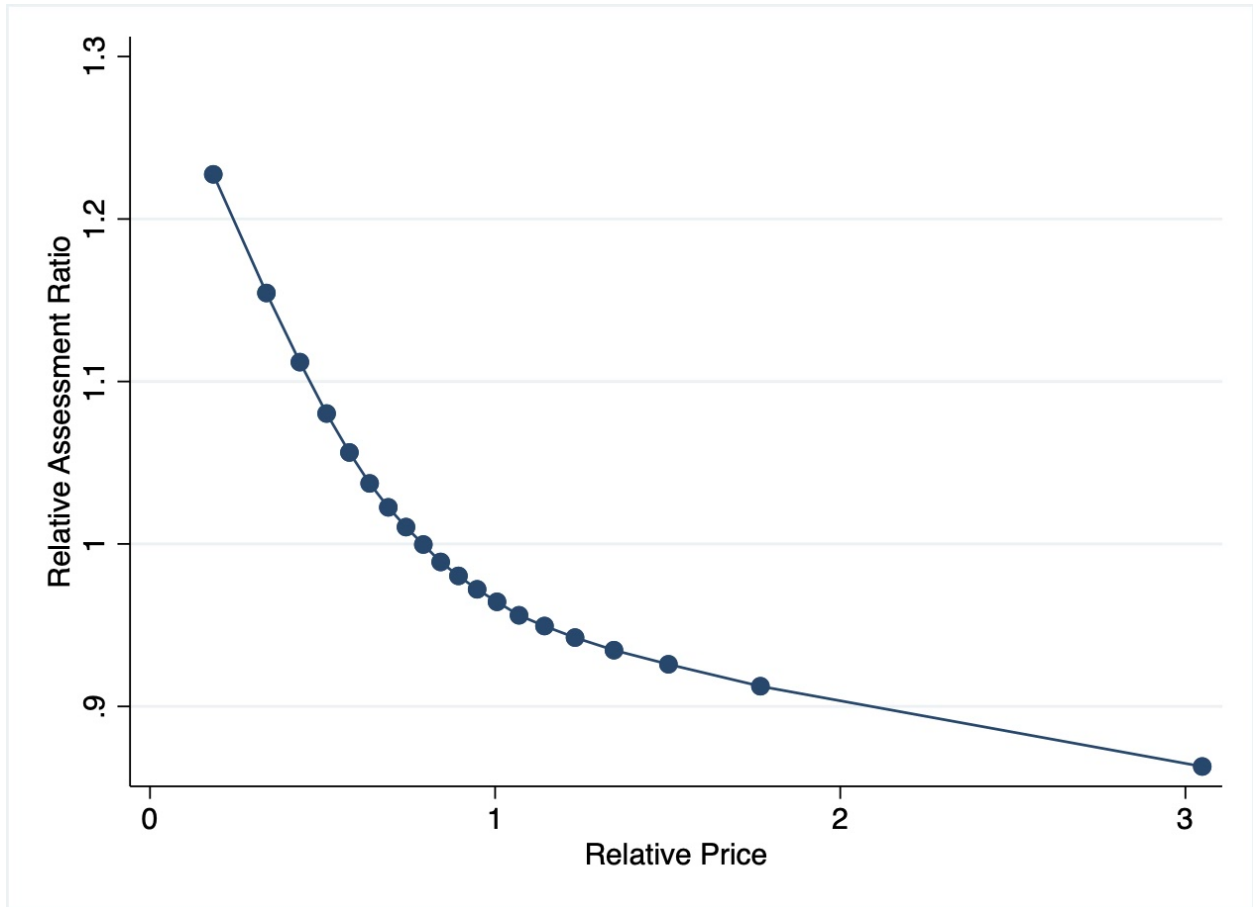


Figure 2: National Profile of Assessment Regressivity

Notes: Relative price is property sale price divided by county average price in year of sale. Relative assessment ratio is property's sales ratio divided by county average sales ratio in year of sale. Binned scatter plot shows average relative assessment ratio and average relative price by 20 quantiles of relative sale price. Based on 26 million residential sales contained in Corelogic tax and deed database.

Sales Ratio

Tax Rate

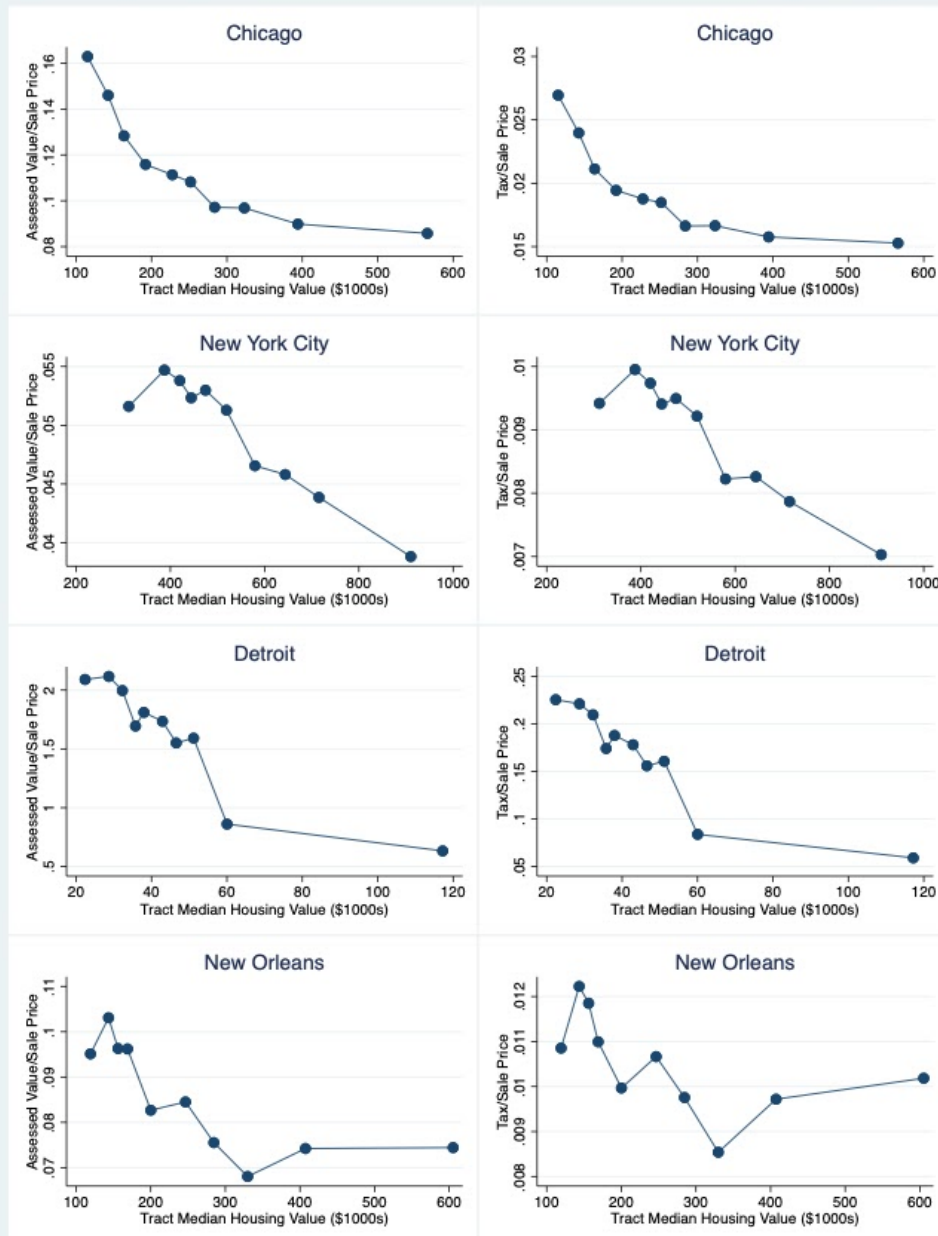


Figure 3: Regressivity Using Census Housing Values in Chicago, New York, Detroit, and New Orleans

Notes: Binned scatter plots show average sales ratio (left column) and average tax rate (right column) by decile of tract-level median housing value. Sales ratio is the assessed value divided by sale price; tax rate is the tax due in year of sale divided by sale price. Each property is matched to its census tract, and properties are divided into 10 equally sized bins based on tract-level median value of owner-occupied housing from the Census. Data for single-family homes that sold between from 2015 to 2017: 9,638 sales in Chicago; 25,652 in New York City; 12,335 in Detroit; 7,546 in New Orleans.

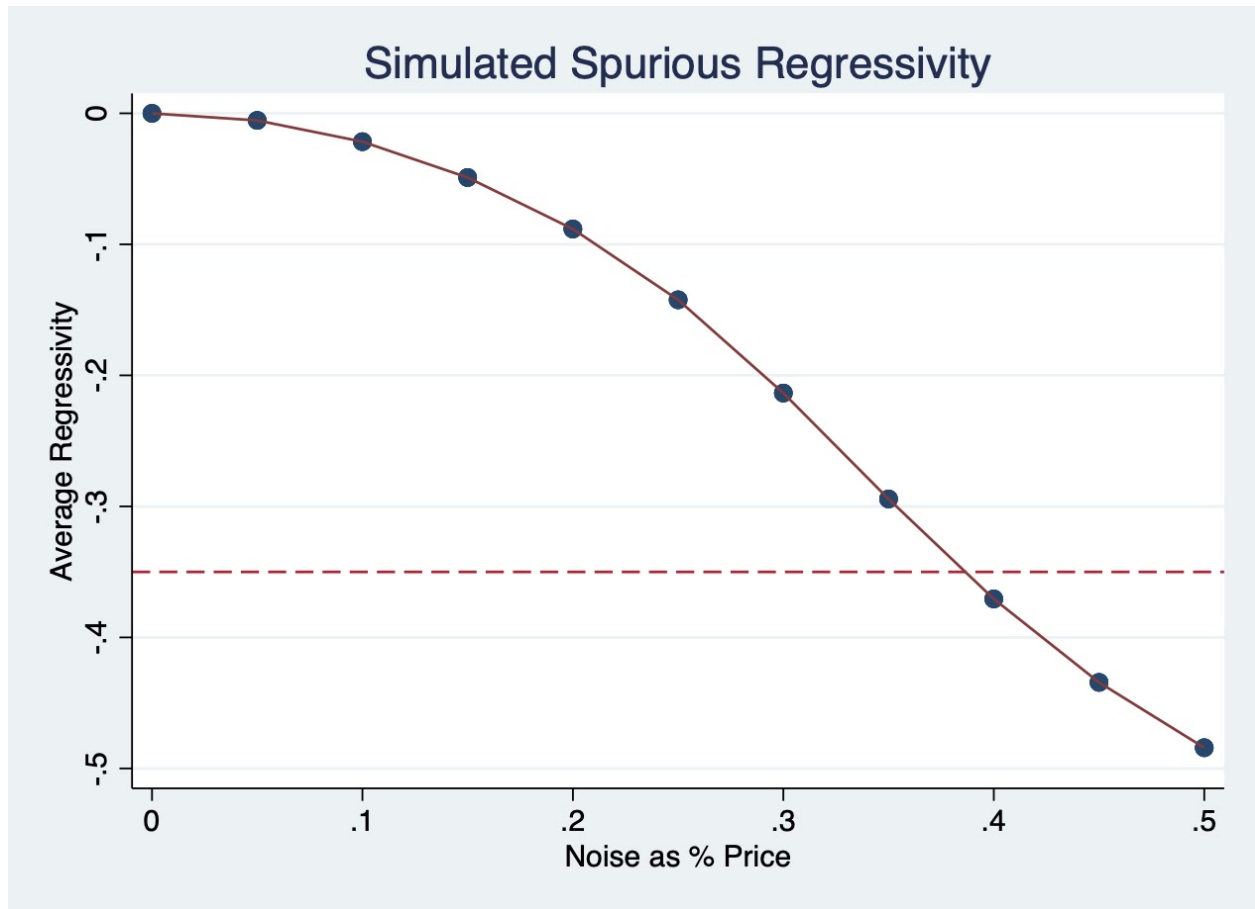


Figure 4: Monte Carlo Simulation of Measurement Error and Regressivity

Notes: Figure shows average coefficients from regressions of log assessment ratio against simulated prices. Simulated prices are set equal to assessed value plus noise, where noise is normally distributed random variable with mean zero with a standard deviation equal to a varying share of the sale price. For each level of noise, ranging from 0 to 50% in increments of 5%, 20 regressions were run, with log of the simulated sales ratio regressed against log of the simulated sale price, and the average value of the coefficient is reported. The horizontal dashed line represents the estimate from the real data, taken from model (4) of Table (2).

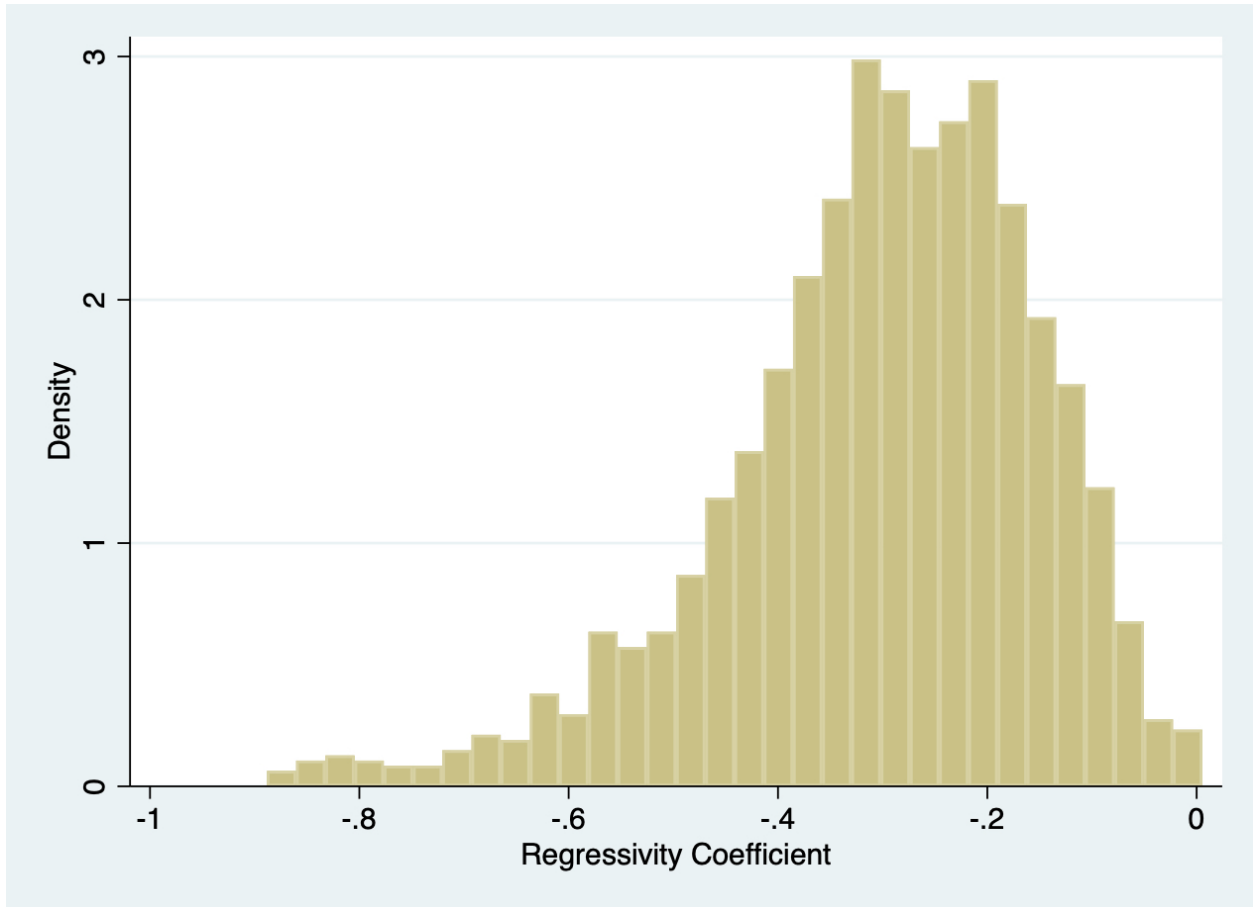


Figure 5: Histogram of County-level Regressivity Using Sale Price

Notes: Distribution of r -squared values from regression of log assessment ratio against log sale price for counties with at least 1,000 sales. Figure excludes largest and smallest 1 percent of coefficients to improve interpretability.

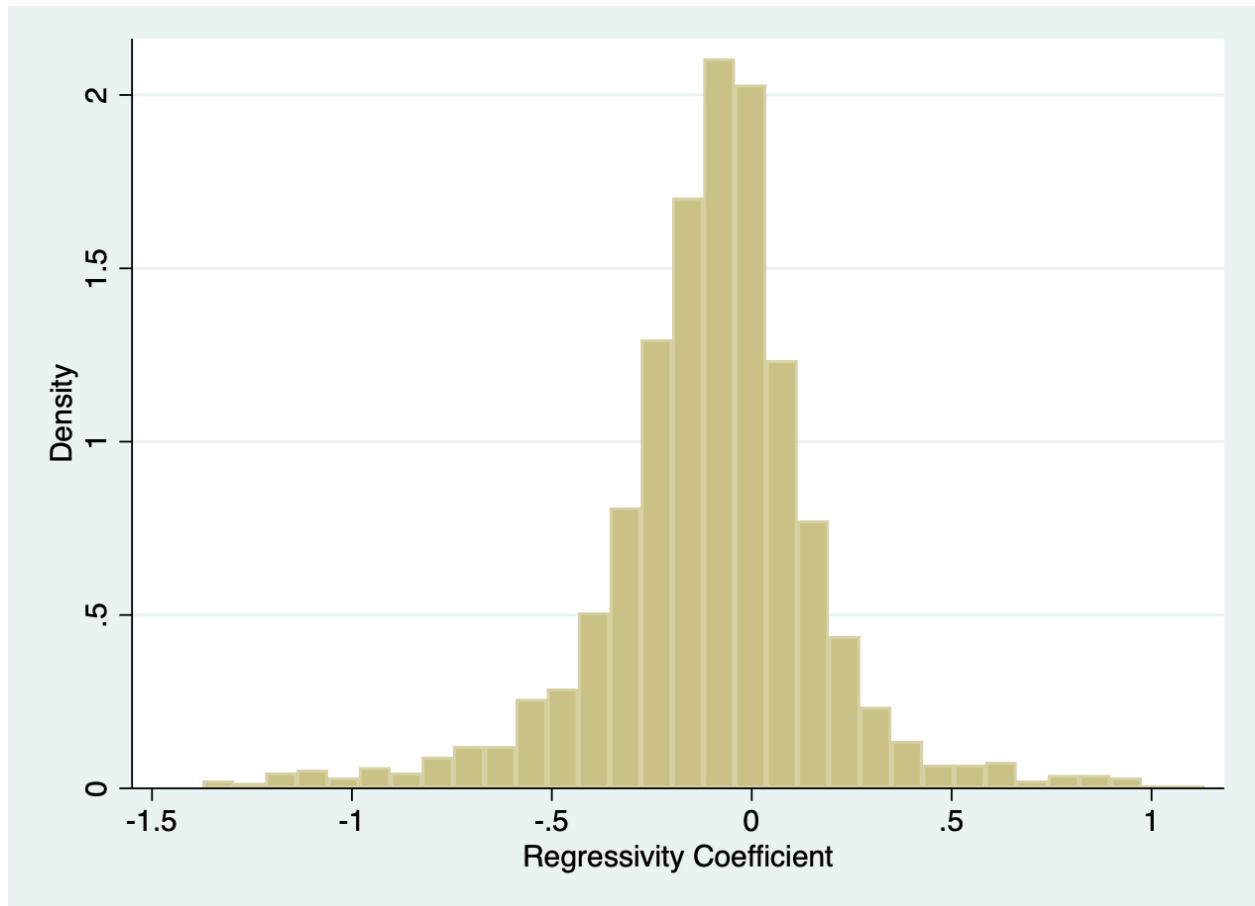


Figure 6: Histogram of County-level Regressivity Using Tract Median Value

Notes: Distribution of r-squared values from regression of log assessment ratio against log of Census tract median housing value for counties with at least 1,000 sales. Excludes largest and smallest 1 percent of coefficients to improve interpretability.

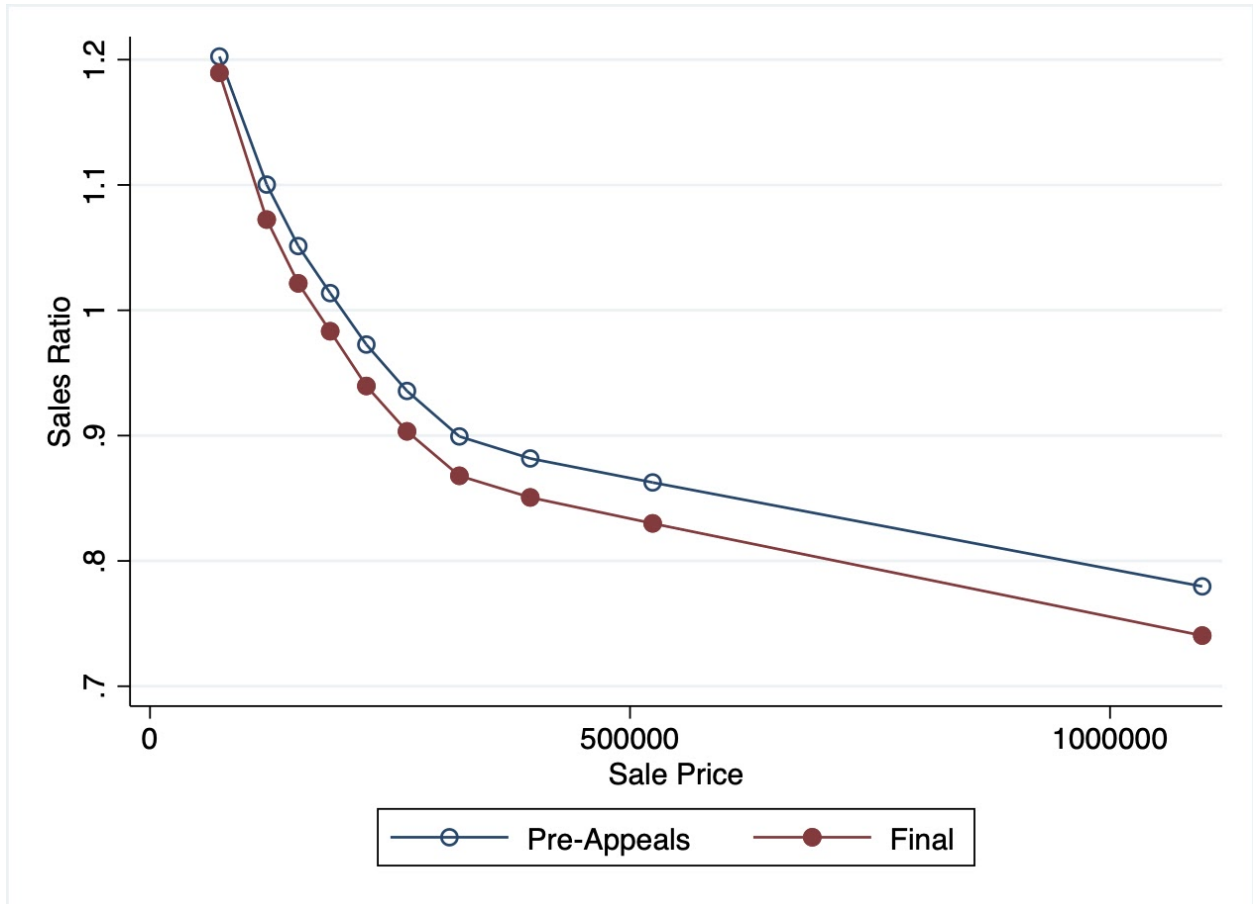


Figure 7: Binned Scatter Plot of Cook County Regressivity Before & After Appeals

Notes: Hollow circles represent average sales ratio before appeals and solid circles represent average sales ratio after appeals, by sales price decile. Data from Cook County, Illinois, for properties sold from 2011 to 2016.

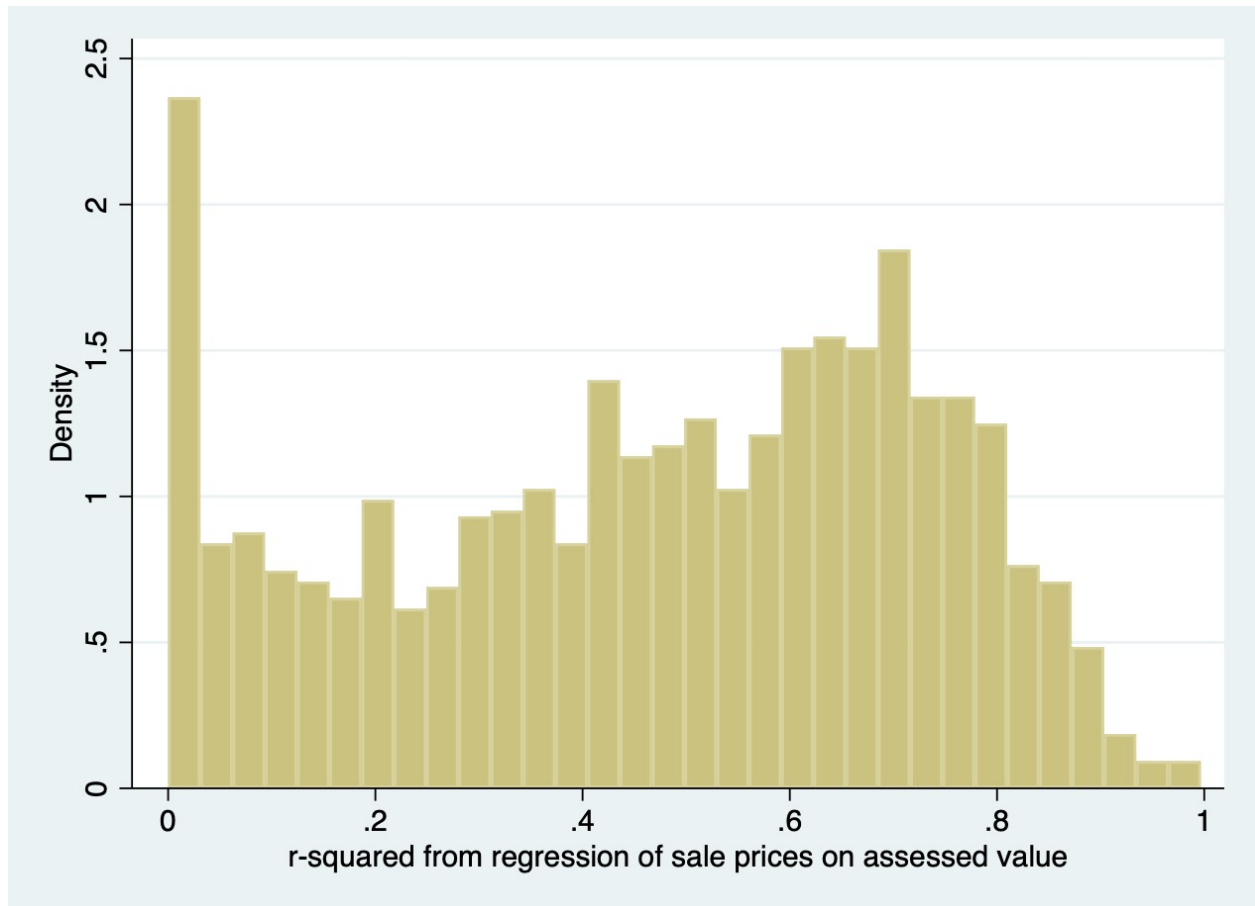


Figure 8: Histogram of county-level r^2 values

Notes: Figure represents the distribution of r^2 from a regression of log sale price against log assessed value for 1,749 counties with at least 1,000 sales.

Table 1: Descriptive Statistics

Year	Avg. Sales		Avg. Assessed Value	Avg. Tax Rate	Number of Transactions	Number of Counties Represented
	Ratio	Avg. Sale Price				
2007	54.5%	\$ 246,156	\$ 105,975	1.38%	1,660,689	1,046
2008	65.9%	\$ 234,429	\$ 122,720	1.77%	2,065,847	1,575
2009	75.0%	\$ 215,338	\$ 126,929	2.06%	2,175,244	1,791
2010	71.7%	\$ 228,180	\$ 128,041	1.93%	2,160,334	2,081
2011	72.2%	\$ 229,749	\$ 126,077	2.07%	2,216,836	2,290
2012	68.0%	\$ 256,913	\$ 127,015	1.92%	2,583,060	2,388
2013	62.8%	\$ 249,877	\$ 128,302	1.73%	3,023,318	2,444
2014	61.9%	\$ 271,189	\$ 132,114	1.63%	3,169,235	2,498
2015	60.2%	\$ 309,135	\$ 136,756	1.51%	3,540,358	2,477
2016	59.7%	\$ 299,405	\$ 145,647	1.42%	3,441,107	2,273
Total	64.7%	\$ 259,910	\$ 129,962	1.72%	26,000,000	2,653

Notes: Sales ratio is the assessed value in place on January 1 of the sale year divided by the sale price.
The tax rate is the total property tax due in the sale year divided by the sale price.

Table 2: Regressions of Assessment Ratio against Sale Price

	(1)	(2)	(3)	(4)
ln(Sale Price)	-0.337*** (0.0138)	-0.349*** (0.0136)	-0.353*** (0.0131)	-0.362*** (0.0134)
Observations	25,801,036	25,795,551	25,791,035	25,781,302
R-squared	0.774	0.795	0.793	0.807
	County x	School x		Jurisdiction
Fixed Effects	Year	Year	City x Year	x Year

Notes: The dependent variable is the log of the sales ratio. In model (4), jurisdictions are unique overlapping combinations of county, city, and school district. Robust standard errors clustered by county are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Assessment Ratio against Census Demographics

Regression	Census Variable	Coefficient	S.E.	Observations	R-squared
(1)	ln(Median Value)	-0.0966***	(0.0173)	25,748,826	0.768
(2)	ln(Median HH Income)	-0.0841***	(0.0184)	25,778,911	0.767
(3)	Pct. Black Non-Hispanic	0.193***	(0.0373)	25,781,096	0.767
(4)	Pct. Hispanic	0.0881	(0.0536)	25,781,096	0.767
(5)	Pct. BA or Higher	-0.281***	(0.0581)	25,781,094	0.768
(6)	Census-predicted price	-0.109***	(0.0199)	25,747,895	0.768
(7)	Corelogic tract median sale price	-0.159***	(0.0171)	25,764,387	0.773

Notes: Each row presents the results of a different regression. The log sales ratio is the dependent variable in all regressions. In models (1) through (5) the log of the sales ratio is regressed against the named tract-level covariate. In model (6) it is regressed against the predicted values from a regression of all 5 tract-level covariates against sale price. In model (7) it is regressed against the tract-level average sale price. All models include jurisdiction-by-year fixed effects. Robust standard errors clustered by county are in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Repeat Sales

	(1)	(2)	(3)
ln(Prior sale price)	0.825*** (0.0110)		0.705*** (0.0144)
ln(Prior assessed value)		0.592*** (0.0223)	0.164*** (0.0123)
Observations	1,778,885	1,743,405	1,743,405
R-squared	0.773	0.615	0.788
Jurisdiction FE	Y	Y	Y
Year of first sale FE	Y	Y	Y
Year of second sale FE	Y	Y	Y

Notes: Regressions of prior sale price and assessed value against next sale price. Analysis restricted to properties that sold more than once at least three years apart. Sale price t1 is the price at first sale, assessed value t1 is the assessed value in place on January 1 of the year of the first sale. The dependent variable, sale price t2, is the price at the next sale. All models include jurisdiction fixed effects and fixed effects for the year of each sale. Robust standard errors clustered by county in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Assessment Growth Caps

	(1)	(2)
ln(Sale Price)	-0.373*** (0.0155)	-0.348*** (0.0229)
Observations	14,403,586	11,377,716
R-squared	0.795	0.815
Fixed Effects	Jurisdiction x year	Jurisdiction x year
Cap	No	Yes

Notes: The dependent variable is the log of the sales ratio, $\ln(A/P)$. Model (1) includes states with assessment growth caps, model (2) includes states without assessment growth caps. States are classified according to the data in Dornfest et al. (2020). Robust standard errors clustered by county in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: Property Classification

	(1) Single Family	(2) Duplex	(3) Condominium	(4) All
ln(Sale Price)	-0.331*** (0.0104)	-0.499*** (0.0441)	-0.543*** (0.0218)	-0.362*** (0.0123)
Observations	22,277,822	3,060,061	421,192	25,759,075
R-squared	0.812	0.823	0.941	0.816
Fixed Effects	Jurisdiction- year	Jurisdiction- year	Jurisdiction- year	Jurisdiction- year-type

Notes: The dependent variable is the log sales ratio, $\ln(A/P)$. Models (1), (2), and (3) are restricted to single-family homes, duplexes, and condominiums, respectively. Model (4) includes all three property types and adds jurisdiction-by-year-by-property type fixed effects. Robust standard errors clustered by county in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7: Assessment Appeals in Cook County

	(1)	(2)	(3)	(4)
ln(Sale Price)	0.0228*** (0.00138)	-0.184*** (0.00142)	-0.194*** (0.00149)	-0.191*** (0.00148)
ln(First-pass sales ratio)	-0.00217 (0.00225)			
Appeal dummy				-0.145*** (0.00273)
Constant	-0.0670*** (0.0171)	2.182*** (0.0177)	2.264*** (0.0185)	2.258*** (0.0184)
Observations	169,172	169,172	169,962	169,962
R-squared	0.029	0.106	0.099	0.113
Dependent variable	Appeal Dummy	Pre-Appeal Ratio	Post-Appeal Ratio	Post-Appeal Ratio

Notes: Data from Cook County Assessor's Office, includes properties sold from 2011 to 2016. The dependent variable in model (1) is a dummy variable indicating whether the owner appealed the first-pass assessed value. The dependent variable in model (2) is the sales ratio based on the first-pass assessed value. The dependent variable in models (3) and (4) is the post-appeal sales ratio for properties that appealed or the first-pass assessed value for properties that did not appeal.

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Yearly Estimates of Assessment Regressivity

Year	Coefficient	S.E.	Observations	R-squared
2007	-0.318***	(0.0222)	1,612,518	0.802
2008	-0.358***	(0.0179)	2,005,494	0.826
2009	-0.382***	(0.0176)	2,124,814	0.838
2010	-0.379***	(0.0171)	2,133,715	0.825
2011	-0.367***	(0.0162)	2,191,696	0.822
2012	-0.405***	(0.0368)	2,564,542	0.811
2013	-0.343***	(0.0204)	3,011,232	0.808
2014	-0.350***	(0.0187)	3,161,576	0.790
2015	-0.380***	(0.0219)	3,536,730	0.783
2016	-0.322***	(0.0195)	3,438,985	0.783

Notes: Each row reports a regression for one year of sales data. The dependent variable is the log of the sales ratio and the independent variable is log sale price. All models include fixed effects for jurisdiction (unique overlapping combinations of county, city, and school district). Robust standard errors clustered by county in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Appendix: The Price-Related Differential

Within the professional assessing community, the most commonly used metric of assessment regressivity is the *price-related differential*, or PRD (IAAO 2013), which is the arithmetic mean of the sales ratio divided by the price-weighted mean sales ratio. If higher-priced properties are assessed at higher sales ratios than lower-priced properties, the PRD will be greater than one. The PRD is subject to the same bias due to measurement error that was discussed in Section IV with respect to regression-based methods (see Officers 2018a, p. 488). Further, the PRD can be significantly influenced by a small number of very high-priced properties (Gloudemans and Almy 2011). Given the potential biases of the PRD, the International Association of Assessing Officers (IAAO) has established the “acceptable” range for the PRD as being from .98 to 1.03. PRD values above 1.03 are considered regressive, while those below .98 would be considered progressive (IAAO 2013).

The PRD is widely used among professional assessors and often presented in official reports (see Center for Municipal Finance 2018). As such, it is worthwhile to examine PRD estimates as a point of comparison with the regression-based results presented previously. I computed the PRD for each county in the data, separately for each year, resulting in 20,238 county-year PRD estimates. Among the 15,274 county-years with at least 100 sales, the average value of the PRD is 1.25; the median is 1.13. Ninety percent of these county-years register a PRD in excess of the 1.03 threshold. Figure A1 displays a histogram of the county-year PRD estimates, showing the mass of values indicating regressive assessments.

As noted, the PRD is also subject to bias from measurement error in sale prices, which could lead the PRD to exceed 1 even when assessments are not regressive. To gauge the extent which such bias may influence my analyses, I ran a variation of the simulations described in Section IV. As before, I run simulations in which simulated sale prices for each property are set equal to assessed value plus noise, where noise is a normally distributed random variable with mean zero and standard deviation, q , that varies across simulations. I consider values of q ranging from .05 to .50, in increments of .05, which reflects noise in sale prices equivalent to anywhere from 5% to 50% of assessed value. I run 20 simulations for each value of q , and in each iteration of the simulations I compute the PRD for every county. Figure A2 displays the average county-level PRD across all the simulations for a given value of q . As with the regression-based simulations shown in Figure 4, it is clear that large amounts of noise in sale prices can lead to spurious indications of regressivity in the PRD. However, at plausible levels of noise, the bias in the PRD is minimal. When noise is set to 5% of sale price, the average PRD is 1.002. When noise

is set to 10%, the average PRD is still only 1.01. Recall that the median value of the PRD in the real data is 1.13. According to the simulations, a PRD of 1.13 is not seen, on average, until noise reaches 30% of sale price. Based on these simulations, it appears safe to conclude that the high PRDs seen in the real data are indicative of genuine regressivity, not measurement error.

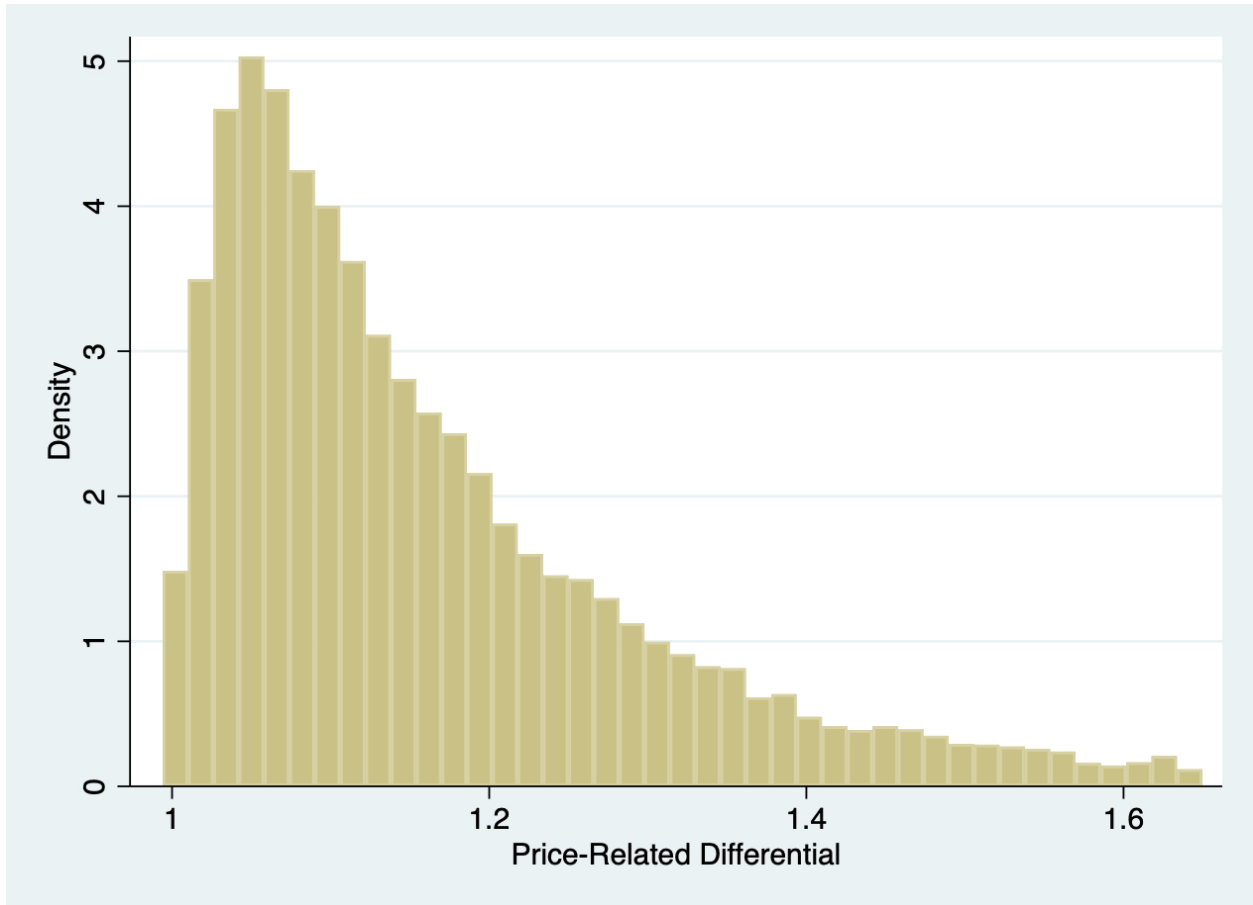


Figure A1: Histogram of County-Year Price-Related Differentials

Notes: The price-related differential (PRD) is the mean sales ratio divided by the price-weighted mean sales ratio. PRDs are calculated for each county in each year, and the figure shows the distribution of PRDs for all county-years with at least 100 sales. To improve interpretability, the figure excludes the top 5% and bottom 1% of PRD values.

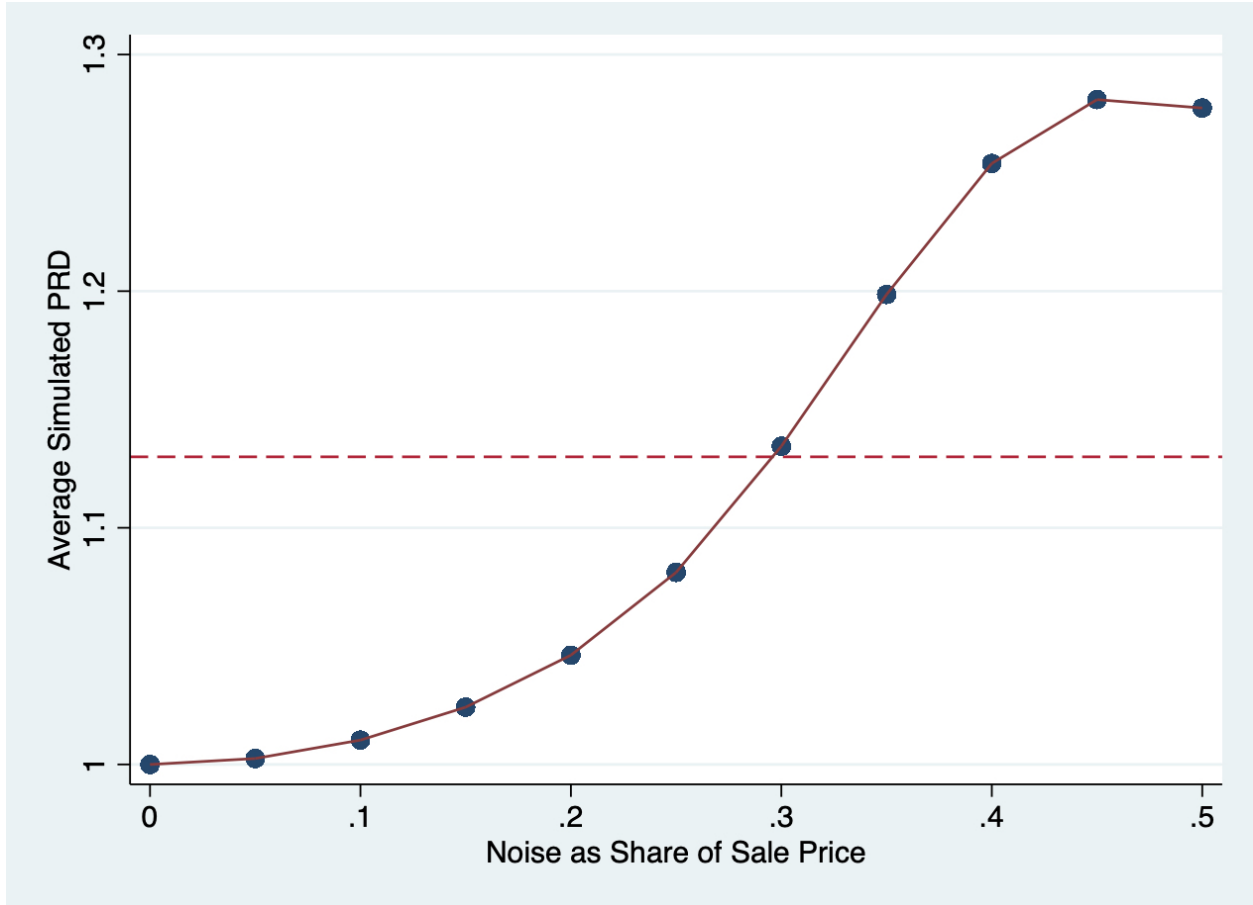


Figure A2. Monte Carlo Simulation of PRD Bias from Measurement Error

Notes: Figure shows average PRDs based on simulated prices set equal to assessed value plus noise, where noise is a normally distributed random variable with mean zero with a standard deviation equal to a varying share of the sale price. For each level of noise, ranging from 0 to 50% in increments of 5%, 20 simulations were run and the average PRD for every county is computed. The average value of the simulated PRD is reported. The horizontal dashed line represents the median county-year PRD in the real data.